Accident-Induced Absence from Work and Wage Growth*

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Abstract

How do short absences from work affect workers' labor trajectory? We use linked employer-employee administrative data from Hungary, with rich administrative health records, and use unexpected and mild accidents with no permanent labor productivity losses as exogenous drivers of short absences. Our Difference-in-Differences results show that, relative to the counterfactual of no accident, even short (3–6-months long) periods of absence due to accidents decrease wages for up to two years by 1.5 percent, and workers end up with lower-paying firms. Missed opportunities to move to higher-paying firms account for 7–37 percent of the wage loss over a two-year period.

Keywords: wage growth; accidents; health shocks; temporary absence from work
JEL codes: J22; J23; I10

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1 Introduction

Many workers may feel the importance of being physically and mentally present at work, suggesting that even temporary absence from work could have persistent detrimental effects on their labor outcomes. Absence from work is not uncommon, yet it is difficult to identify its consequences due to the endogenous nature of most absences (*e.g.*, quitting or being laid-off, parental leave, caregiving, *etc.*). This paper provides the first causal estimates of the extent to which short periods of involuntary absence from work, induced by unexpected and mild accidents with no persistent health effects, affect individuals' wage trajectory and reallocation to employers that may be of lower productivity.

When individuals are present at work, they have the chance to receive new, external, wage offers from competing employers, and to accumulate human capital. New external wage offers allow them to move to higher-paying employers or to bargain a higher wage with their current employer (Cahuc et al., 2006; Caldwell and Harmon, 2019; Flinn and Mullins, 2021; Lachowska et al., 2022),[1] while higher levels of human capital could lead to a higher wage from their current employer. In the absence of shocks that lead to absence from work, individuals continue to receive wage offers and they might accumulate human capital (Bagger et al., 2014), both leading to wage growth; at each new position with a higher wage, workers are then more selective on the set of wage offers they accept.[2]

In this paper, we quantify the importance of an overlooked mechanism related to wage growth: *forgone opportunities of wage growth* during temporary absence from work. We show that involuntary periods of absence from work, even if short with no persistent labor productivity losses, lead to persistently lower wages for absent workers. We estimate the wage loss stemming from absent workers missing out on opportunities to move to higher-productivity firms that are able to pay higher wages, and the wage loss stemming from forgone opportunities of achieving wage growth at the same firm.

We use unexpected and mild accidents—such as a broken leg, open wound, or dislocation of joints at the shoulder—as exogenous drivers of short (3–6-months long) absence from work, which we define as not being attached to a firm, or not receiving wage income, or being on sickness benefit. We choose accidents with the possibility of full recovery and no permanent labor productivity losses, to exclude the contaminating effect of worse

[1] These mechanisms are only present when wage setting is at the firm-level as opposed to competitive labor markets where identical workers in different firms are paid the same wage. Carvalho et al. (2023) use auctions with an ending time randomly generated by a computer to show that firm-level wages respond to firm-level demand, rejecting competitive labor markets, and to test predictions from wage bargaining and wage posting.

[2] *See* the seminal work of Burdett and Mortensen (1998) for the first theoretical work considering this type of labor market. Cahuc et al. (2006) propose an extension to the traditional job ladder framework where individuals get wage growth by switching employers and by their current employers increasing their wage to keep them from moving.

health on individual labor productivity. Our focus is not on the labor market implications of a particular health shock itself—in our case, accidents—rather on the labor market consequences of being temporarily absent, induced by our chosen set of accidents.

We use unique Hungarian administrative matched employer-employee data for 2009–2017, linked to rich individual-level administrative health history on medical diagnosis codes, drug prescriptions, hospitalizations, and sickness benefits receipt. The data contains detailed information on labor market outcomes of a random 50 percent sample of the entire population. The detailed health records included in the data also make it possible to precisely observe specific types of accidents treated in the public health system.

Our primary empirical aim is to test whether a short (3–6-months long) period of absence, due to an unexpected and mild accident, leads to lower wages and a reallocation across firms for treated individuals, relative to the controls (with no accident). In our Difference-in-Differences (DiD) empirical strategy, we explicitly control for any level differences between the treated and the controls *prior* to the accident. We also test whether absence leads to lower wages for the treated, relative to their own pre-absence wages.

Our main results are the following: (i) even short (only 3–6-months long) periods of absence, due to an unexpected and mild accident, decrease workers' wages relative to what they would have been in the absence of the accident; the estimated wage loss is 2.9 percent upon return to work and around 1.5 percent even 1–2 years after; and (ii) relative to the case of no accidents, workers who were absent due to an accident end up with lower-paying employers (captured by the estimated firm-specific wage premium, following Abowd et al., 1999). We also find that absent workers' wage upon return to work is not significantly different, on average, from their *past self*'s pre-accident wage.

We present four pieces of evidence which suggest that our chosen accidents do not have a long-term, direct, effect on workers' productivity, after and beyond any channel *via* absence. First, while there are signs of worse health around the time of the accident—such as higher chances of being hospitalized, higher uptake of sickness benefits, increased monthly drug spending overall and on antiinfectives and musculoskeletal drugs specifically—these effects are short-lived, and taper off within at most six months, regardless if there is an absence following the accident. Second, there is no sign that individuals would work in less stressful and physically less demanding occupations in response to accidents, after return to work in the longer-run. Third, there is no permanent decrease in their hours worked. Fourth, the persistent effects on wages and firm effects 1–2 years after returning to work are driven by young and white-collar workers, whose productivity is less likely to be affected by mild accidents than that of the blue-collars'.

Our main results are robust to (a) instrumenting absence with having suffered an accident, in an Instrumental Variable (IV) identification strategy, (b) exploiting the random nature of the time of the accident in an alternative, but related, DiD identification strategy, and (c) within-firm matching of individuals suffering and not suffering an accident. In (a), we measure the explicit effect of *accident-induced absence*, for the *compliers*, who are absent only because they suffered an accident, but who would have stayed present in the absence of the accident. In (b), we compare the labor market outcomes of almost identical individuals who suffered the accident at least three years apart in time; then, individuals who have not yet suffered the accident, serve as controls for those who have already suffered it. In (c), we compare the outcomes of individuals who suffered an accident to individuals who were employed at the same firm at the time of the accident but did not suffer an accident, and were also comparable along several other characteristics.

To shed light on the mechanisms, we decompose the wage loss for the treated into a part that stems from missed opportunities to switch to higher-paying firms, and into a part that is due to relatively lower wages at the same firm. We find that upon return 7 percent of the wage drop stems from missed opportunities to switch to higher-paying firms, and this share increases to 37 percent 2 years after. Similarly, we ask how much of the wage loss for the treated comes from missed opportunities to switch to better-paying occupations, and find that absence does not lead individuals to end up with lower-paying occupations relative to the counterfactual of no accident; thus, this share is negligible.

To explain our findings, we propose a new model of wage growth with accidents and temporary absence from work. In our model, individuals accumulate human capital and receive competing wage offers from firms while working, and potentially suffer accidents which may result in them being absent from work. In such an environment, wages grow *via* human capital accumulation and external offers which allow workers to either switch to higher-paying firms or to renegotiate a higher wage with their current employer. Upon suffering an accident, and while being temporarily absent from work, workers no longer receive external wage offers, and do not accumulate human capital (their human capital might even decrease). Then, the impact of temporary absence on individuals' labor trajectory depends on how much their wage would have grown had they not been absent. Our model proposes four channels *via* which absence leads to lower wages relative to the counterfactual of no absence: (1) missed opportunities to move to a higher-paying firms, (2) missed opportunities to renegotiate a higher wage with the current employer, (3) missed opportunities to accumulate human capital, and (4) human capital depreciation.

Our theoretical framework buttresses the importance of distinguishing between individuals' *past self* and their *counterfactual self*. Consistent with the notion that it is hard for firms to fire individuals or lower their wages after an accident, we consider individuals who are entitled to keep their jobs following an absence. We are agnostic about whether, upon return, individuals should have a lower, equal or higher wage than the wage of their *past self.* But, based on our model, individuals should have a lower wage relative to what they would have had in the case of no accident—*i.e.*, lower wage than the wage of their *counterfactual self*—due to the *forgone opportunities of wage growth* mechanism.

As a falsification test, we confirm that, consistently with our model's implications, workers who suffer an accident but have no subsequent absence spell thereafter experience wage losses only for the duration of recovery, and there is no impact on which (higher- or lower-paying) firms these individuals work for beyond the first months after the accident.

Our paper contributes to several strands of literature. First, job (or wage) ladders have been shown to be an important factor for the misallocation of labor in the Great Recession (Moscarini and Postel-Vinay, 2016, 2018; Bilal et al., 2022), the misallocation of labor when technologies are harder to imitate by new firms (Bilal et al., 2021), wage dispersion (Postel-Vinay and Robin, 2002), and the extent to which firms exploit search frictions (Bontemps et al., 2000). Several studies have attempted to quantify the importance of ladders, *via* structural estimation (Bowlus et al., 1995; Bontemps et al., 2000), by matching the drop in earnings following job loss (Jarosch, 2023), or documenting which type of workers flow across firms (Haltiwanger et al., 2018). However, none of them have quantified the importance of *forgone opportunities of wage growth*, arising from temporary absence from work without job loss. To the best of our knowledge, we are the first ones to use well-identified individual-level shocks to analyze this mechanism.

Second, our paper also relates to the large literature on job displacement. A first generation of papers show how displacement leads to persistent drops in displaced individuals' wage and employment (Ruhm, 1991; Jacobson et al., 1993), to what extent the permanent wage drop depends on skill differences across jobs before and after displacement (Poletaev and Robinson, 2008), and to what extent the displacement of the main earner affects the spouse's decision to work (Halla et al., 2020). While these papers empirically document the effect of displacement on wages and employment, they do not quantify the mechanism of *forgone opportunities of wage growth*, which we do, using periods of absence from work following unexpected and mild accidents.

A second generation of papers on job displacement decompose the persistent wage loss after displacement stemming from (i) workers moving to worse employers, and (ii) worse employer-worker matches after displacement (Lachowska et al., 2020; Helm et al., 2023). While they investigate to what extent the persistent wage losses arise due to *falling from the wage ladder*, we focus on and quantify the mechanism of *forgone opportunities of wage growth*. More generally, by investigating episodes of job loss, the job displacement literature studies episodes in which the wage of the treated individual decreases relative to what they had before (*past self*) and to what they would have had otherwise (*counterfactual self*). In contrast, by focusing on absences due to accidents without long-term health consequences, we study episodes in which the wage of treated individuals does not decrease relative to their *past self*'s but only relative to their *counterfactual self*'s. Furthermore, the job displacement literature often uses mass layoffs or plant closures as drivers of job loss, which likely result in General Equilibrium (GE) effects in the (local) labor market and are often anticipated; instead, we focus on unexpected individual-level accidents that likely do not affect any of the employers and have no GE effects.

We contribute to the literature on job displacement, also by decomposing how much of the wage loss of the treated stems from missed opportunities to move to higher-paying firms *versus* missed opportunities of wage growth within the same firm in other contexts and subsamples. In this stream of literature, Lachowska et al. (2020) decompose how much of the wage loss upon displacement is due to firm wage premiums and worse match quality, and find that 17 percent of the wage loss is due to lower firm wage premiums (*i.e.*, worse employers). Helm et al. (2023) focus on the manufacturing sector, where establishment premiums are higher, and document substantial heterogeneity between low- and high-wage workers in how much of the wage loss upon displacement is attributed to lower-paying establishments.[3] Although in the context of a different source of wage loss and decomposition exercise, our estimates and the estimates of Lachowska et al. (2020) and Helm et al. (2023) all underline the key role of within-employer wage variation.

Third, our paper also relates to the growing literature studying the effects of career leave. By allowing for human capital accumulation and depreciation, our theoretical framework highlights how human capital plays a role in the lower wages upon returning to work from an absence, relative to the counterfactual of no absence (Mincer and Polachek, 1974; Adda et al., 2017). By also studying reallocation across firms relative to the counterfactual, we distinguish ourselves from recent research on the effect of co-workers' paid leave on workers' within-firm ranking (relative to their co-workers) and subsequent earnings trajectory, which also buttresses the importance of being present or visible for career progression.[4] But, while Johnsen et al. (2023) exploit exogenous paternal leave policy variation in a worker's ranking within a contest, not *via* his own leave status but that of his competitors' leave status, we focus on the effect of workers' own absence induced by mild accidents, on their own wage trajectories and reallocation across firms.

Furthermore, our study is also related to the debate on the impacts of maternity leave on women's career and consequently on the gender wage gap. While many studies suggest that longer maternity leave has negative effects on women's wage growth (Datta Gupta

[3] Di Addario et al. (2023) focus on new hires to estimate an augmented multiple-way fixed effect wage regression following Abowd et al. (1999), motivated by models of wage renegotiation. They find that 23 percent of the wage variation is accounted for by the current employer premium and 0.7 percent is explained by the previous employer.

[4] Consistent with the importance of being present or visible, Cullen and Perez-Truglia (2023) find that more face-to-face interactions with managers have a strong positive effect on promotion.

et al., 2008; Ejrnæs and Kunze, 2013; Cukrowska-Torzewska and Lovasz, 2020), others do not find such effects (Lalive et al., 2014; Schönberg and Ludsteck, 2014). Empirical evidence from the United States, where mothers are typically eligible for only a short (12-weeks long) maternity leave, suggests persistent negative wage effects of motherhood. For instance, Waldfogel (1998) finds an almost 10 percent wage penalty for the first child; Loughran and Zissimopoulos (2009) estimate that a first birth lowers female wages by 2–3 percent without any effect on wage growth; and Budig and England (2001) estimate a wage penalty of 7 percent per child. We distinguish our analysis by identifying the impact of unexpected short absences driven by mild accidents without long-term health consequences and labor productivity losses, which is different from the impact of parental leave that is typically planned and is related to a major event – the birth of a child.

Fourth, our work relates to the literature on the impact of health shocks on individuals' labor market outcomes (Gallipoli and Turner, 2011; Crichton et al., 2011; Heinesen and Kolodziejczyk, 2013; Halla and Zweimüller, 2013; García-Gómez et al., 2013; Turner and Gallipoli, 2013; Dobkin et al., 2018; Parro and Pohl, 2021; Fadlon and Nielsen, 2021, among many others). This literature generally estimates negative effects of health shocks on employment and earnings, although the estimates vary, partly due to the different types of health shocks analyzed. We contribute to this literature by using specific temporary unanticipated health shocks (mild accidents) with no long-term health consequences and labor productivity losses, instead of more drastic health shocks (such as the arrival of chronic conditions, a heart attack, a stroke, or fracture of a skull).

In what follows, Section 2 describes the relevant institutional features of the Hungarian labor market, focusing on sickness benefits. Section 3 describes the data and measurement. Section 4 provides motivating descriptive evidence, foreshadowing that mild accidents inducing short absence from work have permanent wage effects. Section 5 describes our empirical strategies, and Section 6 presents our estimates. To shed light on the underlying mechanisms, Section 7 presents the decomposition exercise to see to what extent relatively lower wages for treated relative to control individuals arise from missed opportunities to switch to better, higher-paying employers, and to better-paying occupations. Section 8 presents our model to explain our estimates, and Section 9 concludes.

2 Institutional Background

Act LXXXIII of 1997 of the Hungarian Labor Code defines the "inability to perform at work" due to deteriorated health of workers. The 102/1995. (VIII. 25.) government declaration contains the regulation with respect to employees' inability to perform at work, and the regulation with respect to how inability needs to be assessed and established, generally by the employer's physician. Once the employee's inability to work is established, she is entitled to 15 days of "sick leave" for which the employer pays 70 percent of her absence pay (that is taxable, and is fully paid by the employer). Thus, each year, 15 days of sick leave can be taken by private and public sector workers, but not by the self-employed. The number of sick leave days is proportionally less for those who started at their current employer later than the 1^{st} of January of that year.

After 15 days, workers who are still not able to perform at work, are entitled to receive sickness benefit, conditional on them having social insurance and paying social security contributions. Sickness benefit is paid by the state, for a maximum of 1 year, during which workers can not be laid off (except for termination of contract without notice for serious reasons). The length and generosity depend on the length of the continuous social insurance spell and on previous sickness benefit payments. The amount corresponds to 50-60 percent of workers' wage income, on average. In the case of workplace accidents, the amount is 100 percent, with an immediate entitlement to sickness benefit.[5]

Job (and wage) protection is regulated and defined in the 33/1998 (VI.24.) government declaration, which determines that an exceptional occupational aptitude test shall be carried out, if there has been a change in the workers' health status due to a health shock, which likely renders them unable to perform the job (at all or without further health risks (7(1)). If a worker is determined to be still apt, she is eligible for their pre-shock wage, but if not, she is deemed (partially) inapt and may receive a lower wage. Thus, workers' wage upon return may be directly indicative of the severity of the health shock, and their pre-shock wages are not guaranteed for them under all circumstances.

Regarding workers' rights, regulatory employment protection is relatively low in Hungary, compared to other OECD countries (OECD, 2020), and it is relatively easy to dismiss regular workers. Unionization rate was around 10 percent in our analyzed period, and is one of the lowest in the OECD (OECD, 2023). Wage bargaining typically takes place on the individual level. However, there is a binding nationwide minimum wage, and since 2006 there is also a higher minimum wage for skilled jobs.

Finally, a person with a permanent health deterioration resulting in limited or no ability to work is entitled to a disability benefit, conditional on strict medical checkups. In this paper, we focus on mild accidents leading to short periods of absence from work with no permanent labor productivity effects, that do not lead to disability benefit take-up.

[5] As a comparison, employees in Austria are entitled to 100 percent of their wage if they are on sick leave up to 6–12 weeks — conditional on the length of continuous employment — and 50 percent for additional four weeks (Entgeltfortzahlungsgesetz 2.). In Slovakia, for the first three days 25 percent, then 55 percent of the reference wage is paid by the employer, then the employee is entitled to a sickness benefit up or a year (Social Insurance Agency webpage). There are no federal regulations for sick leave payment in the US; companies which fall under the Family and Medical Leave Act provide an unpaid sick leave for at most 12 weeks, conditional on certain requirements (US Department of Labor).

3 Data, Sample Selection, and Measurement

3.1 Data Sources

Our main data is an employer-employee linked panel dataset, covering 50 percent of the Hungarian population with a social security number in 2003, which, besides information on their labor outcomes for 2003-2017, also contains administrative data on workers' medical records and drug expenditures in the public health system, for 2009–2017.[6]

The employment-related data, containing the identity of the employer (firm), the type of employment, wage, occupation, and working hours, is provided by the *Hungarian Central Administration of National Pension Insurance*. It contains all sources of income liable for paying social security contribution and all employment relation that is counted in the length of service as a base for pension. Information on transfers stems from the *Hungarian Central Administration of National Pension Insurance* and the *Hungarian National Health Insurance Fund Administration* for maternal benefit, disability benefit and pension, and from the *Hungarian Ministry of Finance* for unemployment benefit.

Detailed data on health—hospital stays and outpatient care episodes, with monthly number of healthcare days and fine disease codes, number of primary care visits, and spending on prescribed medication by ATC codes—is available from 2009, originating from the *Hungarian National Health Insurance Fund Administration*.[7]

For each individual, we observe their gender and age (education only for a subsample), monthly amount of unemployment/child-related/pension benefits, monthly number of days on sickness benefit, monthly number of days in hospital by the cause of hospitalization (*e.g.*, accidents, heart attacks, cancer, *etc.*), monthly number of prescriptions, and monthly amount spent by drug code (*e.g.*, antibiotics, antidepressants, respiratory tract drugs, *etc.*). To the extent that the individual works in a given month, we also observe wage income and hours worked, occupation, industry, and firm identifier, in that month.

On the firms' side, our data includes financial data, and information on employment, industry, and foreign ownership, stemming from the *Hungarian National Tax and Customs Administration*, for all double-entry bookkeeping firms in Hungary, reported by the firms annually (in balance sheets, and profit and loss statements).

Three characteristics of the data are key to our analysis. First, we observe individuals even in months in which they are not attached to any employer—this is in large contrast

[6] The administrative database is a property of the National Health Insurance Fund Administration, the Central Administration of National Pension Insurance, the National Tax and Customs Administration, the National Employment Service, and the Educational Authority of Hungary. The data was processed and provided by the Databank of the Centre for Economic and Regional Studies in Hungary.

[7] Disease codes are based on the ICD "International Statistical Classification of Diseases and Related Health Problems" classification, and ATC stands for "Anatomical Therapeutic Chemical."

to most employer-employee datasets in which workers can only be observed if they are employed, and observing spells of non-employment is crucial for our analysis. Second, our data contains rich information on prescriptions and drug expenditures, which allow us to verify that our chosen accidents do not give rise to permanent changes in drug consumption (but, it does not contain any information on private healthcare use or the consumption of drugs without a prescription). Third, we observe the number of days on which the worker received sickness benefit in a given month (but not the number of days on sick leave). We do not observe if a given accident happened at the workplace or not.

Finally, we link the publicly available Occupation Information Network Database (O*NET) to our data, which describes the task content of occupations (*see* O*NET Resource Center).

3.2 Sample Selection Along With the Definition of Treatment

We follow the sample selection steps below, consistently with how we define *treatment*: as being temporarily—for 3–6 months—absent from work after having suffered an accident, with the counterfactual state of not having suffered an accident.

First, we select the *treated* sample of individuals who suffer the mild accidents listed below, are *employed* in the month of the accident or in the preceding month, and then are *absent from work* for at least 3 and at most 6 months following the accident (*i.e.*, we require return to work within half a year, thereby excluding the long-term unemployed).[8] We define *employed* as being attached to a firm with an employment contract,[9] having valid data on wage and working hours, and spending at most 5 days on sickness benefit in a given month.[10] We define being *absent from work* as not being employed.[11]

We choose the following unexpected and mild accidents with temporary health consequences, using ICD-10 "S" codes: superficial injuries, open wounds, fractures, dislocations, sprains of joints and ligaments, injuries of nerves, injuries of muscle and tendon of

[8] Although interesting in itself, we leave the study of such group for future research.

[9] The main types of employment in our data are: civil servant, public servant, *working with employment contract*, working for an armed force, part of a cooperative, self-employed, working in a partnership, contractual employment, agricultural worker, temporary worker, public worker. In 67 percent of all the observations with information on employment the person works with an employment contract. We only consider those with an employment contract as potentially being employed, and exclude self-employed.

[10] The data administrators do not define employment according to the ILO ("International Labour Organization") definition (having worked for pay or profit for at least one hour during a given reference week or having a job from which being absent for holidays, sick leave, maternity leave, *etc.*), therefore we construct our own employment definition keeping it as close as possible to the ILO definition.

[11] For 94 percent of the treated individuals, when being absent after an accident, it is the case that the person is still attached to a firm in our data, but spends more than 5 days on sickness benefit (99 percent of the observations with a firm identifier), has no hours worked attached, earns no wage or is not employed with an employment contract. In the remaining 6 percent, the person is not attached to a firm anymore, but only 27 percent of these receive any unemployment benefit.

selected body parts (ankle, foot, knee, lower leg, hip, thigh, wrist, hand, elbow, forearm, shoulder and upper arm), sprains of joints and ligaments of the head, the neck, the thorax and the upper lumbar spine. We exclude drastic fractures, such as that of the skull.

Based on the ICD-10 codes attached to inpatient episodes, we label individuals as having suffered an accident, if at least one of the above listed codes is recorded for them in a given month. To focus on clear cases, we only consider individuals with only one accident (corresponding to 83.8 percent of all people ever having an accident), which might be a 'clustered' event as defined below. Hospitalization events connected to accidents frequently occur in multiple consecutive months,[12] and because these occurrences are often the consequence of the same accident, we handle a sequence of at most four consecutive months with multiple accidents as a single 'clustered' event—in such cases, we take the last month before the first accident as the last month before the event, and the first month after the last accident as the first month after the event.

Almost half (46 percent) of the treated return in the 3^{rd} month after the accident, and 28.6, 15.7 and 9.7 percent return in the 4^{th}, 5^{th}, and 6^{th} month thereafter, respectively.

Second, we select the *control* sample of individuals who have never suffered any of the above-listed accidents, and are employed in the month of the pseudo-event, choosing the times of both the pseudo-event and pseudo-return (3–6 months thereafter) randomly.[13]

In sum, our *main sample* includes (i) those with an accident and a short-term (3–6-months long) period of absence thereafter, and (ii) those with no accident. Then, in our IV identification strategy (in Sections 5.1.2 and 6.3), we use (i), (ii), and also (iii) those with an accident but no absence thereafter—we call this sample *IV sample*.[14]

3.3 Measurement of the Outcome Variables

3.3.1 Hourly Deflated Log-Wage

We measure (the logarithm of) workers' deflated hourly wage, for a given month, as:

$$\ln(w) = \ln\left(\frac{\text{wage income}_m}{\frac{\text{days in month}}{7}\text{working hours}_w}/\text{defl}_y\right), \tag{1}$$

[12]Within individuals who have only one accident (which can also be a clustered event), 83.8 percent have no other accidents, 14.8 percent have an additional accident-related ICD-10 code for inpatient care within the next three months and 1.4 percent have further events within the next three months.

[13]Specifically, for the control individuals the time of the pseudo-return is a randomly chosen month from those within 3–6 months after the pseudo-event in which they are employed.

[14]In one of our robustness checks, we only use those in (i) who had their accidents at least 3 years apart, so that those who have not yet had their accident can serve as a control individual to those who have had the accident—we call this sample *accident sample exploiting random timing.* Finally, when we assess whether the data is consistent with our model's implications for those with an accident but no short-term period of absence thereafter, we use (ii) and (iii)—we call this sample *falsification sample.*

where wage income$_m$ contains all the monthly income which are used to calculate social security contributions. For a given month m, working hours$_w$ contain the weekly working hours, and defl$_y$ is the yearly deflator (yearly CPI with the base year being 2003). We winsorize working hours to be between 20 and 40 hours, and set the value of the hourly wage to missing if the individual is not employed (as per our definition), and/or receives disability benefit or sickness benefit in a given month.

3.3.2 Estimated AKM Firm Effect as a Measure of Firm-Specific Wage Premium, Reflecting Firm Productivity (Firm Quality)

Key to our analysis is to capture firm productivity (or firm quality), y, which we approximate with the extent to which a given firm is able to pay higher wages, *i.e.*, by estimating firm-specific wage premiums. In this subsection, we briefly outline how we estimate firm-specific wage premiums ϕ_j, using linked employer-employee data, and following the tradition of Abowd, Kramarz and Margolis (AKM, Abowd et al., 1999).

Consider the following wage equation for worker i at firm j at time t:

$$\ln(w_{ijt}) = X'_{ijt}\beta + \vartheta_i + \phi_j + \varepsilon_{ijt}, \tag{2}$$

where $\ln(w_{ijt})$ is the (logarithm of) i's wage at firm j at t, X_{ijt} is a vector of time-varying observable characteristics, ϑ_i is the time-invariant worker ability (or type), ϕ_j is the time-invariant firm-specific wage premium, and ε_{ijt} is the time-varying error term.[15]

With linked employer-employee data at hand for I individuals and J firms over $t = 1, ..., T$ time-periods, wages can be observed for the same individual for some periods at one firm and for some other periods at other firms; *i.e.*, worker switching and reallocation across firms is observable—this feature of the data is needed for identification of the time-invariant vectors of ϑ and ϕ in equation (2) (Card et al., 2013).

To get an estimate for time-invariant worker ability ϑ_i and firm-specific wage premium ϕ_j, we estimate the model in (2) using the entire sample of the linked employer-employee data, for years 2003–2017. Following Card et al. (2013), Card et al. (2016), and Card et al. (2018), we include the quadratic and cubic forms of age ($(age - 40)^2$, $(age - 40)^3$), as well as a full set of year dummies, in X (but, we do no include any firm-level variables).[16] After the estimation of the model in (2), we take the estimated

[15]If there are random match effects, those are also included in the error term. The stochastic error, ε_{ijt}, following Card et al. (2013), consists of two separate random effects: a unit root component which captures a drift in the portable component of the individual's earnings potential arising from, *e.g.*, unobserved human capital accumulation, health shocks, or the arrival of outside offers, and a transitory component which captures left-out mean-reverting factors.

[16]Card et al. (2013), Card et al. (2016), and Card et al. (2018) all also include the interaction of these variables with education dummies, but due to data limitations, we are unable to do that.

coefficient vectors $\hat{\vartheta}$ and $\hat{\phi}$, and call them "estimated AKM individual (fixed) effect" and "estimated AKM firm (fixed) effect", respectively.

3.3.3 An Alternative Firm Productivity (Firm Quality) Measure

In addition to the estimated AKM firm fixed effect, we also estimate the value added-based Total Factor Productivity (TFP) of firms, as an alternative measure of firm productivity (or firm quality). When doing so, we use the *prodest* STATA code of Rovigatti and Mollisi (2020) and apply the estimation procedure of Wooldridge (2009): specifically, we regress the logarithm of value added (defined as gross revenue minus the cost of goods sold) on year effects, the logarithm of firm size (variable input) and the logarithm of subscribed capital (state variable),[17] while using material and service costs as proxies for unobserved productivity. Our TFP estimate, for each firm, is the residual estimated from the aforementioned regression. Finally, we take the firm-specific average of the TFP indicator over the entire sample period, for years 2003–2017.

3.3.4 Measures of Occupation Characteristics (Stressful and Physically Demanding Nature of an Occupation)

To capture how physically demanding or how stressful an occupation is, we use the O*NET data. We build on Hardy et al. (2018) in assigning O*NET SOC-based occupation categories[18] to European ISCO (International Standard Classification of Occupations) classification and finally to Hungarian occupation codes (so-called "FEOR" codes). Based on the 2011 February O*NET edition, we create two variables, indicating how stressful an occupation tends to be and to what extent it is physically demanding.

We define the stressful nature of an occupation based on O*NET values of *Achievement and Effort, Stress Tolerance, Specialized Protective or Safety Equipment, Consequence of Effort, Level of Competition* and *Time Pressure.*[19] We use O*NET values on *Psychomotor Abilities, Physical Abilities, Sensory Abilities, Performing Physical and Manual Work Activities* and selected aspects on *Physical Work Conditions* to create the variable describing how physically demanding an occupation is. Both variables are standardized indices of O*NET task content values (with mean 0 and standard deviation of 1). Higher values indicate more stressful and more physically demanding occupations.

[17] Ideally, we would measure capital stock using tangible (and intangible) assets, but unfortunately these are not observable in the data.

[18] The Standard Occupational Classification (SOC) system classifies occupations used in the US.

[19] For instance, the *Achievement and Effort* dimension is defined as "job requires establishing and maintaining personally challenging achievement goals and exerting effort toward mastering tasks," or the *Stress Tolerance* dimension is defined as "requires accepting criticism and dealing calmly and effectively with high-stress situations." (O*Net Online).

4 Descriptive Analysis

4.1 Raw Patterns of Wages for Treated and Control Individuals

To see the raw data in our *main sample*, Figure 1 shows the wage trajectory of treated and control individuals, in a 2-year-long window around the event (only partialling out individual fixed effects). In Figure 1, we consider all (monthly) observations of treated individuals (*i.e.*, those being employed in a given month, suffering an accident and being absent for 3–6 months thereafter) and control individuals (*i.e.*, those being employed in a given month without an accident), we take the residual of deflated hourly log-wage on individual fixed effects and plot the group-specific averages of those residuals by quarters, with 95 percent Confidence Intervals (CIs), for $q = -8, ..., -1, 1, ..., 8$. $q = 0$ corresponds to the month of the event, and the average is normalized to 0 at $q = -4$. $q = 1$ is not shown for the treated, as they are absent from work in the first months after the accident.

The first observation from Figure 1 is that the average residual wage of the control individuals is gradually increasing over q, with no breaks or jumps around the event—which is as expected, given the time of the event was randomly chosen for the control individuals, and they do not suffer an accident. Second, the CIs around the point estimates for the treated and the controls reassuringly overlap in the quarters preceding the event (*i.e.*, no pre-trends). Third, the average residual wage of the treated at $q = 2$ is roughly the same as it was *prior* to the accident at $q = -1$, but falls below and never catches up with the average wage of the controls, not even by 2 years after the accident.

The rest of the paper investigates (i) this raw relationship between post-accident absence and wages, after controlling for a rich set of firm- and individual-level characteristics, for all and by subgroups; (ii) the robustness of our results to restricting the sample to those who suffer an accident but at different points in time, and to comparing pairs of individuals working at the same firm before the accident event; and (iii) the wage trajectories of individuals who had an accident but no subsequent absence spell, as a falsification test. Before analyzing (i)–(iii), we show the distribution of workers with our chosen accidents by severity and body part affected (in Section 4.2), and show evidence for our chosen accidents not leading to long-term productivity losses (in Section 4.3).

4.2 Distribution of Workers with Accidents

Table 1 shows the distribution of workers with our chosen accidents, by severity and body part affected, separately for those who were and were not absent from work after the accident. Table 1, in which the shares of individuals sum to 100 both for those with and without absence, reveals that fractures, sprains, and dislocations of joints are the most common accident types, and injuries of the knee/lower leg, the wrists/hands/fingers, elbows/forearms and shoulders/upper arms are more prevalent injured body parts; *e.g.*, among those who were absent from work after the accident, one-fifth suffered a fracture in the knee/lower leg, and around 10 percent in each group had their elbow/forearm or wrist/hand/fingers fractured, or knee/lower leg sprained or dislocated. Dislocation and sprain are more common, while fracture is less common among those who were not absent from work after the accident. The prevalence of injuries is the same in the two groups.[20]

There are no major differences in the frequency of accident types among individuals who were absent from work after the accident between blue- and white-collar workers, with the exception that injuries and fractures of the hand/fingers/wrist are over-represented among blue-collar workers, and injuries and dislocations of the knee/lower-leg are over-represented among white-collar workers (*see* Appendix Table C1).

4.3 Event Studies for Health Variables Around the Accidents

To confirm whether our chosen accidents have indeed no direct persistent labor productivity loss implications beyond the effect *via* absence, we provide supporting evidence that they have no permanent effects on workers' health, on sickness benefit uptake, hospitalization, and prescription drug spending (on all and on specific drug categories).

We estimate event study regressions, where the month of the accident is set to event time 0 ($k = 0$), the reference month is the month one year before the accident ($k = -12$), and the sample includes all individuals who suffer an accident (separately who are and who are not absent thereafter), in a 3-years long window around the time of the accident. The event study equation is the following (as in Schmidheiny and Siegloch, 2023):

$$\Omega_{it} = \sum_{k=-36}^{36} \delta_k d_{i,t-k} + \theta_t + \mu_i + \varepsilon_{it}, \tag{3}$$

where Ω_{it} is the outcome variable for individual i at monthly date t, $d_{i,t-k}$ is a set of indicators for i at time t being k periods after the accident (where negative k's refer to periods before the accident), θ_t denotes monthly date and μ_i denotes individual fixed effects. We graph the $\hat{\delta}_k$'s, with their 95 percent Confidence Intervals, for $k = -36, ..., 0, ..., 36$, to see if various indicators of sickness prevail before, around, and after $k = 0$.

Figure 2 reports the event study estimates for individuals having suffered an accident (*i.e.*, excluding controls with no accidents) by absence afterwards. While there is an

[20] We show in Appendix Figure B1 that in a logit model of absence after an accident, demographic indicators, the type of the accident, and further individual and employer characteristics all contribute to the prediction of absence. Altogether, the area under the ROC (Receiver Operating Characteristic) curve is 0.76, where 0.5 would mean no predictive capacity, whereas 1 would mean perfect prediction.

indication of worse health around the time of the accident, in terms of higher chances of being hospitalized, higher uptake of sickness benefits, and increased monthly prescription drug spending overall, as well as on antiinfectives and musculoskeletal drugs specifically, these effects are short-lived. For instance, the share of individuals spending at least 5 days per month in hospital tapers off to close to zero 4 months after the accident (*see* Panel (a)), while the average number of days spent on sickness benefits approaches zero 6 months after the accident (*see* Panel (b)), after which these averages are statistically indistinguishable from zero. Spending on antiinfectives and musculoskeletal drugs also tapers off after 2 and 5 months, respectively (*see* Panels (d)–(e)), while there is no spike in spending on psychoanaleptics neither around the accident, nor after (*see* Panel (f)).

Figure 2 also shows that there is no *prior* indication of sickness for either of the groups, in terms of hospitalization, uptake of sickness benefits, or drug spending, before the accident, supporting the unanticipated nature of our chosen accidents.[21] Furthermore, it shows that for individuals who are not absent after an accident (*i.e.*, who are neither part of our treated group nor our control group), the health effects are shorter-lived and taper off even more quickly – consistently with their typically less severe accidents.

4.4 Descriptive Statistics of Treated and Control Individuals

Individuals who suffered an accident are predominantly male and blue-collar workers (77 and 67 percent among them are males and blue-collar workers, respectively), work at lower-quality firms less likely to be foreign-owned, and have higher prior health expenditures than those who have not suffered an accident. Among those who suffered an accident and were absent from work thereafter, blue-collar workers and those in manufacturing are overrepresented, while those in trade are underrepresented (relative to those who stayed present after the accident). The estimated individual and firm AKM fixed effects are both smaller for the absentees, and so is their lagged wages, suggesting that these individuals are, on average, less successful on the labor market. The absentees are less (more) likely to work at foreign-owned (larger) firms, and have lower health expenditures in the year preceding the accident (*see* Appendix Table C2).

Overall, while there are clear differences between the various groups, our rich data enables us to control for a rich set of medical and labor history of individuals, to account for any selection along these dimensions. In what follows next, we describe our various Difference-in-Differences and matching empirical strategies in which we allow and control for any level differences between treated and control individuals *prior* to the accident.

[21]The slight increase in spending on musculoskeletal drugs in the no absence group 1-3 months prior the health shock (event time zero) indicates that occasionally the hospitalization due to an accident might occur only a short time after the accident.

5 Empirical Strategies

In this section, we describe our empirical strategies with which, to the extent that the relevant identification requirements hold, we measure the labor market effects of short absences due to unexpected and mild accidents. We use four empirical strategies.

In the first and main empirical strategy, described in Section 5.1.1, we compare the trajectories of labor market outcomes of individuals who suffer a mild accident and are absent for 3–6 months thereafter, with the trajectories of those who never suffer a mild accident, using Ordinary Least Squares (OLS), in a Difference-in-Differences framework.

In the second empirical strategy, described in Section 5.1.2, we use an Instrumental Variable (IV), and measure the effect of *accident-induced absence* specifically, on the *compliers*, using data also on those who suffer an accident but are not absent thereafter.

In the third empirical strategy, as a robustness check and addressing any selection concerns, we re-estimate the OLS model, exploiting the arguably random timing of mild accidents (described in Section 5.2). In this approach, we compare the trajectories of labor market outcomes of individuals who are sufficiently similar to each other, but suffered a mild accident at least 3 years apart in time; then, the individual who has not yet suffered the mild accident, only later in time, can be used as the "control" individual for the individual who suffered the mild accident at an earlier point in time. With this strategy, we can address the concern whether our main estimates are driven by less able—'clumsy' or 'fragile'—individuals who are prone to suffer an accident, and potentially would have worse trajectories of labor market outcomes even in the absence of an accident.

Finally, to confirm that our results are not driven by (unobserved) differences between the employers of treated and controls, we perform within-firm matching (Section 5.3).

5.1 Difference-in-Differences (DiD) Strategy Using Individuals With and Without an Accident

5.1.1 Linear Model using Ordinary Least Squares (OLS)

In our main Difference-in-Differences (DiD) empirical strategy, we compare key labor market outcomes—hourly deflated log-wages, estimated AKM firm effects, and the Total Factor Productivity of the worker's firm—upon return to work, and 1–2 years after, of

1. "treated" individuals who suffer an accident at time e while being employed, are absent for d periods thereafter, and return to work at $e+d$ within a half year, with the labor market outcomes of (in terms of labor and medical history) similar

2. "control" individuals with no accidents, who are employed at e and $e+d$ (where the times of the pseudo-event e and the pseudo-return $e+d$ are randomly chosen).

We make two important remarks on how we define our treated and control group. First, in our main sample on which we estimate the OLS model, we do not use data on those who suffer an accident but are not absent thereafter. Second, there is never an overlap between our treated and controls; *i.e.*, control individuals do not switch in or out of treatment; thus, our main empirical strategy only includes never-treated individuals in the control group. Furthermore, as we describe below, we control for absence duration and calendar year, and thus we always compare treated to our never-treated control individuals at the same time relative to accident, avoiding the problem of "*forbidden comparisons*" highlighted by Borusyak et al. (2021) and Goodman-Bacon (2021).

Intuitively, identification in our DiD approach using OLS requires that conditionally on our chosen control variables, in the absence of the treatment the labor trends of treated individuals would have followed the same trend as that of the control individuals. Importantly, as the treatment is staggered, the canonical parallel trends assumption needs to be extended to the staggered setting. Following Roth et al. (2023), the simplest extension is that had treatment not occurred, the outcomes for all adoption groups would have evolved in parallel. If this holds, then any differences between the labor trajectories of the treated and the controls can be attributed to absence following an accident.

We define $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ as a dummy variable that takes the value of 1 for individual i who suffers an accident at time e_i *and* has a temporary (3–6-months long) period of absence thereafter (for d_i periods); this variable takes the value of 0 for individuals who never have an accident. For each individual in each estimated regression model, we have two observations: one corresponding to the pre-event date, and the other to the post-event and post-return date. Let $\mathbb{1}\{t \geq e_i + d_i\}$ denote a dummy variable equal to 1 for individual i in all periods t after the return date $e_i + d_i$, and equal to 0 otherwise. Then, our estimation equation for i, employed at firm j at time t, is given by:

$$\begin{aligned} \Omega_{ijt} &= \alpha_0 + \alpha_1 \mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} + \alpha_2 \mathbb{1}\{t \geq e_i + d_i\} \\ &+ \alpha_3 \mathbb{1}\{t \geq e_i + d_i\} \cdot \mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} + \alpha_4 X_j^{pre} + \alpha_5 X_i^{pre} + \mu_t + \mu_d + \nu_{ijt}, \end{aligned} \tag{4}$$

where Ω_{it} is the outcome variable for individual i at firm j at monthly date t, X_j^{pre} represents characteristics of the employer j that the individual i had at the moment of the event e_i, (such as size, foreign ownership, average wage) and average lagged estimated firm AKM fixed effect (for lags 4,...,12 months *prior* to the event), X_i^{pre} represents characteristics of i at the moment or before of the event e_i (such as average lagged logarithm of wage for lags 4,...,12, logarithm of the sum of inpatient, outpatient and prescription drug spending 3–12 months preceding the event, binary indicators of any spending on prescription drugs 3–12 months preceding the event by 1^{st} level ATC categories, estimated individual AKM fixed effect, and a full set of indicators for gender, occupation, industry and age). μ_t are event date fixed effects (capturing monthly calendar time of the event), μ_d are fixed effects for absence duration, and ν_{ijt} is the unobserved error term.

The coefficients of interests are α_1, α_2 and α_3.

α_1 captures the pre-event differences, conditional on observable X_j^{pre} and X_i^{pre}, between the treated and the control individuals. α_2 represents the change in a given outcome Ω_{ijt} over time, from before the event to after, for the control individuals (*i.e.*, for whom $ACC_i = 0$). α_3 represents the difference in trends between the treated and control individuals; it tells how differently Ω_{ijt} would have evolved compared to the counterfactual scenario of no accident for the treated, *i.e.*, relative to their *counterfactual self*. To the extent that the DiD identification requirement holds, α_3 is the Average Treatment Effect (ATE) of a short-term absence following a mild accident on the trend of Ω_{ijt}.

Finally, $\alpha_2 + \alpha_3$ represents the change in a given Ω_{ijt} over time, from before the event to after, for the treated individuals (*i.e.*, for whom $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} = 1$), relative to their *past self*. In principle, we have no reason to expect a particular sign for $\alpha_2 + \alpha_3$. Absence from work might lead to human capital depreciation, leading to lower wages upon return to work from an absence, in which case $\alpha_2 + \alpha_3 < 0$. At the same time, in the presence of wage rigidity—*e.g.,* due to cultural norms or institutional restrictions—an employer might be unable to pay a worker a lower wage than what they had before the absence, despite lower human capital, in which case $\alpha_2 + \alpha_3 \geq 0$.

5.1.2 Linear Model using Instrumental Variables (IV)

In this subsection, we explain how we use suffering an accident as an IV for being absent. This approach has the advantage from an interpretation point of view: while with the linear model using OLS, we are able to measure the ATE of short-term absence following an accident, with the linear model using IV, we are able to measure the Local Average Treatment Effect (LATE) for the *compliers*: those who (i) become absent following an accident in e_i, and return to work at $e_i + d_i$, but who (ii) would have stayed present in the absence of an accident. The compliers are exactly the relevant population for whom we want to and are able to estimate the effect of *accident-induced absence*.

For ACC_i to be a valid IV for $Absence_i$, ACC_i should be as good as randomly assigned, and should be related to a given labor outcome only through being absent, and not through other direct channels—which fails if accidents affect individual labor productivity directly, above and beyond "just" pushing the individual into absence. Thus, this identification strategy requires that accidents lower labor productivity at most *via* human capital depreciation that happens exclusively and only during absence from work.

We provide numerous pieces of evidence suggesting that accidents do not have a direct effect on labor productivity beyond the effect operating through absence.

First and foremost, we look at whether our chosen accidents lead to permanent changes in health outcomes (hospitalizations, drug expenditures, *etc.*), which we saw in Section 4 that they do not. Second, we show in Section 6 that there is no indication that individuals—either blue-collar or white-collar workers—would work in physically less demanding or less stressful occupations, 1–2 years after returning to work following an accident. Third, we find no permanent decrease in hours worked, despite finding permanent decreases in wages. Fourth, for some of our outcomes, we look far enough in the future (2 years later) such that it is hard to imagine that a mild accident (*e.g.*, a broken leg) could have an impact for that long on worker productivity. Fifth, in Section 6 we also verify that our results are present for both white-collar and blue-collar workers (with the idea being that while it is possible that accidents lead to permanent changes in labor productivity for blue-collars, it is hard to argue that this is the case for white-collars). Our finding that our results persist when focusing on white-collar workers only is inconsistent with permanent drops in labor productivity fully explaining them. In fact, we find that the persistent wage effects are stronger for young and white-collar workers.

Finally, for the IV estimator to provide the LATE, we also need the monotonicity requirement to hold such that the IV affects the propensity of being absent in only one direction; *i.e.*, it cannot happen that someone is absent when not having had an unanticipated accident, but remains present when having had one. We consider the monotonicity requirement very likely to hold and an innocuous one.

In sum, in our estimation equation below we instrument $\mathbb{1}\{Absence_i = 1\}_i$ with $\mathbb{1}\{ACC_i = 1\}_i$, and the interpretation of β_1, β_2, β_3 are analogous to the interpretation of α_1, α_2, α_3, respectively, except that β_3 captures the LATE for the *compliers*:

$$\Omega_{ijt} = \beta_0 + \beta_1 \mathbb{1}\{Absence_i = 1\}_i + \beta_2 \mathbb{1}\{t \geq e_i + d_i\} + \beta_3 \mathbb{1}\{t \geq e_i + d_i\} \cdot \mathbb{1}\{Absence_i = 1\}_i + \beta_4 X_j^{pre} + \beta_5 X_i^{pre} + \tau_t + \tau_d + \vartheta_{ijt}. \quad (5)$$

5.2 Robustness Check: Exploiting the Random Timing of Accidents, Using Only Those with Accidents

Addressing any remaining selection concerns—*i.e.*, whether individuals select themselves into the state of suffering an accident, even conditional on our rich set of control variables of labor and medical histories—, we measure the effect of absence also by using only the sample of treated individuals, by comparing the labor trajectories of individuals who are similar to each other, but suffered the accident at least 3 years apart in time. Then, the individual who has not yet suffered the accident, only later in time, can be used as the "control" individual for the individual who suffered the accident earlier. The estimation equation for individual i, suffering an accident at time e_i is given by

$$\Omega_{ijt} = \alpha_0 + \alpha_1 \tilde{\mathbb{1}}\{ACC_i = 1 \,\&\, Absence_i = 1\}_t + \alpha_2 \mathbb{1}\{t \geq e_i + d_i\}$$
$$+ \alpha_3 \mathbb{1}\{t \geq e_i + d_i\} \cdot \tilde{\mathbb{1}}\{ACC_i = 1 \,\&\, Absence_i = 1\}_t + \alpha_4 X_j^{pre} + \alpha_5 X_i^{pre} + \mu_t + \mu_d + \nu_{ijt}, \tag{6}$$

where $\tilde{\mathbb{1}}\{ACC_i = 1 \,\&\, Absence_i = 1\}_t$ is a dummy variable that takes the value of 1 for all observations of i such that $t \geq e_i$ and 0 for all observations of i such that $t \leq e_i - 3$ years.

This approach is the one used by Fadlon and Nielsen (2021), and, similarly for assessing robustness, by Halla et al. (2020). This identification strategy requires the "no anticipation" assumption, meaning that if an individual is untreated in period t (has not yet suffered an accident), her outcome does not depend on what time period she will be treated (will suffer an accident) in the future (Roth et al., 2023).

Importantly, in this robustness check, we only use the 'not-yet-treated' observations of the control individuals before they themselves suffer an accident. Thus, concerns about dynamic treatment effects in a staggered design (*e.g.*, Goodman-Bacon, 2021 and discussed in De Chaisemartin and D'Haultfoeuille, 2022) do not apply in our case. Moreover, for our main strategy (using individuals who never had an accident as controls), since control individuals do not switch in or out of treatment, such concerns about dynamic treatment effects do not apply either. In neither of our empirical strategies do we use two-way fixed effects DiD estimators, with both time and individual fixed effects.

5.3 Robustness Check: Within-Firm Matching

To investigate if our results are driven by (unobserved) differences between the employers of treated and control individuals, we perform within-firm matching. In this exercise, we use the *main sample* used in the OLS regressions, but, we expand the control group such that each potential control individual is included in the data as many times as many "time of event" and time of "return" combinations are feasible for them (instead of picking random "time of event" and time of "return" values, as in our *main sample*).

We apply a kernel matching method, as described among others by Heckman et al. (1998) and Smith and Todd (2005).[22] We require exact matching on gender, firm iden-

[22]With kernel matching, all treated are matched with a weighted average of all controls with kernel weights that are inversely proportional to the distance between the propensity scores of treated and

tifier at the time of the accident, decile of wage at the time of the accident, decile of AKM individual fixed effect, (*i.e.*, coarsened exact matching on wage at the time of the accident, and on AKM individual fixed effect), date of the accident, and absence duration (in months). In addition, we include the following matching variables: average logarithm of wage for lags 4,...,12 months *prior* to the event, logarithm of the sum of inpatient, outpatient and prescription drug spending 3-12 months preceding the event, binary indicators of any spending on prescription drugs 3-12 months preceding the event by 1^{st} level ATC categories, and a full set of indicators for occupation, and age.[23]

The core identification assumption behind our matching procedure is a mean independence assumption: conditional on the propensity score and the variables on which we require exact matching, the expected value of the potential outcome without treatment is independent from the treatment itself (*see* assumption (A-4) in Heckman et al., 1998).[24] The matching method also assumes common support, *i.e.*, overlap between the treatment and control groups on the variables used in the matching. The large set of potential control units ensures in our case that the common support assumption holds, which is reflected also in Appendix Figure A1. In Section 6, we present the matching estimates of the Average Treatment Effect on the Treated (ATT).[25]

6 Estimation Results

6.1 Main OLS Estimates for Main Outcomes

Panel A of Table 2 shows the estimation result of our Difference-in-Differences OLS model, as specified in Section 5.1.1. Columns (1) and (2) show the estimates for the hourly wage ($ln(w_i)$) and estimated AKM firm effect ($\hat{\phi}_j$) outcomes upon return to work after the period of absence (*i.e.*, after d periods), while columns (3)–(4) and (5)–(6) show

controls (Becker and Ichino, 2002; Smith and Todd, 2005). We use the Stata command *kmatch* by Jann (2017) with the default bandwidth selection method, based on a pair-matching algorithm.

[23]In Appendix A, Table A1 provides balancing statistics. The first three columns of the table indicate that considering the means of the matching variables, the treated and control sample are comparable even before the matching. For the majority of matching variables, the balance improves after the matching. The biggest standardized difference is for log health spending 3-12 months before the event, but even there the standardized difference is only 0.111. In the matched sample the gender, employer at the time of the accident, decile of wage at the time of the accident, decile of the AKM individual fixed effect, date of the accident, and absence duration are identical for the treated and their matched controls.

[24]This is different from the identifying assumption in the DiD method (the common trend assumption).

[25]Intuitively, the ATT estimate is the mean difference in the outcome of each treated individual and a weighted average of the outcomes of the control individuals, with the weights depending on the difference in the propensity score of the treated and control individuals (smaller difference implying larger weight). *See*, *e.g.,* Becker and Ichino (2002); Imbens (2004) for details. As we have a much larger number of control observations than treated, we focus on the ATT, instead of the average treatment effect, in line with Abadie and Imbens (2006) and Ferman (2021).

the estimates 1 year after and 2 years after return to work, respectively. Standard errors are estimated by clustering on the calendar month, leading to 108 clusters.

Results in the first row of column (1) indicate that *prior* to the event of a mild and unexpected accident, wages of treated individuals are, on average, 0.94 (s.e. 0.18) percent higher than wages of the control group, which indicates a small positive selection *prior* to the event. The second row indicates that the controls' wages are 2.85 (s.e. 0.25) percent higher upon return after the (random) pseudo-event, in the absence of events such as accidents. The third row reports our estimate of the Average Treatment Effect (ATE) of being temporarily (for 3–6-months) absent following an accident: wages are significantly, on average, 2.87 (s.e. 0.31) percent lower upon return, relative to what they would have been in the absence of the event – relative to the wage of the worker's *counterfactual self*.

To test that upon return from absence, individuals have an equal or higher wage than the wage of their *past self*, we formally test $H_0 : \alpha_2 + \alpha_3 \geq 0$. The estimate for that sum is -0.0002 and the corresponding *p-value* is 0.95. Thus, post-absence wages are, on average, almost the same as pre-absence wages for the *treated* individuals, upon return to work following a mild accident, and they are not significantly different at any level.

Column (2) shows similar results for the estimated AKM firm effect, corresponding to firm productivity (firm quality). Results in the first row of column (2) indicate that *prior* to the event, AKM firm effects are statistically indistinguishable between treated and control individuals (with an estimate of 0.0001). The result in the second row of column (2) shows that the estimated AKM firm effect is higher by 0.0028 (s.e. 0.0002) for the control group after the (random) pseudo-event (corresponding to a 0.28 percent higher firm-specific wage premium), consistently with the framework in which individuals experience wage growth by sequentially moving to better, higher-paying, employers. Our estimate of the ATE for the AKM firm effect is -0.002 (s.e. 0.001). The baseline average of the AKM firm effect among the controls is 0.0285 (s.d. 0.344); thus, the -0.002 point estimate corresponds to a 0.58 percentage points decrease of a standard deviation.[26]

Results in columns (3) and (4) show that the effects persist even 1 year after returning to work, following the event. An absence following an accident significantly decreases wages by 1.11 (s.e. 0.33) percent, on average, 1 year after returning to work, and it decreases the estimated AKM firm effect by 0.003 (s.e. 0.002). Finally, results in columns

[26]The sample sizes of the wage and AKM firm effect regressions differ because the missing rate of the AKM firm effect is high among the smallest firms, and there are also observations with zero wage (thus missing log wage) but non-missing AKM firm effect. If we restrict the samples to those observations where neither the log wage, nor the AKM firm effect are missing, then the estimated treatment effects remain similar to the baseline results. Also, when we restrict the wage data for the treated upon return to months solely being at or after the first month of sickness benefit in the year of return, the estimated treatment effect on wages remains similar, as well; hence, our wage results are not driven by potentially lower observed wages during sickness absence. These results are available upon request.

(5) and (6) show that the effect on wages and on the estimated AKM firm effect persist even 2 years after returning to work, following an absent period; the effects are -1.5 (s.e. 0.40) percent for wages and -0.0055 (s.e. 0.002) for the estimated AKM firm effect.

We make two remarks on our main estimates. First, our main estimates are almost identical if, instead of a random d drawn using a uniform distribution for the control individuals, we choose d for them randomly but so that their distribution across d matches the distribution of treated individuals across d (*see* Appendix Table C4). Second, following an accident-induced absence, workers might voluntarily reallocate to firms with lower wage premium, relative to the case of no accidents, if these are also firms in which accidents are less likely – in this case, lower AKM firm effects would capture compensating differentials *via* lower accident frequency. Inconsistently with this, however, the correlation between accident occurrence and AKM firm effects is small and negative (-0.011).

6.2 Main OLS Estimates Over Time

In *Panel A* of Table 2 we reported the estimated effect of absence following an accident at three points in time – upon return to work, 1 year after and 2 years after. To gain more insight on the time pattern of the estimated effects and on possible pre-trends, in Figure 3 we display the estimated effect on hourly deflated log-wage and AKM firm effect by quarter, for 8 quarters before the accident and 8 quarters after return to work. For each quarter, we estimate (4), replacing the post-return indicator ($\mathbb{1}\{t \geq e_i + d_i\}$) with an indicator of relative time (quarters before the accident or after return to work).[27]

Despite the aforementioned slight differences in the estimated equation and the sample, the estimated effect of absence following an accident over time in Figure 3 is very much in line with our baseline estimates in *Panel A* of Table 2. The estimated effect of absence following an accident on wage 1–8 quarters after return to work is around -2 percent, whereas the estimated effect on the AKM firm effect increases in absolute value over the same time horizon, and ranges from -0.002 to -0.009. Importantly and reassuringly, we do not observe a pre-trend *prior* to the accident for either of the outcomes.

6.3 IV Estimates for Main Outcomes

Panel B of Table 2 shows the estimation result of our Difference-in-Difference IV specification. The IV strategy allows us to estimate the Local Average Treatment Effect

[27]Since we estimate the model also for time periods before the accident, we exclude from the control variables the lagged logarithm of wage and the lagged estimated firm AKM fixed effect, but add the lagged indicators of firm size, foreign ownership, and firm level average wage for 5–8 quarters *prior* to the event. When estimating the model for the wage outcome, we further restrict the sample to keep only wages which are not generated from job-spell level aggregate wage (the vast majority of wage measures in 2012–2017).

(LATE) for the compliers: those who become absent following an accident but who would have stayed present without an accident, exactly corresponding to the relevant subpopulation for whom we want to estimate the effect of *accident-induced absence*. To get a sense for the strength of the first-stage relationship between *Accident* and *Absence*, we note that while 60 percent of those with an accident have an absence-spell thereafter, the corresponding share of those with no accident is only 1 percent, implying a very strong first-stage relationship and no concerns of passing tests of weak instruments (Stock et al., 2002, and, in the presence of clustering, Olea and Pflueger, 2013).

Results in columns (1) and (2) show that an accident-induced absence decreases wages by 2.39 (s.e. 0.36) percent and the AKM firm effect by 0.003 (s.e. 0.001), relative to the outcomes of the individual's *counterfactual self*. Columns (3) and (4) show that the results persist in magnitude for both wages (−0.86 percent, s.e. 0.46) and AKM firm effect (−0.007, s.e. 0.003) 1 year after return to work. Finally, Columns (5) and (6) show similar patterns for outcomes 2 years after return to work.

6.4 OLS Estimates for Other Outcomes

Panel C of Table 2 shows that the patterns for firm quality presented in Section 6.1 continue to hold if we use the estimated TFP as another measure of firm quality.

The second row of *Panel C* of Table 2 confirms that employer TFP of control individuals grows over time, consistently with workers switching to better firms. In the third row of *Panel C*, we see that short-term absences following a mild accident lead to 0.011 (s.e. 0.005) lower employer TFP upon return to work, 0.02 (s.e. 0.009) lower employer TFP 1 year later, and 0.016 (s.e. 0.012) lower employer TFP 2 years later. The baseline average of the employer TFP at the control group is 9.8, with a standard deviation of 1.59, therefore the effects ranging between −0.02 and −0.011 correspond to −1.258 to −0.69 percentage points decrease in the standardized measure of employer TFP (that has mean zero and standard deviation of one).

Next, we check if our main estimates could be driven by occupation changes; *i.e.*, if individuals switch to less physically demanding and/or less stressful occupations after suffering an accident—if so, the observed wage loss could indicate compensating lower wages in the new occupations with better working conditions; or, it might also reflect long-term labor productivity losses, especially for blue-collar workers.

Upon return after an absence, individuals tend to have somewhat less physically demanding occupation but with no change in how stressful it is. Over time, this difference in how physical demanding the occupation is disappears. For instance, treated individuals with an absence-spell following an accident work in an occupation with a 0.58 (s.e. 0.3) percent of a standard deviation lower physical score, that said, this difference cannot be detected 1 year and 2 years after return (*Panel A* of Appendix Table C3). With regards to how stressful an occupation is, we find no effect on our stress score measure.

We get similar results for the subsample of white-collar workers (*Panel B* of Appendix Table C3), and for blue-collar workers (*Panel C* of C3). For blue-collar workers, we do not see any indication of switching to an occupation upon or after return to work, that is physically less demanding or less stressful. White-collar workers have an occupation 1 year after return that even has a, on average, 2.66 (s.e. 1.13) percent of a standard deviation higher score in the O*NET "physically demanding" occupation dimension, which increases to 6.13 (s.e. 1.74) percent of a standard deviation 2 years after return.

In sum, we do not find any indication for the estimated negative wage effects being driven by switching to less stressful or less physically demanding occupations after the absence due to an accident. Consequently, our results in this subsection do not suggest any long-term labor productivity losses, for either white- or blue-collar workers.

6.5 Heterogeneity Analysis

In this section, we present to what extent the coefficient estimate on the interaction term $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} \cdot \mathbb{1}\{t \geq e_i + d_i\}$, stemming from estimating the "Linear Model using Ordinary Least Squares (OLS)" in Section 5.1.1, differs by subgroups, based on tenure, age, occupation, gender, and pre-event AKM firm effect and pre-event wage. Appendix Figure B2 shows the estimated effects of absence following an accident.

Upon return to work, the wage effects are significant for all subgroups, and the Confidence Intervals (CIs) typically overlap, except for the tenure and age heterogeneities, where the negative wage effect is stronger for the youngest ($20 - 29$ years old) age group and the one with less than 1 year of tenure at the firm – these groups experience a large 5.3 and 5.7 percent wage loss upon return, relative to their *counterfactual self*'s, respectively (*see top-left panel*). The point estimates on the estimated AKM firm effect upon return (in absolute value) are larger for the youngest and white-collar workers (yet with insignificant estimates around $(-0.004) - (-0.005)$, *see top-right panel*).

The longer-term effects on wages are driven mainly by short-tenured individuals and by white-collar individuals, as well as by the youngest workers in case of wages 1 year after return. The longer-term effects on the AKM firm effect are stronger for the youngest and female workers 2 years after return (although the CIs overlap, the point estimates of one subgroup typically do not fall into the CIs of the other subgroup) (*see middle-left*, *bottom-left*, *middle-right* and *bottom-right panels*).

The point estimates by pre-event wage and pre-event AKM firm effect differ in absolute value for all outcomes, and indicate stronger effects for individuals with below median pre-event wage and with below median pre-event AKM firm effect; for instance, the effects on wages are significantly larger (in absolute value) for them, and the effects on the AKM firm effect are driven solely by them, both upon return to work and later.[28]

6.6 Robustness and Falsification Checks

6.6.1 Exploiting the Random Timing of Accidents

Next we consider a robustness analysis with individuals who suffer an accident 3 years or later in the future as control individuals, to address any remaining selection concerns (*i.e.*, whether individuals select themselves into suffering an accident, even conditional on our rich set of control variables). Our results in Table 3 indicate that the estimated negative effects on labor market outcomes of short accident-induced absence are similar to—for the AKM firm effects are even stronger than—the baseline ones, when focusing on just the group of workers who suffer an accident but at different points in time.

Prior to the event the wages of treated individuals were 0.74 (s.e. 0.29) percent higher than that of the control group, and wages for the controls are 2.45 (s.e. 0.24) percent higher after the pseudo-event. The estimate for the effect of being temporarily absent from work post-accident on wages is -2.74 (s.e. 0.43) percent upon return to work, relative to what they would have been in the absence of the accident (Column (1)).

Prior to the event of a mild accident, there is no statistically significant difference in AKM firm effect between treated and control. The estimated AKM firm effect is 0.0061 (s.e. 0.0003) larger for the control group after the event, consistent with the prediction that, in the absence of the accident, individuals experience wage growth by sequentially moving to higher-paying employers. Temporary absence following a mild accident leads to individuals being employed by firms with a 0.0064 (s.e. 0.0016) lower firm quality, relative to the counterfactual of no accident (yet). The baseline average of the AKM firm effect among the controls is 0.0127 (s.d. 0.328); thus, the -0.0064 point estimate corresponds to a 1.95 percentage points decrease of a standard deviation.

Wages are still lower by 0.9 (s.e. 0.40) percent 1 year later and by 0.73 (although no longer significant) percent 2 years later, relative to the counterfactual of no accident (Columns (3) and (5) of Table 3). Our estimates also indicate that firm quality is still 0.012 (s.e. 0.0026) lower 1 year later and 0.012 (s.e. 0.0032) lower 2 years later, relative

[28]We also consider the estimated interaction coefficient and its 95 percent Confidence Interval, by type of injury (fracture, dislocation/sprain, injury/wound) and body parts injured (ankle/foot, knee/lower leg, hip/thigh, wrist/hand, elbow/forearm, upper arm). We do not find any indication that injury by specific body parts would drive our main results, and the point estimates by type of injury are very similar (*see* Appendix Figure B3). Wage and firm effects are negative for both those that were absent for $3-4$ months and those that were absent for $5-6$ months, but, due to a large overlap in CIs we are unable to conclude whether effects are stronger or smaller by duration. We do find that all our main estimates are larger once we consider $3-12$-months-long absences; these results are available upon request.

to the counterfactual of no accident (yet) (Columns (4) and (6) of Table 3).

In sum, our baseline results do not seem to be driven by selection; *i.e.*, they are not driven by less able individuals who are prone to suffer an accident, and potentially would have worse labor trajectories even in the absence of an accident.

6.6.2 Within-Firm Matching

Next we consider results, where, instead of OLS, we apply a kernel matching method, with exact matching on firm identifier at the time of the accident, gender, decile of wage at the time of the accident, decile of AKM individual fixed effect, date of the accident, and absence duration (*i.e.*, control individuals are required to be employed at the time treated individuals return to work at the end of their absence spell).

Table 4 shows that the Average Treatment Effect on the Treated (ATT) estimate on wage is -3.83 (s.e. 0.40) percent upon return, which decreases to -1.97 (s.e. 0.34) percent 1 year later, and to -1.61 (s.e. 0.50) percent 2 years later. The ATT estimate on the AKM firm effect is -0.003 (s.e. 0.001) upon return, -0.006 (s.e. 0.002) 1 year later, and -0.008 (s.e. 0.003) 2 years later.

Overall, our estimated treatment effects on AKM firm effect are stronger under the matching estimation than under the baseline OLS estimation, while the wage effects are similar (though stronger upon return). Thus, our baseline estimates are not driven by (potentially unobserved) differences between the employers of treated and controls.

6.6.3 Exclusion of Co-Workers of Treated Individuals

The absence of a co-worker might directly influence remaining workers especially in smaller firms (*e.g.*, forcing other workers to pick up the slack, which could also give them the opportunity to shine and, so, get promoted, *etc.*), which would challenge the DiD assumption that the controls are not affected by the treatment. To investigate this issue, for every accident event, we drop all controls that, (i) were working in the same firm as the treated individual and (ii) the firm had 50 control-group employees or less in our sample (between the period before the accident and 2 years after return). We find that our estimates are essentially unchanged from the baseline (*see* Appendix Table C5).

6.6.4 Using Alternative Measures of Hourly Wage and Wage Income

We consider further robustness checks, for the “Linear Model using OLS” (Section 5.1.1), using alternative definitions of the hourly wage and wage income, and showing estimates for weekly hours worked. In Appendix Table C6, each of the three consecutive columns refer to the same variable, in which the 1^{st} presents estimates upon return, the 2^{nd} presents estimates 1 year after return, and the 3^{rd} presents estimates 2 years later.

Compared to the baseline hourly wage results (*Panel A* on the left of Appendix Table C6), we adjust hourly wage with actual monthly days insured (*Panel B* on the right), and we also use monthly wage (*Panel B* on the left). As additional wage measures, we adjust monthly wage with actual days insured (*Panel C* on the left) and actual days with an income that month (*Panel C*). The main patterns stay the same, and none of these modifications affect our results: the wage effect is between -3.95 and -2.00 percent upon return and between -1.58 and -0.88 percent 1–2 years after return, no matter if actual days insured (and the corresponding wage income) are taken into account or not.

Estimates for the weekly hours worked (*Panel A* on the right of Appendix Table C6) show a negative effect of absence after the accident, which remains even 2 years after return to work. Still, the magnitude is tiny (about 0.13 hour per week, corresponding to less than 8 minutes, on average); thus, decreasing labor supply in the form of hours or switch to part-time status is not a major mechanism behind our observed wage patterns.

In sum, our baseline results presented in Section 6.1 are robust to sample restrictions (focusing on just the group of individuals who suffer an accident but at different points in time), within-firm matching, exclusion of co-workers of treated individuals in small firms, alternative measurement of hourly wages and wage income.

6.6.5 Falsification Test Using Individuals With Accident but No Absence

Finally, we verify that for individuals with an accident but no subsequent absence spell, an accident affects wages negatively only for the duration of recovery and does not affect employer productivity, apart from a weak negative effect in the first 4 months.

We look at the trajectories of wage and estimated AKM firm effect for individuals with an accident but no subsequent absence spell, using those with no accidents as a control group, for 1–12 months after the (accident) event. We find that individuals for whom $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\} = 1$, are slightly positively selected, in terms of wage, relative to those for whom $ACC_i = 0$, but only in the first 4 months after their accident do they have significantly lower hourly wage than what they would have had in the absence of the accident (*Panels A* and *B* of Appendix Table C7). Furthermore, apart from a weak negative effect in the first 4 months after the accident (an estimated effect of -0.0012 to -0.0016, corresponding to a 0.42 percentage point decrease in the standardized measure of AKM firm effect), there are no significant effects, for any of the time periods in the year after the accident, for firm productivity, as measured by the estimated AKM firm effect (*Panels C* and *D* of Appendix Table C7). Thus, these individuals *do not incur any considerable foregone opportunities of wage growth.* At the same time, control individuals continue to *achieve wage growth*, as indicated by their gradually increasing and significantly positive coefficient estimates on the post-variable ($\mathbb{1}\{t \geq e_i + d_i\}$), both for wages and the estimated AKM firm effect.

6.7 Summary of Results

While we consider the DiD specification estimated by OLS as the baseline, our results are robust to IV estimation, exploiting the random timing of accidents, and within-firm matching. Looking at the treatment effect on wage, the baseline OLS estimate is -2.87 percent, -1.11 percent, and -1.50 percent, upon return, 1 year after, and 2 years after, respectively. The range of point estimates under the various specifications is between (-3.83) and (-2.39) percent, (-1.97) and (-0.86) percent, and, (-1.61) and (-0.73) percent, upon return, 1 year after, and 2 years after the accident, respectively. Looking at the treatment effect on AKM firm effects, the baseline OLS estimate is -0.002, -0.0032, and -0.0055, upon return, 1 year after, and 2 years after, respectively. The range of point estimates under the various specifications is between (-0.0064) and (-0.0020), (-0.0119) and (-0.0032), and, (-0.0120) and (-0.0055), respectively.

7 Mechanisms: Decomposition of the Wage Loss into a Within- and a Between-Firm Component

According to previous results, being temporarily absent from work following a mild accident results in lower wages and lower firm wage premium (AKM firm effect), relative to what individuals would have experienced in the absence of a mild accident. Then, the question naturally arises: *to what extent do relatively lower wages for treated relative to control individuals arise from missed opportunities to switch to better, higher-paying, employers (between-firm wage loss) and/or missed opportunities of wage growth at a given employer (within-firm wage loss).* For an answer, we leverage the framework below.

Let $\Delta \ln(x)$ represent the difference in $\ln(x)$ between the actual and the counterfactual value of (any variable) x for the treated, due to the accident-induced absence. Let y represent current employer quality, ϵ represent the part of log-wages that is present regardless of the current employer the individual is working for, and Δw_{within} be the difference between the actual and the counterfactual log-wages for the treated, due to the accident-induced absence, which stems from missed opportunities of wage growth at a given employer ("within-firm wage loss"). Assume that $\ln(w)$ can be written as

$$\ln(w) = \gamma_0 + \gamma_1 y + \epsilon. \tag{7}$$

Then,

$$\Delta \ln(w) = \gamma_1 \Delta y + \Delta w_{\text{within}}, \tag{8}$$

where γ_1 is a constant and $\Delta w_{\text{within}} = \Delta\epsilon$. Intuitively, for any individual, the wage loss stemming from accident-induced absence can be decomposed into two parts: one that is due to missed opportunities to switch to better, higher-paying, employers ("between-firm wage loss": $\gamma_1 \Delta y$), and a second that is due to missed opportunities of wage growth at a given employer ("within-firm wage loss": Δw_{within}).

Next, note that $\gamma_1 y$, for each individual, represents the employer-specific wage premium they receive from working for their current employer that has productivity (quality) y. Then, across all treated individuals, the share of the wage loss due to the missed opportunities to switch to higher-paying employers is given by the expected decrease in the employer-specific wage premium, divided by the expected total wage loss, stemming from the absence following an accident:

$$\frac{E[\Delta \gamma_1 y]}{E[\Delta ln(w)]}. \tag{9}$$

To form the sample analogue estimator for (9), first recall that ϕ_j in the AKM equation (2) is exactly the time-invariant firm-specific wage premium (*i.e.,* $\phi_j = \gamma_1 y$); hence, for each individual, $\hat{\phi}_j$ is the sample counterpart of $\gamma_1 y$. Second, for the denominator, the interaction estimates in the odd columns of *Panel A* in Table 2 are the ATE estimates for $ln(w)$, corresponding to the average estimated $\Delta ln(w)$ across individuals. Analogously, for the numerator, the interaction estimates in the even columns are the ATE estimates for $\hat{\phi}_j$, corresponding to the average estimated $\Delta \gamma_1 y$.

Using the sample analogue estimator for (9) and estimates from Table 2, we back out the share of the wage loss due to missed opportunities to move to higher-paying employers (*i.e.*, ending up at worse employers relative to the counterfactual). Columns 1–2 of Table 2 *Panel A* imply that this share is 6.97 percent upon return to work, columns 3–4 imply that it is 28.8 percent 1 year later, and columns 5–6 imply that it is 36.7 percent 2 years later.[29] No matter if using the OLS or the IV estimates, the share of the wage loss due to missed opportunities to move to higher-paying employers is increasing with time.

In a similar vein, we can use the occupation-specific wage premia to do the decomposition by occupations. For that, we estimate occupation fixed effects of wages; then, the

[29] Upon return: 0.002/0.0287 = 6.97 percent; the corresponding p-value for testing if this ratio is different from 0 is 0.041 (using the Delta Method). 1 year later: 0.0032/0.0111 = 28.8 percent; the corresponding p-value is 0.042. 2 years later: 0.0055/0.015 = 36.7 percent; the corresponding p-value is 0.005. Our IV estimates from Table 2 *Panel B* imply higher shares: they imply 10.88, 79, and 92.4 percents of the drop in wages upon return, 1 year later, and 2 years later being due to worse employers. These larger estimated shares arise due to larger estimates of the treatment effect on the AKM firm effect under our IV strategy which identifies the effect of *accident-induced absences* for the *compliers*.

ATE estimates for the occupation fixed effects allow us to decompose how much of the ATE estimates for $ln(w)$ come from missed opportunities to move to better-paying occupations. We find that post-absence individuals do not end up in occupations with lower occupation-specific wage premia (estimates are small and insignificant); *e.g.*, only 0.7 percent of the wage loss upon return is due to missed opportunities to move to better-paying occupations, and this share does not increase over time (*see* Appendix Section D).

8 Theoretical Framework

In this section, we provide a theoretical framework capable of rationalizing the effects of absences from work following an accident. For tractability, we consider a framework of *ex ante* identical workers, also since in our empirical analysis we control for a rich set of individual-level characteristics. Our aim is not to propose a model with *all* mechanisms behind our results, but one which encompasses the most important mechanisms.

We consider the problem of individuals who receive external wage offers and accumulate human capital while working. Individuals can be working, absent from work (temporarily not supplying working hours), or unemployed. Firms bargain with workers over wages. There is no cost to wage renegotiation.

Let h denote the stock of human capital of a worker. While working, human capital increases every period by a quantity ϵ. While unemployed or absent from work, human capital decreases by a quantity ε.[30] We focus on general, not firm-specific, human capital.

Consistent with our empirical results, we assume that wages are downward rigid following an absence, even if human capital depreciated during the absence spell. As a result, upon recovery from the accident, when individuals return to work for their employer, their wages are the same as they were *prior* to the accident. This means that, upon return, the *current wage paid* to the worker differs from the wage the worker would be paid if she was paid according to her human capital, reflecting her labor productivity (which we refer to as *market wage*). As time progresses, workers' *market wage* increases *via* human capital accumulation, and the gap between the *market wage* and the *current wage paid* persists until the *market wage* surpasses the *current wage paid.*

With probability ψ an individual working receives an external wage offer. Employers differ by productivity $y \sim F(y)$, where $\infty > y \geq 0$. Upon receiving an external wage

[30] The assumption of human capital increasing with probability one while employed and decreasing with probability one while unemployed or absent is made for tractability, and to make model notation lighter. We assume that human capital only changes by fixed amounts ϵ or ε for the same reasons. Model propositions are unchanged if we consider positive probabilities lower than one, and human capital increases or decreases that vary by human capital level. Results with these modifications are available upon request from the authors.

offer, the current employer and the employer from which the external offer came compete for the worker. Following this competition, the worker ends up with the most productive firm which pays her the maximum she could have obtained with the less productive firm. A worker of human capital h using y' as outside option when bargaining with an employer of productivity y has *market wage* $w(y', y, h)$ where

$$\frac{\partial w(y', y, h)}{\partial y'} \geq 0, \frac{\partial w(y', y, h)}{\partial y} \geq 0, \quad \text{and} \quad \frac{\partial w(y', y, h)}{\partial h} \geq 0. \tag{10}$$

This wage setting procedure is inspired by Cahuc et al. (2006), who show that for high enough worker bargaining power, wages are increasing in current employer productivity. We focus on this particular case.[31] A person that has not yet received any external wage offers ($y' = 0$) is paid $w(0, y, h)$. *See* Appendix Section F for the model value functions.

Next, we derive the propositions that show how the model is able to rationalize and explain our empirical findings (proofs are provided in Appendix Section E.).

Let t denote the time since last unemployed. Let E_0 be the expectation operator given the information in the last period unemployed, normalized to be $t = 0$. Define the expectation at $t = 0$ of the wage at period $T + j$, w_{T+j}, after returning to work in $T + j$ following an accident occurring in period T as $E_0[w_{T+j}|\text{accident at } T]$. Next, define the expectation at $t = 0$ of the wage at period $T + j$, w_{T+j}, of not receiving an accident between T and $T + j$ as $E_0[w_{T+j}|\text{no accident}]$. Define $E_0[y_{T+j}|\text{accident at } T]$ as the expectation at $t = 0$ of the employer productivity at period $T + j$, y_{T+j}, after returning to work in $T + j$ following an accident occurring in period T. Finally, let $E_0[y_{T+j}|\text{no accident}]$ be the expectation at $t = 0$ of employer productivity at period $T + j$, y_{T+j}, if not receiving an accident between T and $T + j$. Then,

Proposition 1.

$$E_0[y_{T+j}|\text{accident at } T] < E_0[y_{T+j}|\text{no accident}], \forall j, \tag{11}$$

$$E_0[w_{T+j}|\text{accident at } T] < E_0[w_{T+j}|\text{no accident}], \forall j. \tag{12}$$

Proposition 1 indicates that consistent with our empirical results, the wage and firm productivity of an individual upon return to work from an absence (following an accident) is smaller than it would have been in the counterfactual scenario of no accident (and continuation of work). Propositions 2 and 3 below make it clear that the negative wage effect arises through two channels: human capital accumulation and the receival

[31] The particular case in which workers have no bargaining power upon renegotiation would correspond to the above wage setting with the modified property that wages are decreasing in current employer productivity, y (*see* Postel-Vinay and Robin, 2002). The case in which firms commit to initial wages and never renegotiate is captured by $w(y', y, h) = w(0, y, h), \forall y'$, as in Burdett and Mortensen (1998).

of external wage offers while working; at the same time, the negative firm productivity effect arises only through the channel of receiving external wage offers while working.

Proposition 2. *Suppose workers do receive external wage offers ($\psi > 0$), but do not accumulate or lose human capital ($\epsilon = 0, and, \varepsilon = 0$). Then, it is still the case that*

$$E_0[y_{T+j}|\text{accident at } T] < E_0[y_{T+j}|\text{no accident}], \forall j, \tag{13}$$

$$E_0[w_{T+j}|\text{accident at } T] < E_0[w_{T+j}|\text{no accident}], \forall j. \tag{14}$$

Proposition 3. *Suppose workers do accumulate human capital ($\epsilon > 0$), but do not receive external wage offers ($\psi = 0$), and $\varepsilon \geq 0$. Then,*

$$E_0[y_{T+j}|\text{accident at } T] = E_0[y_{T+j}|\text{no accident}], \forall j, \tag{15}$$

$$E_0[w_{T+j}|\text{accident at } T] \leq E_0[w_{T+j}|\text{no accident}], \forall j. \tag{16}$$

Intuitively, workers have lower wages upon return to work from an absence because they *miss opportunities to accumulate human capital* and *miss opportunities to receive external wage offers*, where the latter could lead to *missed opportunities to renegotiate wage with current employer* or *missed opportunities to move to a higher-paying firm.* Propositions 2 and 3 clarify that the effect of absence on firm productivity comes exclusively from the channel of external wage offers ($\psi > 0$). When we shut down the channel of external wage offers ($\psi = 0$), there is no on-the-job search, and thus workers never change employers while working. Under $\psi = 0$, since no offers arrive, the worker also has no external wage offers to force renegotiation with the current employer, and her wage only increases *via* human capital accumulation. Through the lens of the model, by verifying how much of the effect of absence on wages comes from reallocation to lower-paying employers, we separate how much of the treatment effect comes from *missed opportunities to move to a higher-paying firm* and how much comes from the combination of *missed opportunities to accumulate human capital* and *to renegotiate wage with current employer.*

Since wages exhibit downward rigidity (which is consistent with our empirical results that, upon return to work wages are the same as their pre-accident value), depreciation of human capital, although present, does not contribute to the wage loss upon return from absence (following an accident), relative to the counterfactual of no accident. In fact, depreciation of human capital only matters for how long the gap between treated and control lasts. Recall that upon return to work from an absence, wages only start to grow again after the wage under no downward rigidity, the *market wage*, surpasses the *current wage paid.* The more human capital depreciated during the absence, the lower will be the *market wage* of the individual upon return to work relative to the *current wage paid*, leading to a longer period without wage increases for the treated.

Lower wages lead to future decreases by making individuals less selective on future external wage offers and employers than they would have been otherwise. Hence,

$$E_0[w_{T+j+\delta}|\text{accident at } T\ ] < E_0[w_{T+j+\delta}|\text{no accident}], \forall j, \forall \delta. \tag{17}$$

$$E_0[y_{T+j+\delta}|\text{accident at } T\ ] < E_0[y_{T+j+\delta}|\text{no accident}], \forall j, \forall \delta. \tag{18}$$

Note that our framework also implies that when accidents do not lead to absences, the individual is still able to receive external offers and accumulate human capital, and thus there is no long run impact on wages and no reallocation of workers to worse firms.

Some final discussion is in order. First, in our model, we focus on human capital and wage ladders, but further mechanisms might also be at play. One possibility is that employers use absences to infer perceived "toughness" or "reliability" of the worker. This channel, if present, would likely be less important for long-tenured individuals. Indeed, the robustness of our empirical results for long-tenured workers suggests that this mechanism likely does not fully explain our findings (but we can not rule it out).

Second, we do not explicitly model where the wage offers arise from when working. These offers can arise from several sources, such as: (i) when workers are present, they may hear about a good job opportunity (either within or outside their firm), that allows them to apply and so they may receive a wage offer; (ii) competing employers may hear about workers who are present and perform well, and make them a wage offer; or (iii) when workers are present, they are "at hand" and thus are the ones that are getting a short-term task, in which they can either improve or impress their managers, and so on.

9 Conclusion

In this paper, we present new evidence that accident-induced periods of absence decrease individuals' wages for up to two years, by around 1.5 percent, relative to what they would have been in the absence of the accident, and that individuals end up with lower-quality employers. Thus, even short absences due to mild and unexpected accidents, with no persistent labor productivity losses, have persistent negative effects on trajectories of labor market outcomes, due to *foregone opportunities of wage growth*.

The persistent effects of *post-accident absence* on wages and firm fixed effects are stronger among young (20–29-years-old), short-tenured, and white-collar individuals, as well as among those who had lower-than-median wage and firm quality *prior* to the event. Our results are robust to restricting the sample to those who suffer an accident but at different points in time, exploiting the random timing of an accident. Finally, there are no permanent wage losses for individuals who had an accident but no subsequent absence spell, and they do not reallocate to lower-quality employers either.

Our results imply that even short absences can have long-run consequences for workers: they might miss opportunities to switch to better, higher-paying firms, or miss opportunities of wage growth at the same firm. Ending up at firms with relatively lower wage premium may have non-monetary consequences as well, since firms with lower wage premia are associated with worse amenities (Sockin, 2022), lower benefit uptake among their workers (Lachowska et al., 2022; Bana et al., 2023), worse management practices (Bender et al., 2018), are more likely to appeal claims to unemployment insurance (Lachowska et al., 2022), and are less preferred by workers (Sorkin, 2018), further worsening the expected well-being of individuals following an absence period from work. Our findings point to how public policies designed to insure individuals in the case of accidents should take into account not only the accident itself but the long-run income loss due to lower wage growth, faced by the individual due to accident-induced absence from work.

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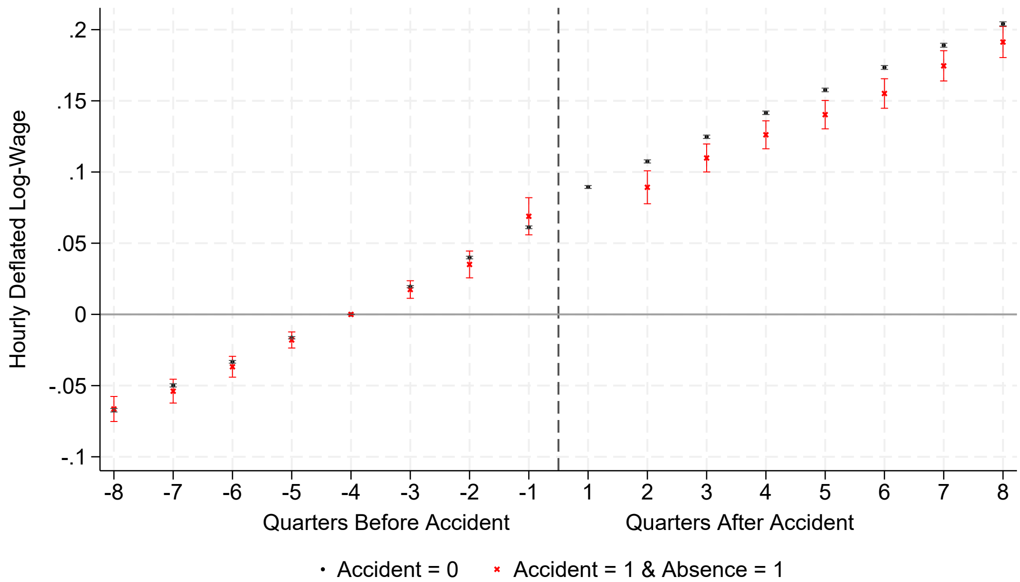
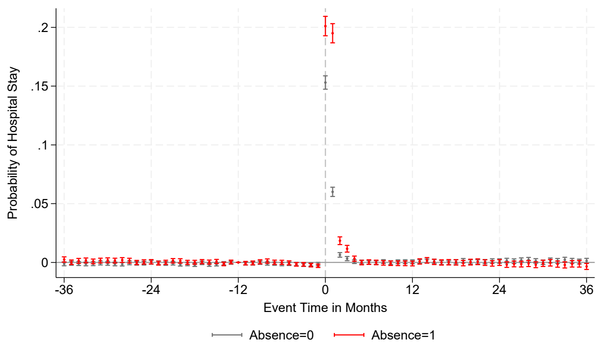
Figures

Figure 1: Wage Trajectory of Treated and Control Individuals Around the Event Time

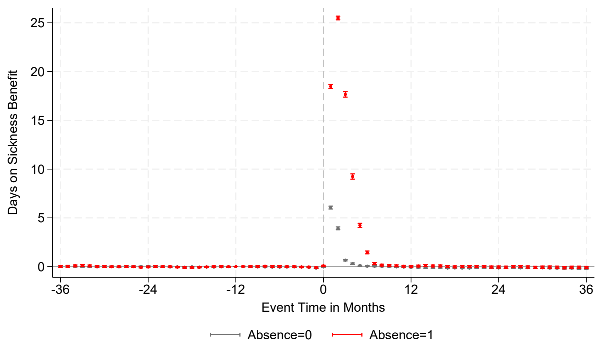


Notes: The figure was constructed by (i) taking all observations of treated individuals (*i.e.*, those suffering an accident and being absent for 3–6 months thereafter) and control individuals (*i.e.*, those with no accidents) in the 2-year-long window around the time of (i) the accident event and (ii) the pseudo-event, respectively; (ii) taking the residual of deflated hourly log-wage on individual fixed effects; and (iii) plotting the average of those residuals across quarters around the event, together with their 95 percent Confidence Intervals, for $q = -8, ..., -1, 1, ..., 8$; $q = 0$ corresponds to the month of the event and pseudo-event, respectively, and the average is normalized to 0 at $q = -4$. The control sample of individuals never have an accident, for them the month of the pseudo-event is chosen randomly (provided they work in that month).
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the pseudo-shock), 2009–2017, but restricting the wage observations to wages that are truly monthly wages, *i.e.*, not generated from job-spell level aggregate wage (the vast majority of wage measures are truly monthly wages between 2012-2017); *main sample*.

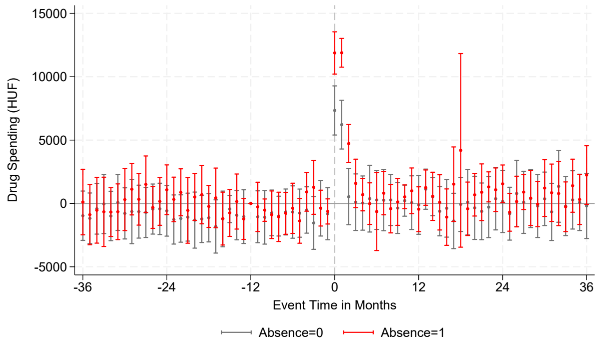
Figure 2: Event Study Plots for Individuals Suffering an Accident



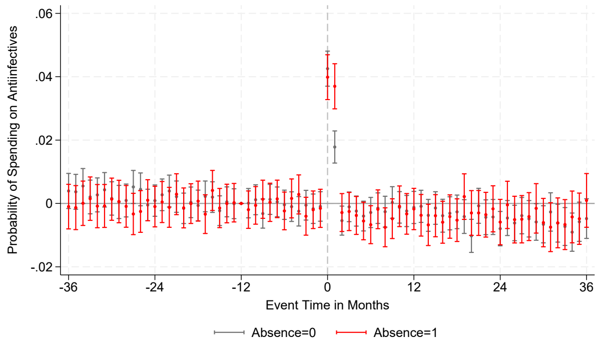
(a) At Least 5 Days per Month in Hospital



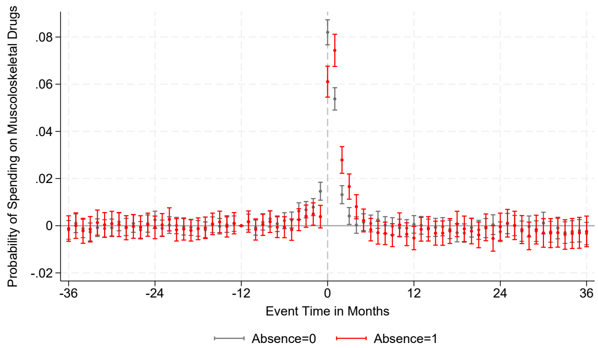
(b) Number of Days on Sickness Benefit



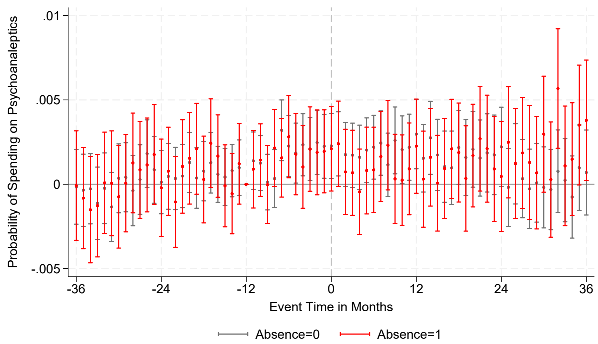
(c) Prescription Drug Spending (HUF)



(d) Any Spending on Antiinfectives



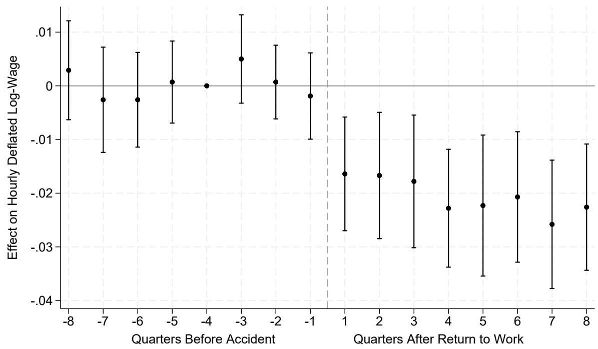
(e) Any Spending on Musculoskeletal Drugs



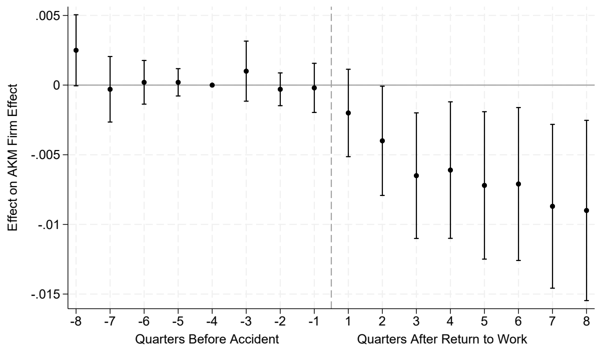
(f) Any Spending on Psychoanaleptics

Notes: We graph the $\hat{\delta}_k$'s stemming from estimating equation (3) separately for the subsample of those who are absent after the accident and who are not, with their 95 percent Confidence Intervals, for $k = -36, ..., 0, ...36$, to see if various indicators of sickness prevail before, around, and after the event, $k = 0$, with $k = -12$ as the reference event time period. Panels (d), (e) and (f) show the fraction of individuals with non-zero spending on three prescription drug categories: antiinfectives for systemic use (ATC J – antibiotics form a major group in this category); musculoskeletal drugs (ATC M – antirheumatic products and drugs for joint or muscle pain form major groups in this category) and psychoanaleptics (ATC N06 – antidepressants form a major group in this category). The Absence=0 group includes both people with zero months of absence as well as those that had an absence of less than 3 months. Control group from regressions are not included here since they are defined as individuals with no accident.
Data: Hungarian administrative matched employer-employee data, aged 20-50, with accidents, employed at the month of the shock or a month before, 2009–2017.

Figure 3: Estimated Effect of Accident and Absence Over Time



(a) Hourly Deflated Log-Wage



(b) AKM Firm Effect

Notes: Figure shows OLS estimation results from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1, replacing the post-return dummy indicator with the indicator of relative time (quarters before the accident or after return to work). Each point (and 95% confidence interval) on the figure originates from a separate regression, using the 4th quarter before accident as the reference time and displaying the interaction term between the relative time (event time) and having suffered an accident with a temporary absence thereafter. All control variables listed in Section 5.1.1 are included in all regression models, except for the lagged logarithm of wage and the lagged estimated firm AKM fixed effect, but adding the lagged indicators of firm size, foreign ownership, and firm level average wage for 5-8 quarters prior the event. The "control" group includes those who have not suffered an accident, and the "treated" group includes those who have suffered an accident and were absent from work for 3-6 months thereafter. The outcome in panel (a) is the hourly deflated log-wage, the outcome in panel (b) is the estimated AKM firm effect.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the pseudo-event), 2009–2017, but restricting the wage observations to wages that are truly monthly wages, *i.e.*, not generated from job-spell level aggregate wage (the vast majority of wage measures are truly monthly wages between 2012-2017).

Tables

Table 1: Distribution of Workers with Accidents (in Percents), by Accident Type and Body Part Affected

	$\mathbb{1}\{Absence_i = 1\}$				$\mathbb{1}\{Absence_i = 0\}$			
	injury, wound	dislocation, sprain	fracture	*total*	injury, wound	dislocation, sprain	fracture	*total*
shoulder, upper-arm	2.33	4.03	7.02	*13.39*	2.14	4.49	6.53	*13.16*
elbow, forearm	0.95	0.50	10.04	*11.49*	1.49	0.37	8.92	*10.77*
hand, fingers, wrist	9.49	0.49	9.85	*19.82*	11.07	0.64	11.08	*22.79*
hip, thigh	0.41	0.11	2.73	*3.25*	1.02	0.06	1.24	*2.32*
knee, lower-leg	7.79	13.17	22.83	*43.80*	5.09	24.67	9.91	*39.66*
ankle, foot, toes	0.22	1.64	4.59	*6.45*	0.31	1.56	2.66	*4.54*
head, neck, thorax, spine		1.81		*1.81*		6.75		*6.75*
total	*21.20*	*21.74*	*57.07*	*100*	*21.12*	*38.54*	*40.34*	*100*

Notes: The table shows the share of individuals (in percents), by accident type (injuries/wounds, dislocations/sprains, and fractures) and body part affected, separately for those who were and were not absent, following an accident ($N = 9,473$ and $N = 6,443$, respectively).
Data: Hungarian administrative matched employer-employee data, aged 20-50, with accidents, employed at the month of the accident or a month before, 2009–2017.

Table 2: Estimation Results of the Linear Difference-in-Difference Models (OLS and IV), on the Main Outcomes and Further Firm Quality Measure

Panel A: OLS Estimates	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	$ln(w_i)$	$\hat{\phi_j}$	$ln(w_i)$	$\hat{\phi_j}$	$ln(w_i)$	$\hat{\phi_j}$
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0094***	0.0001	0.0109***	-0.0004	0.0119***	-0.0004
	(0.0018)	(0.0007)	(0.0019)	(0.0007)	(0.0020)	(0.0008)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0285***	0.0028***	0.0791***	0.0042***	0.1244***	0.0050***
	(0.0025)	(0.0002)	(0.0051)	(0.0004)	(0.0074)	(0.0006)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ $\cdot\mathbb{1}\{t \geq e_i + d_i\}$	-0.0287***	-0.0020*	-0.0111***	-0.0032*	-0.0150***	-0.0055**
	(0.0031)	(0.0010)	(0.0033)	(0.0017)	(0.0040)	(0.0024)
R^2	0.876	0.919	0.870	0.870	0.847	0.831
N	927,504	907,402	877,885	864,751	805,839	801,532
Panel B: IV Estimates	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	$ln(w_i)$	$\hat{\phi_j}$	$ln(w_i)$	$\hat{\phi_j}$	$ln(w_i)$	$\hat{\phi_j}$
$\mathbb{1}\{Absence_i = 1\}$	0.0082***	0.0001	0.0099***	-0.0006	0.0109***	-0.0007
	(0.0026)	(0.0009)	(0.0027)	(0.0009)	(0.0029)	(0.0010)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0287***	0.0029***	0.0792***	0.0043***	0.1245***	0.0051***
	(0.0025)	(0.0002)	(0.0051)	(0.0004)	(0.0073)	(0.0006)
$\mathbb{1}\{Absence_i = 1\}$ $\cdot\mathbb{1}\{t \geq e_i + d_i\}$	-0.0239***	-0.0026*	-0.0086*	-0.0068***	-0.0079	-0.0073**
	(0.0036)	(0.0014)	(0.0046)	(0.0025)	(0.0060)	(0.0035)
R^2	0.876	0.919	0.870	0.869	0.848	0.831
N	938,690	918,530	888,867	875,606	815,928	811,589
Panel C: OLS Estimates Further Firm Quality Measure	(1)	(2)	(3)			
	upon return	after 1 year	after 2 years			
	TFP_j	TFP_j	TFP_j			
$\mathbb{1}\{Absence_i = 1\}$	0.0222**	0.0203**	0.0193*			
	(0.0098)	(0.0098)	(0.0098)			
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0105***	0.0138***	0.0224***			
	(0.0014)	(0.0021)	(0.0027)			
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ $\cdot\mathbb{1}\{t \geq e_i + d_i\}$	-0.0106**	-0.0199**	-0.0158			
	(0.0051)	(0.0093)	(0.0124)			
R^2	0.650	0.614	0.595			
N	794,195	766,030	705,961			

Notes: [1] The OLS estimates in Panels A and C—estimates of coefficients and standard errors—stem from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1. The IV estimates in Panel B—estimates of coefficients and standard errors—stem from estimating the "Linear Model using Instrumental Variables (IV)", presented in Section 5.1.2. [2] Standard error estimates are in parentheses, and are clustered at the monthly date level, *** p<0.01, ** p<0.05, * p<0.1. [3] All control variables listed in Section 5.1.1 are included in all regression models. [4] In the OLS sample, the "control" group includes those who have not suffered an accident, and the "treated" group includes those who have suffered an accident and were absent from work for 3-6 months thereafter (*i.e.*, for whom $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$=1). The IV sample also includes those, in addition to the OLS sample, for whom $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$=1). [5] $\mathbb{1}\{t \geq e_i + d_i\}$ is 1 if the individual is observed in the "post"-period (upon return to work, or 1 year after, or 2 years after). Pre-event values are the average of lag1,lag2,lag3 values of the given outcome variable, where the lag is relative to the event. [6] The various outcomes can be seen in the column titles: $ln(w_i)$ denotes the hourly deflated log-wage, $\hat{\phi_j}$ denotes the estimated AKM firm effect, and TFP denotes the Total Factor Productivity (TFP) measure of the firm. The baseline average of the AKM firm effect at the control group is 0.0285, with a standard deviation of 0.344. The baseline average of the employer TFP at the control group is 9.8, with a standard deviation of 1.6.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the shock), 2009–2017, *main sample* for the OLS estimates, and *IV sample* for the IV estimates.

Table 3: Estimation Results of the Linear Difference-in-Difference Model, On the Sample of Those Suffering an Accident (Robustness Check Exploiting the Random Timing of Accidents)

	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$
$\tilde{\mathbb{1}}\{ACC_i = 1\,\&\,Absence_i = 1\}$	0.0074**	0.0002	0.0029	-0.0010	-0.0049	-0.0030***
	(0.0029)	(0.0011)	(0.0028)	(0.0011)	(0.0030)	(0.0011)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0245***	0.0061***	0.0702***	0.0097***	0.1157***	0.0104***
	(0.0024)	(0.0003)	(0.0024)	(0.0004)	(0.0033)	(0.0004)
$\tilde{\mathbb{1}}\{ACC_i = 1\,\&\,Absence_i = 1\}$	-0.0274***	-0.0064***	-0.0090**	-0.0119***	-0.0073	-0.0120***
$\cdot\mathbb{1}\{t \geq e_i + d_i\}$	(0.0043)	(0.0016)	(0.0040)	(0.0026)	(0.0052)	(0.0032)
R^2	0.840	0.894	0.826	0.844	0.795	0.793
N	182,090	180,609	181,210	179,536	180,399	179,201

Notes: [1] The underlying Ordinary Least Squares (OLS) model is presented in Section 5.2. [2] Standard error estimates are in parentheses, and are clustered at the monthly date level, *** p<0.01, ** p<0.05, * p<0.1. [3] All control variables listed in Section 5.1.1 are included in all regression models. [4] The "control" group includes those who have not suffered an accident yet at a given time, and will suffer the accident at least 3 years from now. The "treated" group includes those who have suffered an accident and were absent from work for 3-6 months thereafter (*i.e.*, for whom $\tilde{\mathbb{1}}\{ACC_i = 1\,\&\,Absence_i = 1\}$=1). [5] $\mathbb{1}\{t \geq e_i + d_i\}$ is 1 if the individual is observed in the "post"-period (upon return to work, or 1 year after, or 2 years after). Pre-event values are the average of lag1,lag2,lag3 values of the given outcome variable, where the lag is relative to the event. [6] Outcomes are in the column titles: $ln(w_i)$ is the hourly deflated log-wage, and $\hat{\phi}_j$ is the AKM firm effect. The baseline average of the AKM firm effect at the control group is 0.0127, with a standard deviation of 0.328.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before, 2009–2017; *accident sample, exploiting random timing.*

Table 4: Within-Firm Matching Estimation Results

	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$
$A\hat{T}T$	-0.0383***	-0.0029***	-0.0197***	-0.0064***	-0.0161***	-0.0076**
	(0.0040)	(0.0010)	(0.0034)	(0.0024)	(0.0050)	(0.0031)
N	5,323,899	5,409,137	4,858,989	4,957,705	4,042,516	4,247,511
Matched Treated Obs.	5,656	6,450	5,779	5,858	4,723	4,879
Unmatched Treated Obs.	3,510	3,675	3,380	3,291	2,748	2,790

Notes: [1] Table reports kernel matching estimation results with exact matching on gender, firm identifier at the time of the accident, decile of wage at the time of the accident, decile of the AKM individual fixed effect, date of the accident (e_i), and absence duration (d_i). $A\hat{T}T$ abbreviates Average Treatment Effect on the Treated. [2] Bootstrap standard error estimates are in parentheses, *** p<0.01, ** p<0.05, * p<0.1. [3] All control variables listed in Section 5.1.1 are included as matching variables in all models, except for the characteristics of the employer. [4] The "control" group includes those who have not suffered an accident, and the "treated" group includes those who have suffered an accident and were absent from work for 3-6 months thereafter. [5] Outcomes are in the column titles: $ln(w_i)$ is the hourly deflated log-wage, and $\hat{\phi}_j$ is the AKM firm effect. The baseline average of the AKM firm effect at the control group is 0.166, with a standard deviation of 0.20.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the shock), 2009–2017; *main sample with full set of potential controls.*

For Online Publication: Appendix

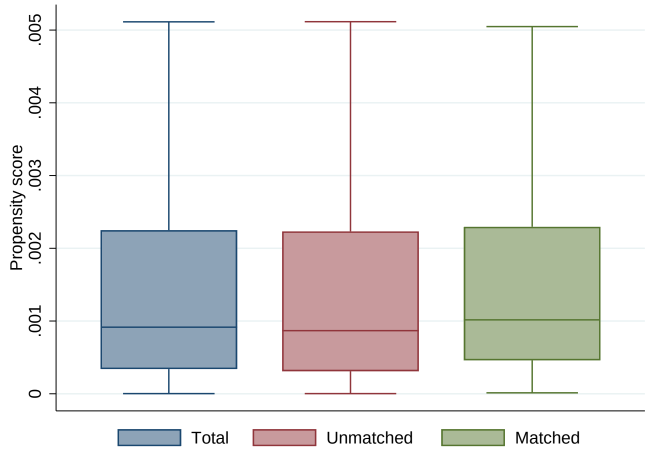
A Within-Firm Matching

Table A1: Balance Statistics

	Raw			Matched		
	Treated	Control	StdDif	Treated	Control	StdDif
Age						
20	0.011	0.004	0.076	0.009	0.007	0.031
21	0.014	0.008	0.056	0.014	0.012	0.021
22	0.018	0.012	0.055	0.019	0.017	0.018
23	0.023	0.014	0.064	0.022	0.022	0.001
24	0.024	0.016	0.056	0.023	0.023	0.002
25	0.024	0.017	0.047	0.023	0.023	-0.002
26	0.025	0.018	0.049	0.027	0.027	-0.002
27	0.023	0.019	0.033	0.024	0.026	-0.011
28	0.027	0.019	0.056	0.029	0.031	-0.018
29	0.027	0.019	0.052	0.027	0.028	-0.011
30	0.027	0.020	0.049	0.027	0.031	-0.027
31	0.029	0.021	0.049	0.029	0.034	-0.034
32	0.032	0.023	0.058	0.032	0.034	-0.014
33	0.032	0.025	0.042	0.035	0.036	-0.005
34	0.032	0.027	0.033	0.031	0.037	-0.034
35	0.039	0.028	0.059	0.040	0.040	0.000
36	0.037	0.030	0.038	0.038	0.039	-0.004
37	0.040	0.031	0.048	0.041	0.043	-0.011
38	0.037	0.032	0.027	0.040	0.045	-0.030
39	0.041	0.034	0.039	0.039	0.040	-0.006
40	0.042	0.034	0.040	0.041	0.042	-0.008
41	0.042	0.034	0.042	0.042	0.041	0.005
42	0.039	0.034	0.027	0.040	0.040	-0.002
43	0.040	0.033	0.040	0.039	0.037	0.011
44	0.037	0.032	0.025	0.035	0.037	-0.013
45	0.037	0.032	0.028	0.035	0.036	-0.001
46	0.039	0.031	0.044	0.040	0.036	0.019
47	0.038	0.030	0.045	0.040	0.036	0.025
48	0.041	0.029	0.063	0.040	0.033	0.038
49	0.040	0.029	0.061	0.038	0.031	0.037
50	0.042	0.028	0.079	0.041	0.034	0.042
Gender	0.779	0.556	0.489	0.774	0.774	0.000
Occupation						
Manager	0.054	0.057	-0.016	0.030	0.033	-0.014
Professional	0.038	0.075	-0.162	0.038	0.049	-0.045
Other white collar	0.129	0.260	-0.336	0.124	0.153	-0.073
Skilled blue collar	0.380	0.323	0.121	0.380	0.355	0.051
Assembler, machine operator	0.222	0.190	0.080	0.259	0.255	0.011
Unskilled	0.177	0.095	0.241	0.168	0.154	0.039
Log mean wage 4-12 m before	6.337	6.648	-0.613	6.443	6.446	-0.006
Log health spending 3-12 m before	6.656	7.039	-0.089	6.694	6.218	0.111
Any drug spending 3-12 m before						
Alimentary tract, metabolism (ATC A)	0.120	0.152	-0.095	0.119	0.110	0.026
Blood, blood forming organs (ATC B)	0.050	0.062	-0.052	0.049	0.033	0.068
Cardiovascular (ATC C)	0.150	0.240	-0.229	0.156	0.148	0.020
Dermatologicals (ATC D)	0.071	0.083	-0.048	0.073	0.067	0.022
Genito-urinary system (ATC G)	0.021	0.036	-0.088	0.020	0.022	-0.014
Hormonal preparations (ATC H)	0.031	0.038	-0.039	0.031	0.025	0.034
Antiinfectives (ATC J)	0.280	0.289	-0.020	0.288	0.268	0.043
Antineoplastic, immudomodulating (ATC L)	0.002	0.004	-0.037	0.002	0.003	-0.009
Musculoskeletal system (ATC M)	0.164	0.169	-0.012	0.164	0.136	0.076
Nervous system (ATC N)	0.059	0.064	-0.020	0.058	0.048	0.043
Antiparasatic producucts (ATC P)	0.008	0.010	-0.024	0.007	0.007	-0.001
Respiratory system (ATC R)	0.106	0.119	-0.040	0.108	0.099	0.026
Sensory organs (ATC S)	0.046	0.046	0.000	0.046	0.043	0.013
Various (ATC V)	0.001	0.002	-0.012	0.001	0.001	0.006

Notes: Table shows mean values under the average treatment effect on the treated estimation before ("Raw") and after ("Matched") matching. Columns "StdDif" report standardized differences.

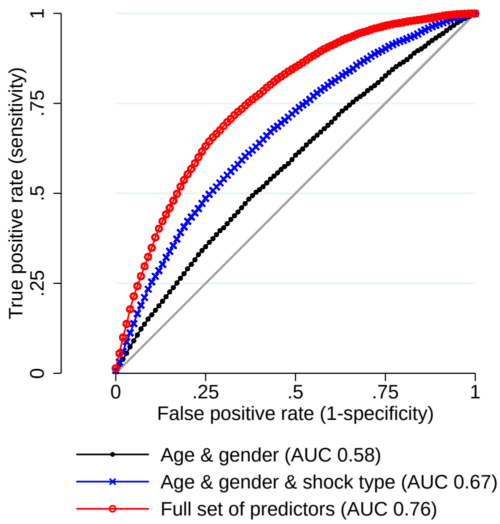
Figure A1: Common Support



Notes: Figure shows the distribution (box plot) of the propensity score of suffering an accident (*i.e.*, belonging to the treatment group) in the total, unmatched and matched sample. The horizontal line in the box indicates the median, the bottom and top of the box indicate the 25th and 75th percentile, respectively. The lowest and highest horizontal lines indicate the lower and upper adjacent values, respectively. The adjacent value is the top (bottom) end of the box plus (minus) 1.5 times the interquartile range (with the lower adjacent value censored at zero). **Data:** Hungarian administrative matched employer-employee data, aged 20-50, suffering an accident and employed at the month of the shock or a month before, 2009–2017.

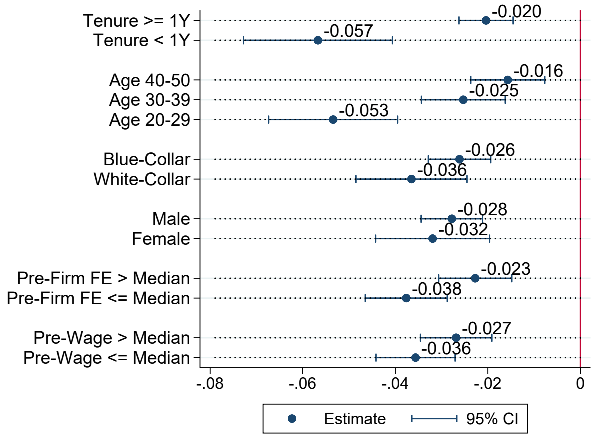
B Further Figures

Figure B1: ROC Curve of Absence Prediction After the Accident

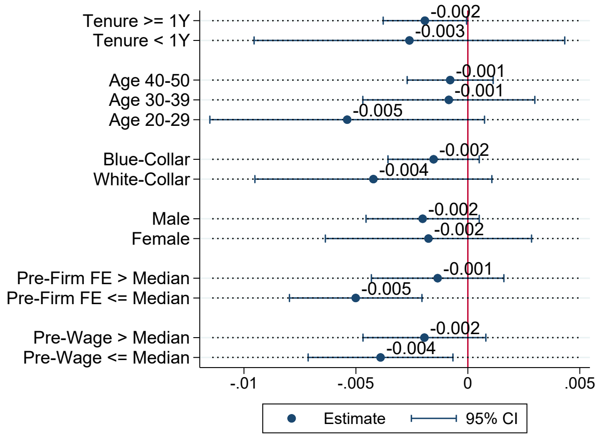


Notes: The ROC (Receiver Operating Characteristic) curve has on the x-axis the false positive rate (proportion of incorrectly predicted absence among all actual no-absence observations, which equals one minus specificity), and on the y-axis the true positive rate (proportion of correctly predicted absence among all actual absence observations, which is called sensitivity). AUC is the abbreviation for area under the ROC curve. The gray line is the 45-degree, corresponding to random prediction. The other three lines are based on logit models of absence. We always include the time of the shock as predictor. The black curve includes age dummies and gender as predictors. The blue curve adds shock types (body part affected and diagnosis codes) as predictors. The red curve adds all predictors we use as control variables in equation (4). **Data:** Hungarian administrative matched employer-employee data, aged 20-50, suffering an accident and employed at the month of the shock or a month before, 2009–2017.

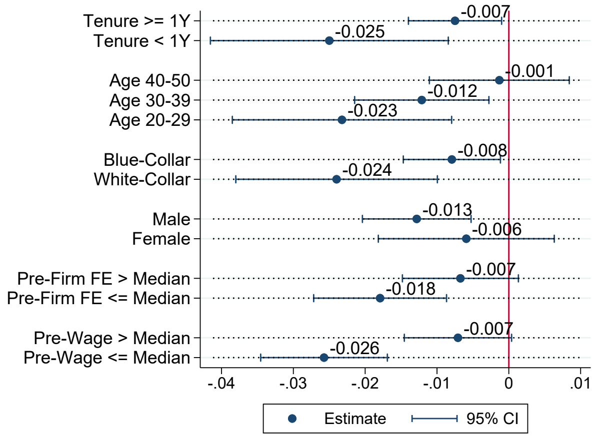
Figure B2: Heterogeneity of the Main DiD (Interaction) Estimate on the Main Outcomes, by Tenure, Age, Occupation, Gender, and Pre-Event AKM Firm Effect and Wage



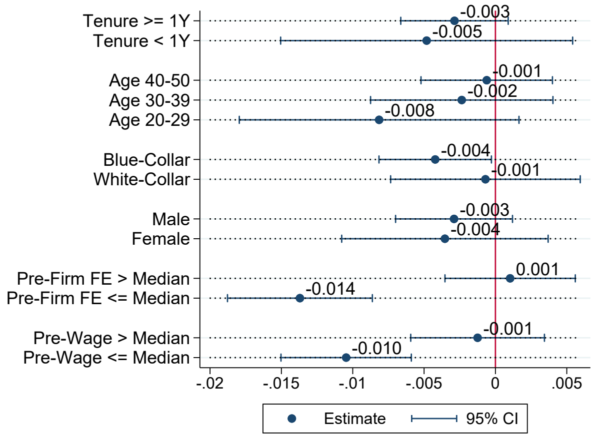
(a) Hourly Deflated Log-Wage, Upon Return



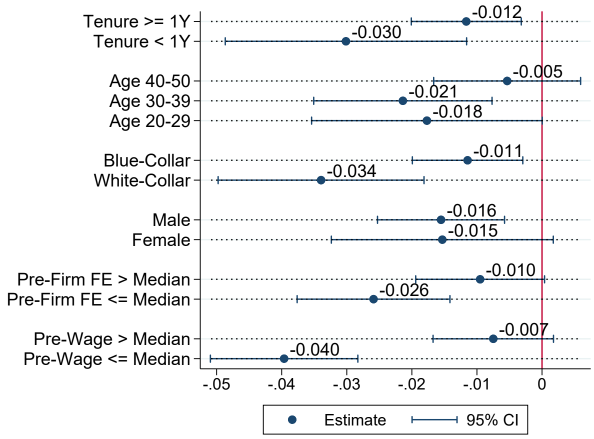
(b) AKM Firm Effect, Upon Return



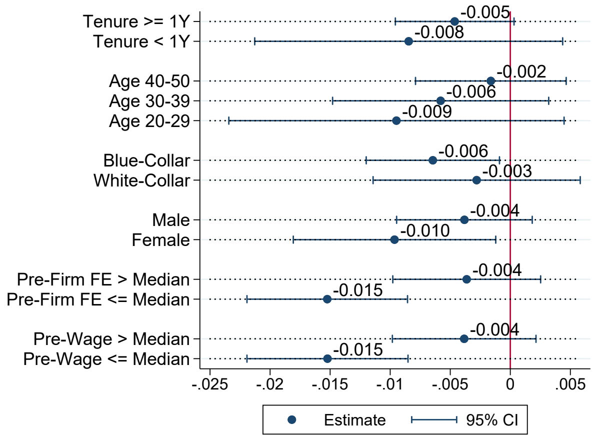
(c) Hourly Deflated Log-Wage, Upon Return +1 Year



(d) AKM Firm Effect, Upon Return +1 Year



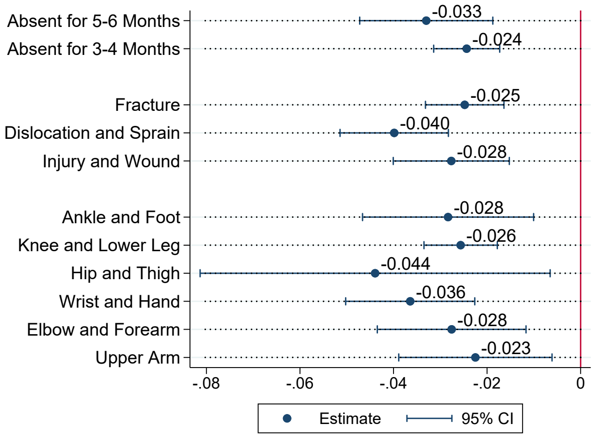
(e) Hourly Deflated Log-Wage, Upon Return +2 Years



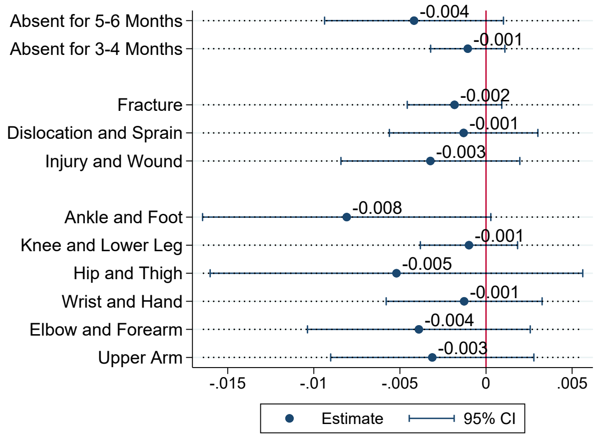
(f) AKM Firm Effect, Upon Return +2 Years

Notes: These figures show the estimated coefficient and its 95 percent Confidence Interval, on the interaction term, stemming from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1, for the main six outcomes, using separate sub-samples as indicated on the vertical axis. The interaction term is $\mathbb{1}\{ACC_i = 1\,\&\,Absence_i = 1\} \cdot \mathbb{1}\{t \geq e_i + d_i\}$. Standard error estimates are clustered at the monthly date level.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the pseudo-shock), 2009–2017; sub-groups within the *main sample*.

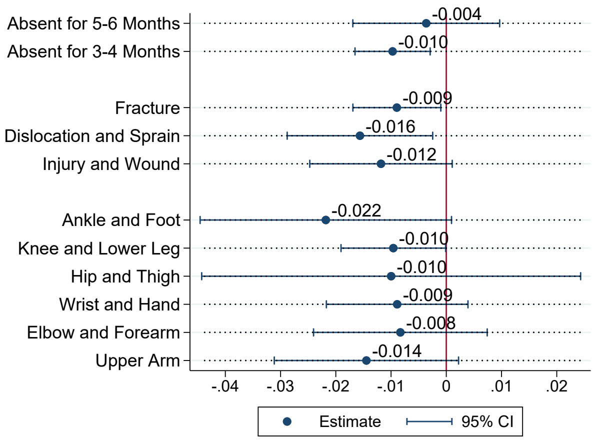
Figure B3: Heterogeneity of the Main DiD (Interaction) Estimate on the Main Outcomes, by Type of Injury, Body Part Injured, and Length of Absence



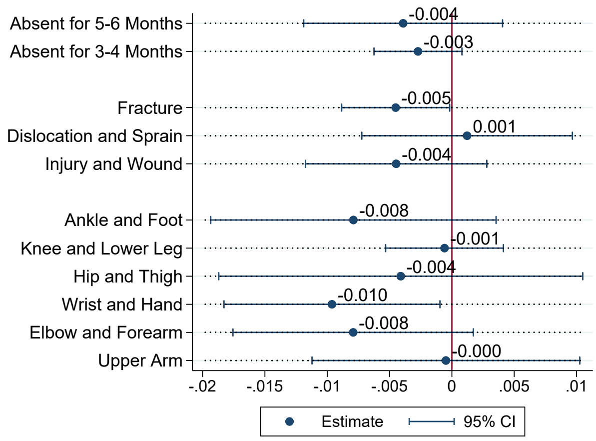
(a) Hourly Deflated Log-Wage, Upon Return



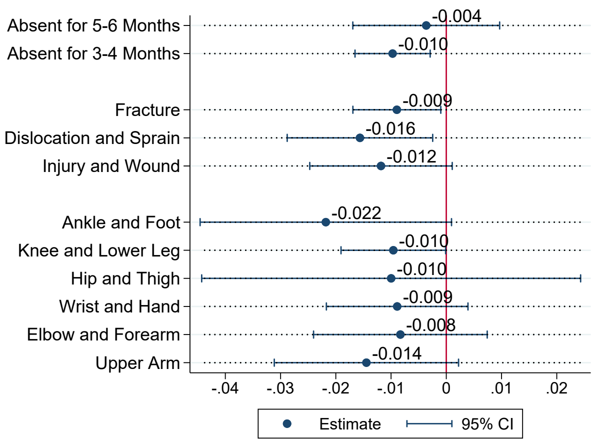
(b) AKM Firm Effect, Upon Return



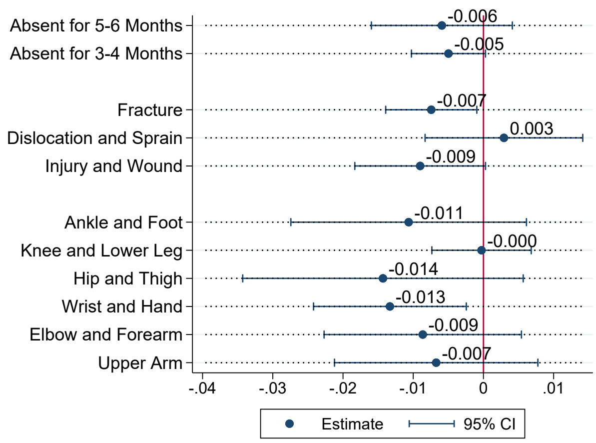
(c) Hourly Deflated Log-Wage, Upon Return + 1 Year



(d) AKM Firm Effect, Upon Return + 1 Year



(e) Hourly Deflated Log-Wage, Upon Return + 2 Years



(f) AKM Firm Effect, Upon Return + 2 Years

Notes: These figures show the estimated coefficient and its 95 percent Confidence Interval, on the interaction term, stemming from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1, for the main six outcomes, using separate accident type sub-samples, as indicated on the vertical axis. The interaction term is $\mathbb{1}\{ACC_i = 1\,\&\, Absence_i = 1\} \cdot \mathbb{1}\{t \geq e_i + d_i\}$. Standard error estimates are clustered at the monthly date level.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the pseudo-shock), 2009–2017, sub-groups within the *main sample*.

C Further Tables

Table C1: Distribution of Blue-Collar and White-Collar Workers with Accidents and Absence (in Percents), by Accident Type and Body Part Affected

	Blue-Collar Workers				*White-Collar Workers*			
	injury, wound	dislocation, sprain	fracture	*total*	injury, wound	dislocation, sprain	fracture	*total*
shoulder, upper-arm	2.47	4.26	6.61	*13.33*	1.87	3.27	8.45	*13.59*
elbow, forearm	1.12	0.53	10.15	*11.81*	0.37	0.37	9.67	*10.42*
hand, fingers, wrist	11.10	0.52	10.97	*22.59*	4.02	0.37	5.98	*10.37*
hip, thigh	0.41	0.11	2.52	*3.04*	0.42	0.09	3.46	*3.97*
knee, lower-leg	6.28	12.42	21.93	*40.63*	12.98	15.79	25.78	*54.55*
ankle, foot, toes	0.23	1.57	4.94	*6.74*	0.19	1.87	3.41	*5.46*
head, neck, thorax, spine		1.86		*1.86*		1.63		*1.63*
total	*21.61*	*21.26*	*57.13*	*100*	*19.85*	*23.40*	*56.75*	*100*

Notes: The table shows the share of individuals (in percents) with accidents and being absent, by accident type and body part, separately for blue-collar and white-collar individuals ($N = 7,327$ and $N = 2,141$, respectively).
Data: Hungarian administrative matched employer-employee data, aged 20-50, with accidents and absence thereafter, employed at the month of the shock or a month before (controls employed at the month of the shock), 2009–2017.

Table C2: Descriptive Statistics (Sample Means for Treated and Control Individuals)

	(1)	(2)	(3)
	$\mathbb{1}\{Accident_i = 1\}$		$\mathbb{1}\{Accident_i = 0\}$
	$\mathbb{1}\{Absence_i = 1\}$	$\mathbb{1}\{Absence_i = 0\}$	
male	0.776	0.764	0.562
age (in years)	36.938	36.032	36.531
blue-collar occupation	0.773	0.511	0.581
in manufacturing	0.299	0.237	0.262
in trade	0.145	0.166	0.178
estimated AKM individual FE	-0.127	0.060	-0.026
hourly log wage (pre)	6.347	6.560	6.470
estimated AKM firm FE (pre)	-0.013	0.034	0.029
in foreign firm (pre)	0.280	0.304	0.325
n. of employees of the firm (pre)	1,601	1,214	1,696
log health expenditures (year prior)	6.693	6.902	6.661

Notes: The table shows the mean of various control variables, separately for those who suffered and who have not suffered an accident ($N = 15,916$ and $N = 492,281$ (in column (3)), respectively), as well as separately for those who were and were not absent, following an accident ($N = 9,473$ and $N = 6,443$, respectively, in columns (1) and (2)). Over the analyzed period (2009–2017), one euro cost 291 Hungarian forints (HUF).
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the pseudo-shock), 2009–2017; *IV sample.*

Table C3: Estimation Results of the Linear Difference-in-Difference Model (OLS) on Occupation Characteristics (Stressful and Physically Demanding Nature of the Job), For All and By Occupation

Panel A: For All	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	std.physical	*std.stress*	*std.physical*	*std.stress*	*std.physical*	*std.stress*
$\mathbb{1}\{ACC_i = 1\,\&\,Absence_i = 1\}$	0.0656***	0.0626***	0.0675***	0.0644***	0.0673***	0.0650***
	(0.0066)	(0.0090)	(0.0067)	(0.0090)	(0.0067)	(0.0090)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0033**	0.0009	0.0086***	0.0042*	0.0173***	0.0038
	(0.0015)	(0.0012)	(0.0029)	(0.0022)	(0.0038)	(0.0028)
$\mathbb{1}\{ACC_i = 1\,\&\,Absence_i = 1\}$	-0.0058*	0.0004	-0.0012	0.0010	-0.0029	-0.0125
$\cdot\mathbb{1}\{t \geq e_i + d_i\}$	(0.0030)	(0.0036)	(0.0060)	(0.0083)	(0.0081)	(0.0105)
R^2	0.659	0.246	0.643	0.234	0.635	0.228
N	931,206	931,206	828,899	828,899	768,151	768,151
Panel B: For White-Collar	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	std.physical	*std.stress*	*std.physical*	*std.stress*	*std.physical*	*std.stress*
$\mathbb{1}\{ACC_i = 1\,\&\,Absence_i = 1\}$	0.0634***	0.0650***	0.0576***	0.0681***	0.0529***	0.0691***
	(0.0152)	(0.0151)	(0.0154)	(0.0152)	(0.0153)	(0.0151)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0102***	0.0011	0.0376***	0.0042	0.0607***	0.0052
	(0.0016)	(0.0013)	(0.0029)	(0.0027)	(0.0038)	(0.0037)
$\mathbb{1}\{ACC_i = 1\,\&\,Absence_i = 1\}$	0.0007	0.0016	0.0266**	-0.0091	0.0613***	-0.0068
$\cdot\mathbb{1}\{t \geq e_i + d_i\}$	(0.0065)	(0.0083)	(0.0113)	(0.0128)	(0.0174)	(0.0180)
R^2	0.214	0.301	0.212	0.283	0.211	0.272
N	387,638	387,638	347,162	347,162	321,279	321,279
Panel C: For Blue-Collar	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	std.physical	*std.stress*	*std.physical*	*std.stress*	*std.physical*	*std.stress*
$\mathbb{1}\{ACC_i = 1\,\&\,Absence_i = 1\}$	0.0463***	0.0404***	0.0469***	0.0424***	0.0464***	0.0441***
	(0.0073)	(0.0108)	(0.0074)	(0.0108)	(0.0074)	(0.0108)
$\mathbb{1}\{t \geq e_i + d_i\}$	-0.0016	0.0013	-0.0130***	0.0035	-0.0148***	0.0023
	(0.0016)	(0.0026)	(0.0030)	(0.0049)	(0.0040)	(0.0067)
$\mathbb{1}\{ACC_i = 1\,\&\,Absence_i = 1\}$	-0.0051	-0.0010	0.0002	0.0001	-0.0048	-0.0183
$\cdot\mathbb{1}\{t \geq e_i + d_i\}$	(0.0032)	(0.0041)	(0.0066)	(0.0094)	(0.0091)	(0.0131)
R^2	0.361	0.236	0.339	0.226	0.327	0.221
N	543,401	543,401	481,479	481,479	446,617	446,617

Notes: [1] The OLS estimates—estimates of coefficients and standard errors—stem from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1. [2] Standard error estimates are in parentheses, and are clustered at the monthly date level, *** p<0.01, ** p<0.05, * p<0.1. [3] All control variables listed in Section 5.1.1 are included in all regression models. [4] The "control" group includes those who have not suffered an accident, and the "treated" group includes those who have suffered an accident and were absent from work for 3-6 months thereafter (*i.e.*, for whom $\mathbb{1}\{ACC_i = 1\,\&\,Absence_i = 1\}$=1). [5] $\mathbb{1}\{t \geq e_i + d_i\}$ is 1 if the individual is observed in the "post"-period (upon return to work, or 1 year after, or 2 years after). Pre-event values are the average of lag1,lag2,lag3 values of the given outcome variable, where the lag is relative to the event. [6] The various outcomes can be seen in the column titles: *std.stress* stands for a standardized index of the stressful nature of an occupation. *std.physical* denotes a standardized index of how physically demanding an occupation is. Both measures are created using O*NET occupational characteristics, as defined in Section 3.3.4.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the pseudo-shock), 2009–2017, *main sample*.

Table C4: Estimation Results of the Linear Difference-in-Difference Models (OLS), on the Main Outcomes, Selecting d for Individuals in the Control Group Randomly so that the Distribution of the Treated Across d Matches the Distribution of Controls Across d

Panel A: OLS Estimates	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0072***	-0.0003	0.0090***	-0.0009	0.0103***	-0.0010
	(0.0018)	(0.0007)	(0.0019)	(0.0007)	(0.0019)	(0.0008)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0263***	0.0027***	0.0773***	0.0044***	0.1219***	0.0050***
	(0.0023)	(0.0002)	(0.0049)	(0.0004)	(0.0072)	(0.0006)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ $\cdot\mathbb{1}\{t \geq e_i + d_i\}$	-0.0265***	-0.0019*	-0.0092***	-0.0034*	-0.0124***	-0.0055**
	(0.0030)	(0.0010)	(0.0033)	(0.0017)	(0.0040)	(0.0024)
R^2	0.881	0.922	0.873	0.875	0.849	0.835
N	907,958	888,371	868,639	854,236	796,140	790,970

Notes: [1] The OLS estimates—estimates of coefficients and standard errors—stem from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1, but with choosing d not fully randomly, but so that the distribution of control individuals matches the distribution of treated individuals, across d. [2] Standard error estimates are in parentheses, and are clustered at the monthly date level, *** p<0.01, ** p<0.05, * p<0.1. [3] All control variables listed in Section 5.1.1 are included in all regression models. [4] In the OLS sample, the "control" group includes those who have not suffered an accident, and the "treated" group includes those who have suffered an accident and were absent from work for 3-6 months thereafter (*i.e.*, for whom $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$=1). Pre-event values are the average of lag1,lag2,lag3 values of the given outcome variable, where the lag is relative to the event. [6] The various outcomes can be seen in the column titles: $ln(w_i)$ denotes the hourly deflated log-wage, $\hat{\phi}_j$ denotes the estimated AKM firm effect.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the shock), 2009–2017, *main sample.*

Table C5: Estimation Results of the Linear Difference-in-Difference Models (OLS), on the Main Outcomes Excluding Control-Group Co-workers in Small Firms

Panel A: OLS Estimates	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0088***	0.0001	0.0104***	-0.0004	0.0112***	-0.0005
	(0.0018)	(0.0007)	(0.0019)	(0.0007)	(0.0019)	(0.0008)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0276***	0.0026***	0.0782***	0.0039***	0.1232***	0.0048***
	(0.0025)	(0.0002)	(0.0051)	(0.0004)	(0.0073)	(0.0006)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ $\cdot\mathbb{1}\{t \geq e_i + d_i\}$	-0.0287***	-0.0018*	-0.0101***	-0.0029*	-0.0139***	-0.0053**
	(0.0031)	(0.0010)	(0.0033)	(0.0017)	(0.0040)	(0.0024)
R^2	0.879	0.922	0.874	0.875	0.852	0.840
N	882,092	861,298	836,962	822,900	771,626	765,827

Notes: [1] The OLS estimates—estimates of coefficients and standard errors—stem from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1 with the additional restriction of excluding all control individuals that work in the same firm as the treated prior to the accident if the firm in question had at most 50 control-group employees in our sample (between the period before the accident and 2 years after return). [2] Standard error estimates are in parentheses, and are clustered at the monthly date level, *** p<0.01, ** p<0.05, * p<0.1. [3] All control variables listed in Section 5.1.1 are included in all regression models. [4] In the OLS sample, the "control" group includes those who have not suffered an accident (excluding those that worked in the same firm as the treated in a firm with 50 employees or less prior to the accident), and the "treated" group includes those who have suffered an accident and were absent from work for 3-6 months thereafter (*i.e.*, for whom $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$=1). Pre-event values are the average of lag1,lag2,lag3 values of the given outcome variable, where the lag is relative to the event. [6] The various outcomes can be seen in the column titles: $ln(w_i)$ denotes the hourly deflated log-wage, $\hat{\phi}_j$ denotes the estimated AKM firm effect. The baseline average of the AKM firm effect at the control group is 0.0285, with a standard deviation of 0.344.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the pseudo-shock), 2009–2017, *main sample.*

Table C6: Estimation Results of the Linear Difference-in-Difference Model (OLS) on Wage Income, Labor Supply (Hours Worked), and Alternative Hourly Deflated Log-Wage

Panel A: Baseline	(1)	(2)	(3)	(4)	(5)	(6)
		$ln(w) - baseline$			*weekly hours worked*	
	upon return	+1 year	2 years	upon return	+1 year	2 years
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0094***	0.0109***	0.0119***	0.1426***	0.1539***	0.1586***
	(0.0018)	(0.0019)	(0.0020)	(0.0478)	(0.0477)	(0.0473)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0285***	0.0791***	0.1244***	0.0301***	0.0806***	0.1234***
	(0.0025)	(0.0051)	(0.0074)	(0.0094)	(0.0171)	(0.0267)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ $\cdot\mathbb{1}\{t \geq e_i + d_i\}$	-0.0287***	-0.0111***	-0.0150***	-0.0571*	-0.1521***	-0.1295**
	(0.0031)	(0.0033)	(0.0040)	(0.0341)	(0.0501)	(0.0600)
R^2	0.876	0.870	0.847	0.221	0.221	0.211
N	927,504	877,885	805,839	937,422	881,542	809,893

Panel B: Other Wage Measures	(1)	(2)	(3)	(4)	(5)	(6)
		$ln(w_{month})$			$ln(w)$ - *using monthly days insured*	
	upon return	+1 year	2 years	upon return	+1 year	2 years
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0132***	0.0145***	0.0149***	0.0090***	0.0107***	0.0117***
	(0.0023)	(0.0024)	(0.0025)	(0.0017)	(0.0018)	(0.0019)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0301***	0.0806***	0.1234***	0.0305***	0.0807***	0.1260***
	(0.0027)	(0.0055)	(0.0079)	(0.0025)	(0.0051)	(0.0073)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ $\cdot\mathbb{1}\{t \geq e_i + d_i\}$	-0.0395***	-0.0144***	-0.0158***	-0.0200***	-0.0088***	-0.0132***
	(0.0031)	(0.0040)	(0.0047)	(0.0030)	(0.0031)	(0.0041)
R^2	0.831	0.823	0.799	0.880	0.873	0.850
N	935,909	881,539	810,858	927,504	877,885	805,839

Panel C: Other Wage Measures	(1)	(2)	(3)	(4)	(5)	(6)
	$ln(w_{month})$ *using monthly days insured*			$ln(w_{month})$ *using monthly days with income*		
	upon return	+1 year	2 years	upon return	+1 year	2 years
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0138***	0.0151***	0.0156***	0.0136***	0.0151***	0.0157***
	(0.0024)	(0.0025)	(0.0025)	(0.0023)	(0.0024)	(0.0025)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0324***	0.0827***	0.1260***	0.0327***	0.0830***	0.1262***
	(0.0026)	(0.0055)	(0.0078)	(0.0026)	(0.0054)	(0.0078)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ $\cdot\mathbb{1}\{t \geq e_i + d_i\}$	-0.0223***	-0.0127***	-0.0141***	-0.0203***	-0.0131***	-0.0145***
	(0.0033)	(0.0038)	(0.0049)	(0.0031)	(0.0038)	(0.0049)
R^2	0.834	0.825	0.800	0.835	0.825	0.801
N	928,318	879,287	808,292	928,275	879,236	808,249

Notes: [1] The OLS estimates—estimates of coefficients and standard errors—stem from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1. [2] Standard error estimates are in parentheses, and are clustered at the monthly date level, *** p<0.01, ** p<0.05, * p<0.1. [3] All control variables listed in Section 5.1.1 are included in all regression models. [4] The "control" group includes those who have not suffered an accident, and the "treated" group includes those who have suffered an accident and were absent from work for 3-6 months thereafter (*i.e.*, for whom $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$=1). [5] $\mathbb{1}\{t \geq e_i + d_i\}$ is 1 if the individual is observed in the "post"-period (upon return to work, or 1 year after, or 2 years after). Pre-event values are the average of lag1,lag2,lag3 values of the given outcome variable, where the lag is relative to the event. [6] The various outcomes can be seen in the column titles: $ln(w_{hour})$ denotes the hourly deflated log-wage. The baseline version is the same as $ln(w_i)$ in Table 2. $ln(w_{month})$ denotes the monthly deflated logarithm of wage income. In the right part of panel B we adjust the calculated hourly wage with the actual monthly days worked, defined as being eligible to social insurance. In panel C we do the same adjustment for monthly wages, using either the days with social insurance or the days with an income. *weekly hours worked* refers to hours as of contract, winsorized at 40 from above and at 20 from below.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the shock), 2009–2017, *main sample.*

Table C7: Estimation Results of the Linear Difference-in-Difference Model (OLS) on Main Outcomes, For Individuals With Accident But No Absence Spell, By Months After the (Accident) Event

Panel A	$ln(w_{hour})$ - *logarithm of deflated hourly wage*					
Months After Event:	1	2	3	4	5	6
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$	0.0036***	0.0037***	0.0041***	0.0040***	0.0042***	0.0041***
	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0163***	0.0201***	0.0251***	0.0294***	0.0343***	0.0386***
	(0.0021)	(0.0022)	(0.0023)	(0.0026)	(0.0029)	(0.0032)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$	-0.0270***	-0.0125***	-0.0094***	-0.0068***	-0.0026	0.0011
$\cdot\mathbb{1}\{t \geq e_i + d_i\}$	(0.0031)	(0.0024)	(0.0020)	(0.0019)	(0.0020)	(0.0022)
R^2	0.894	0.891	0.888	0.885	0.882	0.879
N	985,442	975,073	967,232	959,254	951,174	943,138

Panel B	$ln(w_{hour})$ - *logarithm of deflated hourly wage*					
Months After Event:	7	8	9	10	11	12
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$	0.0039***	0.0041***	0.0044***	0.0043***	0.0045***	0.0050***
	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0014)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0427***	0.0472***	0.0519***	0.0560***	0.0600***	0.0643***
	(0.0035)	(0.0037)	(0.0038)	(0.0041)	(0.0044)	(0.0048)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$	-0.0017	0.0012	-0.0014	-0.0001	0.0017	0.0006
$\cdot\mathbb{1}\{t \geq e_i + d_i\}$	(0.0026)	(0.0025)	(0.0025)	(0.0028)	(0.0029)	(0.0027)
R^2	0.876	0.874	0.870	0.867	0.865	0.863
N	935,226	927,872	920,788	914,083	907,632	900,901

Panel C	$\hat{\phi}_j$ - *estimated AKM firm effect*					
Months After Event:	1	2	3	4	5	6
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$	0.0005	0.0004	0.0004	0.0003	0.0003	0.0002
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0017***	0.0021***	0.0026***	0.0032***	0.0035***	0.0038***
	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$	-0.0012**	-0.0012**	-0.0016***	-0.0012*	-0.0010	-0.0006
$\cdot\mathbb{1}\{t \geq e_i + d_i\}$	(0.0005)	(0.0006)	(0.0006)	(0.0007)	(0.0008)	(0.0008)
R^2	0.940	0.934	0.928	0.923	0.917	0.912
N	980,798	969,655	959,987	951,375	943,489	935,908

Panel D	$\hat{\phi}_j$ - *estimated AKM firm effect*					
Months After Event:	7	8	9	10	11	12
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$	0.0001	0.0000	-0.0000	-0.0001	-0.0001	-0.0001
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0041***	0.0045***	0.0047***	0.0048***	0.0052***	0.0055***
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$	-0.0003	-0.0005	-0.0008	-0.0005	-0.0014	-0.0016
$\cdot\mathbb{1}\{t \geq e_i + d_i\}$	(0.0009)	(0.0011)	(0.0012)	(0.0012)	(0.0013)	(0.0014)
R^2	0.906	0.901	0.895	0.890	0.886	0.882
N	928,828	922,156	915,924	909,727	903,734	897,582

Notes: [1] The OLS estimates—estimates of coefficients and standard errors—stem from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1. [2] Standard error estimates are in parentheses, and are clustered at the monthly date level, *** p<0.01, ** p<0.05, * p<0.1. [3] All control variables listed in Section 5.1.1 are included in all regression models. [4] The "control" group includes those who have not suffered an accident, and the "treated" group includes those who have suffered an accident and had no absence spell thereafter (*i.e.*, for whom $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 0\}$=1). [5] $\mathbb{1}\{t \geq e_i + d_i\}$ is 1 if the individual is observed in the "post"-period (1–12 months after the (accident) event). Pre-event values are the average of lag1,lag2,lag3 values of the given outcome variable, where the lag is relative to the event. [6] The various outcomes can be seen in the column titles: $ln(w_{hour})$ denotes the hourly deflated log-wage, and $\hat{\phi}_j$ is the estimated AKM firm effect. The baseline average of the AKM firm effect at the control group is 0.0285, with a standard deviation of 0.344.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the shock), 2009–2017, *falsification sample.*

D Details on Occupation Decomposition

In this Section we describe how we can decompose how much of the between-firm and within-firm wage loss stems from missed opportunities to move to better-paying occupations. For the decomposition, we consider the difference between the actual and the counterfactual log-wage, and decompose that into the occupation-specific wage premium and a within-occupation component. First, we assume that $\log(w)$ can be written as

$$\log(w) = \Lambda_0 + \Lambda_1 \log(\boldsymbol{\varkappa}) + \zeta^G, \tag{19}$$

where $\Lambda_1 \log(\boldsymbol{\varkappa})$ represents the occupation-specific wage premium workers receive from working in their current occupation;[32] *i.e.,* $\Lambda_1 \log(\boldsymbol{\varkappa})$ captures the part of the wage that changes when individuals move to better-paying occupations. In contrast, ζ^G captures part of the wage that is present regardless of the individual's current occupation. Then, the difference between the actual and the counterfactual log-wage for the treated, due to accident-induced absence, $\Delta \log(w)$, can be written as

$$\Delta \log(w) = \Lambda_1 \Delta \log(\boldsymbol{\varkappa}) + \Delta\zeta^G, \tag{20}$$

where $\Lambda_1 \Delta \log(\boldsymbol{\varkappa})$ stems from missed opportunities to move to better-paying occupations, and $\Delta\zeta^G$ stems from missed opportunities of wage growth in the same occupation.

Second, let us decompose $\Lambda_1 \Delta \log(\boldsymbol{\varkappa})$ into the part stemming from missed opportunities to move to better-paying occupations *with the same employer*, $\Phi\Delta \log(\boldsymbol{\varkappa})$, and into the part stemming from missed opportunities to move to better-paying occupations *with other employers*, $\Gamma\Delta \log(\boldsymbol{\varkappa})$. In other words, $\Lambda_1 \Delta log(\kappa) \equiv \Phi\Delta \log(\boldsymbol{\varkappa}) + \Gamma\Delta \log(\boldsymbol{\varkappa})$. Similarly, we can decompose $\Delta\zeta^G$ into the part stemming from missed opportunities of wage growth *with the same employer in the same occupation*, $\Delta\zeta^W$, and into the part stemming from missed opportunities to move to *higher-paying employers in the same occupation*, $\Delta\zeta^B$, *i.e.*, $\Delta\zeta^G \equiv \Delta\zeta^W + \Delta\zeta^B$. Once we replace $\Lambda_1 \Delta log(\kappa)$ and $\Delta\zeta^G$ by their expressions, we obtain

$$\Delta \log(w) = \Lambda_1 \Delta \log(\boldsymbol{\varkappa}) + \Delta\zeta^G \equiv \Phi\Delta \log(\boldsymbol{\varkappa}) + \Gamma\Delta \log(\boldsymbol{\varkappa}) + \Delta\zeta^W + \Delta\zeta^B. \tag{21}$$

Third, note that $\Gamma\Delta \log(\boldsymbol{\varkappa}) + \Delta\zeta^B$ represents the total difference between the actual and the counterfactual log-wage for the treated, due to accident-induced absence, stemming from missed opportunities to move to better, higher-paying, employers (either higher paying occupations with new employers, $\Gamma\Delta log(\kappa)$, or higher-paying employers in the same occupation, $\Delta\zeta^B$). It follows that

$$\Gamma\Delta \log(\boldsymbol{\varkappa}) + \Delta\zeta^B \equiv \gamma_1 \Delta y, \tag{22}$$

where $\gamma_1 \Delta y$ represents the change in employer-specific wage premium workers receive when moving employers. Similarly, $\Phi\Delta \log(\boldsymbol{\varkappa}) + \Delta\zeta^W$ represents the total difference be-

[32] As shall be clear in a few paragraphs, this assumed structure for wages is consistent with the wage formulation in equation (7). Furthermore, a wage structure without an occupation-specific premium is just a particular case of this equation, in which $\Lambda_1 = 0$.

tween the actual and the counterfactual log-wage for the treated, due to accident-induced absence, stemming from missed opportunities of wage growth at a given employer (either higher paying occupations with the same employer, $\phi\Delta log(\kappa)$, or within-occupation and employer wage growth, $\Delta\zeta^W$). It follows that

$$\Phi\Delta\log(\boldsymbol{\varkappa}) + \Delta\zeta^W \equiv \Delta w_{\text{within}} \tag{23}$$

where Δw_{within} is the total increase in wages workers obtain from staying with their same employer. Then, from equations (22) and (23), it follows that

$$\Delta\log(w) = \Phi\Delta\log(\boldsymbol{\varkappa}) + \Gamma\Delta\log(\boldsymbol{\varkappa}) + \Delta\zeta^W + \Delta\zeta^B \equiv \gamma_1\Delta y + \Delta w_{\text{within}}, \tag{24}$$

confirming that the wage formulation we consider in this Section is internally consistent with that given by equation (7) in Section 7.

Now we are in a position to characterize shares of wage losses due to various missed opportunities. First, note that $(\Phi+\Gamma)\log(\boldsymbol{\varkappa}) \equiv \Lambda_1\log(\boldsymbol{\varkappa})$ is the wage premium an individual receives from working in her current occupation. Then, across all treated individuals, the share of the wage loss due to the missed opportunities to switch to better-paying occupations is given by the expected decrease in the occupation-specific wage premium, divided by the expected total wage loss, stemming from accident-induced absence:

$$\frac{E[\Delta(\Phi+\Gamma)\log(\boldsymbol{\varkappa})]}{E[\Delta\log(w)]}. \tag{25}$$

Second, note that $\Phi\Delta\log(\boldsymbol{\varkappa})$ is the part of the difference between the actual and the counterfactual log-wage for the treated, due to the accident-induced absence, that stems from missed opportunities to move to better-paying occupations *with the same employer*. Then, across all treated individuals, the share of the wage loss due to the missed opportunities to switch to better-paying occupations *with the same employer* is given by the expected decrease in the occupation-specific wage premium, from which the employer fixed effect has been partialled out, divided by the expected total wage loss:

$$\frac{E[\Delta\Phi\log(\boldsymbol{\varkappa})]}{E[\Delta\log(w)]}. \tag{26}$$

To arrive at sample analogue estimators for (25) and (26), we estimate occupation fixed effects of wages. We use two-digit occupation (ISCO) codes and estimate two alternative variants of equation (2): in the first variant, we do not include firm fixed effects in the model, in the second variant we include firm fixed effects in the model:

$$\ln(w_{ijot}) = X'_{ijot}\beta + \vartheta_i + z_o + \varepsilon_{ijot}, \tag{27a}$$
$$\ln(w_{ijot}) = X'_{ijot}\tilde{\beta} + \tilde{\vartheta}_i + \tilde{\phi}_j + \tilde{z}_o + \tilde{\varepsilon}_{ijot}, \tag{27b}$$

where $\ln(w_{ijot})$ is the logarithmic wage of worker i at firm j in occupation o at time t, X_{ijot} is a vector of time-varying observable characteristics including $(age-40)^2$, $(age-40)^3$ and year effects, ϑ_i ($\tilde{\vartheta}_i$) is time-invariant worker ability (or worker type), ϕ_j ($\tilde{\phi}_j$) is the time-invariant firm-specific wage premium, z_o ($\tilde{z}_o$) is the time-invariant occupation-specific wage premium, and ε_{ijot} ($\tilde{\varepsilon}_{ijot}$) is the time-varying error term. In the following, we call the estimated vectors $\hat{z}_o$ and $\hat{\tilde{z}}_o$ "estimated occupation fixed effects".

In (27a), z_o captures the expected wage of a given occupation o (conditional on X_{ijot} and ϑ_i). In contrast, in (27b), $\tilde{z}_o$ captures the expected wage of a given occupation o, additionally controlling for firm effects, therefore partialling out between-firm variation. The identification of z_o in (27a) requires that there are switches/reallocation of individuals across occupations. The identification of $\tilde{\phi}_j$ and $\tilde{z}_o$ in (27b) requires switches/reallocation of individuals across firms, and across occupations within the same firm, respectively.

To form the sample analogue estimator for (25), first recall that z_o in the AKM equation, not controlling for firm fixed effects, (27a), is the time-invariant occupation-specific wage premium (*i.e.*, $z_o = (\Phi + \Gamma)\log(\boldsymbol{\varkappa})$); hence, for each individual, their occupation's estimated fixed effect, $\hat{z}_o$, is the sample counterpart of $(\Phi + \Gamma)\log(\boldsymbol{\varkappa})$. Then, for the numerator, the interaction estimates in the odd columns of Table D1 are the ATE estimates for $\hat{z}_o$, corresponding to the average estimated $\Delta(\Phi + \Gamma)\log(\boldsymbol{\varkappa})$ across individuals. (For the denominator, just as before, the interaction estimates in the odd columns of *Panel A* in Table 2 are the ATE estimates for $\log(w)$, corresponding to the average estimated $\Delta\log(w)$ across individuals.)

To form the sample analogue estimator for (26), first recall that $\tilde{z}_o$ in the AKM equation, controlling for firm fixed effects, (27b), is the time-invariant occupation-specific wage premium (*i.e.*, $\tilde{z}_o = \Phi\log(\boldsymbol{\varkappa})$); hence, for each individual, their occupation's estimated fixed effect, $\hat{\tilde{z}}_o$, is the sample counterpart of $\Phi\log(\boldsymbol{\varkappa})$. Second, for the numerator, the interaction estimates in the even columns of Table D1 are the ATE estimates for $\hat{z}_o$, corresponding to the average estimated $\Delta\Phi\log(\boldsymbol{\varkappa})$ across individuals.

Using the sample analogue estimator for (25) and estimates from Tables 2 and D1, we back out the share of the wage loss due to missed opportunities to move to better-paying occupations (*i.e.*, ending up at worse occupations relative to the counterfactual). Column 1 of Table D1 reports results of estimating our OLS model (4) for $\hat{z}_o$ (the *occupation fixed effect estimated from a wage regression not controlling for firm fixed effect*).[33] The second row tells us that $\hat{z}_o$ (*i.e.*, the occupation-specific wage premium) is higher by 0.0004 (s.e. 0.0001) for the control group after the (random) event – consistent with the framework in which individuals experience wage growth by moving to better-paying occupations. Our ATE estimate for $\hat{z}_o$ is -0.0002 (s.e. 0.0004) upon return to work, which, is insignifant at any level. Dividing this ATE estimate for $\hat{z}_o$ by the ATE estimate for log-wages (from Column 1 of *Panel A* in Table 2) tells us that only 0.8 percent of the wage drop upon return is due to missed opportunities to move to better-paying occupations (and this estimate is statistically insignificant).[34]

Using the sample analogue estimator for (26) and estimates from Tables 2 and D1, we back out the share of the wage loss due to missed opportunities to move to better-paying occupations *with the same employer*. Column 2 of Table D1 reports results of

[33]We restrict the samples in Table D1 to observations where the log wage and AKM firm effect are not missing.

[34]$(-0.0001769)/(-0.0287) = 0.0080464$; the p-value for testing if this ratio is different from 0 is 0.516 (using the Delta Method).

estimating our OLS model (4) for $\hat{z}_o$ (the *occupation fixed effect estimated from a wage regression controlling for firm fixed effect*). Our ATE estimate for $\hat{\hat{z}}_o$ is -0.0002 (s.e. 0.0003) upon return to work; dividing this ATE estimate for $\hat{\hat{z}}_o$ by the ATE estimate for log-wages (from Column 1 of *Panel A* in Table 2) tells us that 0.6 percent of the wage drop is due to missed opportunities to move to better-paying occupations *with the same employer*, and this estimate is also statistically insignificant.[35] These numbers imply that of the wage drop due to missed opportunities to move to higher paying occupations, 76.58 percent comes from missed opportunities to switches with the same employer.[36] Looking at Column 3, 4, 5, and 6, we see that share of wage effects originating from occupation transitions 1 and 2 years later is close to zero and statistically insignificant.

Table D1: Estimation Results of the Linear Difference-in-Difference Model (OLS) on Occupation Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	$\hat{z}_o$	$\hat{\hat{z}}_o$	$\hat{z}_o$	$\hat{\hat{z}}_o$	$\hat{z}_o$	$\hat{\hat{z}}_o$
	firm FE not taken out	*firm FE taken out*	*firm FE not taken out*	*firm FE taken out*	*firm FE not taken out*	*firm FE taken out*
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0017***	-0.0003	0.0016***	-0.0004	0.0016***	-0.0004
	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0003)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0004***	0.0004***	0.0015***	0.0014***	0.0015***	0.0015***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ $\cdot\mathbb{1}\{t \geq e_i + d_i\}$	-0.0002	-0.0002	0.0010	0.0006	0.0008	0.0005
	(0.0004)	(0.0003)	(0.0007)	(0.0006)	(0.0009)	(0.0008)
R^2	0.831	0.908	0.771	0.850	0.749	0.827
N	895,513	895,513	849,940	849,940	780,299	780,299

Notes: [1] The OLS estimates—estimates of coefficients and standard errors—stem from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 5.1.1, with having the estimated occupation fixed effects (FE) as outcome variable. [2] Standard error estimates are in parentheses, and are clustered at the monthly date level, *** p<0.01, ** p<0.05, * p<0.1. [3] All control variables listed in Section 5.1.1 are included in all regression models. [4] The "control" group includes those who have not suffered an accident, and the "treated" group includes those who have suffered an accident and were absent from work for 3-6 months thereafter (*i.e.*, for whom $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$=1). [5] $\mathbb{1}\{t \geq e_i + d_i\}$ is 1 if the individual is observed in the "post"-period (upon return to work, or 1 year after, or 2 years after). Pre-event values are the average of lag1,lag2,lag3 values of the given outcome variable, where the lag is relative to the event. [6] The outcome is the estimated occupation fixed effects, specific to two-digit occupation codes. In the odd numbered columns, firm fixed effect is not taken out when estimating the occupation fixed effects (equation (27a)). The baseline average of this occupation fixed effect at the control group is -0.0083, with a standard deviation of 0.103. In the even numbered columns, firm fixed effect is taken out when estimating the occupation fixed effects (equation (27b)). The baseline average of this occupation fixed effect at the control group is -0.0078, with a standard deviation of 0.104.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before (control individuals employed at the month of the shock), 2009–2017, *main sample.*

[35] $(-0.0001769)/(-0.0287) = 0.0061627$; the p-value for testing if this ratio is different from 0 is 0.571 (using the Delta Method).
[36] $0.006162/0.0080464 = 0.7658$.

E Proofs

Proof of Proposition 1. Consider the case of an individual who was paid a wage $w_{-1}(y', y_{-1}, h)$ prior to the accident. First, let's show that the employer productivity of the individual upon recovery is lower than it would have been otherwise. First, let's consider the case of an individual being paid her *market wage*. Then,

$$E_T[y_{T+j}|\text{accident at } T, y_{-1}] = y_{-1} < \psi \int_{y_{-1}}^{\infty} x dF(x) + \psi \int_{0}^{y_{-1}} y_{-1} dF(x) + (1-\psi) y_{-1}$$
$$= E_T[y_{T+1}|\text{no accident}, y_{-1}] < E_T[y_{T+2}|\text{no accident}, y_{-1}]$$
$$< E_T[y_{T+3}|\text{no accident}, y_{-1}] < ... < E_T[y_{T+j}|\text{no accident}, y_{-1}], \forall y_{-1}. \quad (28)$$

Hence, it follows that

$$E_T[y_{T+j}|\text{accident at } T] < E_T[y_{T+j}|\text{no accident}]. \quad (29)$$

Now using the law of iterated expectations

$$E_0[y_{T+j}|\text{accident at } T] < E_0[y_{T+j}|\text{no accident}]. \quad (30)$$

Now consider the case of an individual who is being paid a wage different than her *market wage*. Consider an individual with human capital h, being paid a wage $w_{-1}(y', y_{-1}, \hat{h})$ last period. First, let us consider the case in which $w_{-1}(y', y_{-1}, \hat{h}) < w(y_{-1}, y'', h), \forall y'' > y_{-1}$. In this situation, the worker changes employer whenever she draws a $y'' > y_{-1}$. As a result,

$$E_T[y_{T+j}|\text{accident at } T, y_{-1}] = y_{-1} < \psi \int_{y_{-1}}^{\infty} x dF(x) + \psi \int_{0}^{y_{-1}} y_{-1} dF(x) + (1-\psi) y_{-1}$$
$$= E_T[y_{T+1}|\text{no accident}, y_{-1}] < E_T[y_{T+2}|\text{no accident}, y_{-1}]$$
$$< E_T[y_{T+3}|\text{no accident}, y_{-1}] < ... < E_T[y_{T+j}|\text{no accident}, y_{-1}], \forall y_{-1}. \quad (31)$$

Using the law of iterated expectations,

$$E_0[y_{T+j}|\text{accident at } T] < E_0[y_{T+j}|\text{no accident}]. \quad (32)$$

Finally, consider the case of an individual with human capital h, being paid a wage $w_{-1}(y', y_{-1}, \hat{h})$ last period when $\exists q$ such that $q > y_{-1}$ and $w_{-1}(y', y_{-1}, \hat{h}) = w(y_{-1}, q, h)$. Note that because w is increasing in all arguments this implies, $w_{-1}(y', y_{-1}, \hat{h}) \leq w(y_{-1}, y'', h), \forall y'' \geq q$ and $w_{-1}(y', y_{-1}, \hat{h}) > w(y_{-1}, y'', h), \forall y'' < q$. In this situation, the worker changes employer whenever she draws a $y'' \geq q(y', y_{-1}, \hat{h})$. It follows that

$$E_T[y_{T+j}|\text{accident at } T, y_{-1}] = y_{-1} < \psi \int_{q(y',y_{-1},\hat{h})}^{\infty} x dF(x) + \psi \int_0^{q(y',y_{-1},\hat{h})} y_{-1} dF(x) + (1-\psi) y_{-1}$$

$$= E_T[y_{T+1}|\text{no accident}, y_{-1}] < E_T[y_{T+2}|\text{no accident}, y_{-1}]$$

$$< E_T[y_{T+3}|\text{no accident}, y_{-1}] < ... < E_T[y_{T+j}|\text{no accident}, y_{-1}], \forall y_{-1}. \quad (33)$$

Using the law of iterated expectations,

$$E_0[y_{T+j}|\text{accident at } T] < E_0[y_{T+j}|\text{no accident}]. \quad (34)$$

Since $\frac{\partial w(y',x,h)}{\partial x} > 0$, there always exists a q such that $\forall y'' > q$, $w_{-1}(y', y_{-1}, \hat{h}) < w(y_{-1}, y'', h)$. As a result, when the individual is not paid her market wage the only two cases to consider are the ones above.

Next, let us consider wages. First, let's consider the case of an individual who is being paid their *market wage*. Then,

$$E_T[w_{T+j}|\text{accident at } T, w_{-1}(y', y, h)] = w_{-1}(y', y, h) < \psi \int_y^{\infty} w(y, x, h+\epsilon) dF(x)$$

$$+ \psi \int_{y'}^{y} w(x, y, h+\epsilon) dF(x) + (1 - \psi F(y')) w_{-1}(y', y, h+\epsilon)$$

$$= E_T[w_{T+1}|\text{no accident, employed at } T+1, w_{-1}(y', y, h_j)]$$

$$= E_T[w_{T+1}|\text{no accident}, w_{-1}(y', y, h_j)] < E_T[w_{T+2}|\text{no accident}, w_{-1}(y', y, h_j)]$$

$$< E_T[w_{T+3}|\text{no accident}, w_{-1}(y', y, h_j)] < ...$$
$$< E_T[w_{T+j}|\text{no accident}, w_{-1}(y', y, h_j)], \forall w_{-1}(y', y, h_j). \quad (35)$$

Hence,

$$E_T[w_{T+j}|\text{accident at } T] < E_T[w_{T+j}|\text{no accident}] \quad (36)$$

Now using the law of iterated expectations

$$E_0[w_{T+j}|\text{accident at } T] = E_0[w_{T+1}|\text{no accident}] < E_0[w_{T+j}|\text{no accident}], \forall j. \quad (37)$$

Now consider the case of an individual who is being paid a wage different than their *market wage*. If this is the case, it must be that the individual is being paid a wage higher than their *market wage*. Consider an individual with human capital h, being paid a wage $w_{-1}(y', y, \hat{h})$ last period. Then,

$$E_T[w_{T+j}|\text{accident at } T, w_{-1}(y',y,\hat{h}),h] = w_{-1}(y',y,\hat{h})$$

$$< \psi \int_y^\infty \max\{w_{-1}(y',y,\hat{h}), w(y,x,h+\epsilon)\}dF(x)$$

$$+\psi \int_{y'}^{y} \max\{w_{-1}(y',y,\hat{h}), w(x,y,h+\epsilon)\}dF(x) + (1-\psi F(y'))\max\{w_{-1}(y',y,\hat{h}), w(y',y,h+\epsilon)\}$$

$$= E_T[w_{T+1}|\text{no accident, employed at } T+1, w_{-1}(y',y,\hat{h}),h]$$

$$= E_T[w_{T+1}|\text{no accident}, w_{-1}(y',y,\hat{h}),h] < E_T[w_{T+2}|\text{no accident}, w_{-1}(y',y,\hat{h}),h]$$

$$< E_T[w_{T+3}|\text{no accident}, w_{-1}(y',y,\hat{h}),h] < ...$$

$$< E_T[w_{T+j}|\text{no accident}, w_{-1}(y',y,\hat{h}),h], \forall w_{-1}(y',y,\hat{h}),h. \quad (38)$$

Hence,

$$E_T[w_{T+j}|\text{accident at } T] < E_T[w_{T+j}|\text{no accident}] \quad (39)$$

Now using the law of iterated expectations

$$E_0[w_{T+j}|\text{accident at } T] = E_0[w_{T+1}|\text{no accident}] < E_0[w_{T+j}|\text{no accident}], \forall j. \quad (40)$$

□

Proof of Proposition 3. Just redo Proof of Proposition 1 with $\psi = 0$.

In this case, trivially, as long as the individual is employed they do not change employers (there is no on the job search) and so y does not change. As a result,

$$E_0[y_{T+j}|\text{accident at } T] = E_0[y_{T+j}|\text{no accident}]. \quad (41)$$

Now for wages, first consider the case of an individual who is being paid their *market wage*. Then,

$$E_T[w_{T+j}|\text{accident at } T, w_{-1}(y',y,h)] = w_{-1}(y',y,h) < w_{-1}(y',y,h+\epsilon)$$

$$= E_T[w_{T+1}|\text{no accident}, w_{-1}(y',y,h)] < E_T[w_{T+2}|\text{no accident}, w_{-1}(y',y,h)]$$

$$< ... < E_T[w_{T+j}|\text{no accident}, w_{-1}(y',y,h)], \forall w_{-1}(y',y,h). \quad (42)$$

Then using again the law of iterated expectations as in Proposition 1 we get

$$E_0[w_{T+j}|\text{accident at } T] < E_0[w_{T+j}|\text{no accident}], \forall j. \quad (43)$$

Now consider the case of an individual who is being paid a wage different than their *market wage*. If this is the case, it must be that the individual is being paid a wage higher than their *market wage*. Consider an individual with human capital h, being paid a wage $w_{-1}(y', y, \hat{h})$ last period. Then,

$$\begin{aligned}
E_T[w_{T+j}|\text{accident at } T, w_{-1}(y', y, \hat{h}), h] &= w_{-1}(y', y, \hat{h}) \\
&\leq \max\{w_{-1}(y', y, \hat{h}), w(y', y, h+\epsilon)\} \\
&= E_T[w_{T+1}|\text{no accident, employed at } T+1, w_{-1}(y', y, \hat{h}), h] \\
= E_T[w_{T+1}|\text{no accident}, w_{-1}(y', y, \hat{h}), h] &\leq E_T[w_{T+2}|\text{no accident}, w_{-1}(y', y, \hat{h}), h] \\
&\leq ... \leq E_T[w_{T+j}|\text{no accident}, w_{-1}(y', y, \hat{h}), h], \forall w_{-1}(y', y, \hat{h}), h. \quad (44)
\end{aligned}$$

Next, using the law of iterated expectations we get

$$E_0[w_{T+j}|\text{accident at } T] \leq E_0[w_{T+j}|\text{no accident}], \forall j. \quad (45)$$

□

Proof of Proposition 2. Just redo Proof of Proposition 1 with $\epsilon = 0$ and $\varepsilon = 0$. Allow for the possibility that each individual has an inherent fixed human capital level h.

Consider the case of an individual who was paid a wage $w_{-1}(y', y_{-1}, h)$ prior to the accident. If there is not human capital depreciation, even with downward wage rigidity, the individual's market wage coincides with the current wage paid. First, let's show that the employer productivity of the individual upon recovery is lower than it would have been otherwise. Then,

$$\begin{aligned}
E_T[y_{T+j}|\text{accident at } T, y_{-1}] = y_{-1} &< \psi \int_{y_{-1}}^{\infty} x dF(x) + \psi \int_0^{y_{-1}} y_{-1} dF(x) + (1-\psi) y_{-1} \\
&= E_T[y_{T+1}|\text{no accident}, y_{-1}] < E_T[y_{T+2}|\text{no accident}, y_{-1}] \\
&< ... < E_T[y_{T+j}|\text{no accident}, y_{-1}], \forall y_{-1}. \quad (46)
\end{aligned}$$

Hence, it follows that

$$E_T[y_{T+j}|\text{accident at } T] < E_T[y_{T+j}|\text{no accident}]. \quad (47)$$

Now using the law of iterated expectations

$$E_0[y_{T+j}|\text{accident at } T] < E_0[y_{T+j}|\text{no accident}]. \quad (48)$$

Next, let us consider wages.

$$E_T[w_{T+j}|\text{accident at }T, w_{-1}(y', y, h)] = w_{-1}(y', y, h) < \psi \int_{y}^{\infty} w(y, x, h) dF(x)$$

$$+ \psi \int_{y'}^{y} w(x, y, h) dF(x) + (1 - \psi F(y')) w_{-1}(y', y, h)$$

$$= E_T[w_{T+1}|\text{no accident, employed at }T+1, w_{-1}(y', y, h)]$$

$$= E_T[w_{T+1}|\text{no accident}, w_{-1}(y', y, h)] < E_T[w_{T+2}|\text{no accident}, w_{-1}(y', y, h)]$$

$$< ... < E_T[w_{T+j}|\text{no accident}, w_{-1}(y', y, h_j)], \forall w_{-1}(y', y, h). \quad (49)$$

Hence,

$$E_T[w_{T+j}|\text{accident at }T] < E_T[w_{T+j}|\text{no accident}] \quad (50)$$

Now using the law of iterated expectations

$$E_0[w_{T+j}|\text{accident at }T] < E_0[w_{T+1}|\text{no accident}] < E_0[w_{T+j}|\text{no accident}], \forall j. \quad (51)$$

□

F Model Value Functions

In this Section, we go over the value functions associated to each state in our model. Note that time is discrete and workers discount time according to parameter β. Workers and firms are risk neutral.

Denote by $W(y', y, h, w(y', y, \hat{h}))$ the value function for a worker with human capital h, employer y, outside option y', being paid a wage $w(y', y, \hat{h})$. Intuitively, workers might be paid a different wage than the one implied by their employer y, outside option y', and human capital h, due to downward rigidity in wages with a same employer. $W(y', y, h, w(y', y, \hat{h}))$ for when $h \neq \hat{h}$ represents the case in which the worker is paid a wage different than their *market wage*. Let $W_m(y', y, h)$ be the value function of a worker with human capital h, employer y and outside option y', being paid their *market wage*, $w(y', y, h)$. In other words, $W_m(y', y, h) = W(y', y, h, w(y', y, h))$. Now let us consider an individual working with human capital h, whose last time not working was when she was unemployed. Since unemployment her wage has been consistently increasing (via human capital accumulation, bargaining, and moving to better employers). As a result, working individuals whose last time not working was when unemployed have value function $W_m(y', y, h)$. With probability δ the individual becomes unemployed. With probability μ an individual suffers an accident. We assume that the probability of two events occurring simultaneously (*e.g.*, losing job and getting new wage offer, or, losing job and receiving accident, *etc.*) is zero. Let $S(y', y, h, w(y', y, \hat{h}))$ be the value function of an individual with human capital h recovering from an accident, who was working for an employer of productivity y with outside option y' being paid wage $w(y', y, \hat{h})$ when she suffered the accident. Finally, let $U(h)$ be the value of unemployment. Then,

$$W_m(y', y, h) = w(y', y, h) + \beta\psi \int_y^{\infty} W(y, x, h+\epsilon)dF(x) + \beta\psi \int_{y'}^{y} W(x, y, h+\epsilon)dF(x) + \\ + \beta\mu S(y', y, h, w(y', y, h)) + \beta\delta U(h) + \beta(1 - \psi(1 - F(y')) - \mu - \delta)W(y', y, h+\epsilon). \tag{52}$$

The particular case of individuals who were just hired from unemployed and did not accumulate any outside option yet is just captured by the case in which $y' = 0$.

The value function of the worker employed by y using outside option y' depends on her flow income from employment, $w(y', y, h)$, and her continuation value. Conditional, on not losing their job or suffering an accident, the individual's human capital increases to $h + \epsilon$. With probability ψ the individual gets a new wage offer. If the offer is from an employer of higher productivity than her current employer, $x > y$, she switches to this new employer, using her current employer, y, as outside option in the wage negotiation with the new employer. In that case, she gets value function $W(y, x, h + \epsilon)$. If the offer is from an employer of lower productivity than her current current employer but better than her current outside option, $y' \leq x \leq y$, she remains with her current employer but uses the new offer, x, to increase her wage to $w(x, y, h + \epsilon)$. In that case, she gets $W(x, y, h+\epsilon)$. Finally, with probability μ she suffers an accident, and with probability δ she moves to unemployment, entering these new states with her human capital h.

By similar arguments we have

$$U(h) = b + \beta\psi \int \max\{W(0, x, h), U(h - \varepsilon)\}dF(x) + \beta(1 - \psi)U(h - \varepsilon). \tag{53}$$

The value function of the unemployed, $U(h)$, depends on the flow income when unemployed (b) and the continuation value. The continuation value comes from the fact that at rate ψ the individual receives a job opportunity drawn from the employer productivity distribution $F(x)$. Upon receiving the offer the individual chooses the maximum between staying unemployed (in which case her human capital depreciates to $h - \varepsilon$), receiving $U(h-\varepsilon)$, or accepting the offer, receiving, $W(0, x, h)$. Finally, with probability $1-\psi$ she does not receive an offer and stays unemployed, receiving $U(h - \varepsilon)$.

Consistent with our empirical analysis, we consider accidents that push individuals into absence. While absent they are unable to work and are unable to search for new jobs, but stay attached to their employer. We model the individuals as keeping their job to be consistent with legislation in many countries that makes it hard to fire workers after they suffer an accident. During the recovery period, the worker is paid a fraction γ of her wage prior to the accident, $w(y', y, \hat{h})$, where $0 < \gamma \leq 1$. Consistent with our empirical findings that absent individuals receive the same wage as their *past self*, upon recovery the individual goes back to working with her employer at the wage she was paid prior to the accident, $w(y', y, \hat{h})$. As a result, the individual is paid more than she would be paid if she was paid according to her current human capital, *market wage*. Recall that $W(y', y, h, w(y', y, \hat{h}))$ is the value function of an individual working for firm y, with outside option y', human capital h, being paid wage $w(y', y, \hat{h})$. Then, the value function associated with recovering from an accident, being absent from work, is

$$\begin{aligned} & S(y', y, h, w(y', y, \hat{h})) = \\ & \qquad \gamma w(y', y, \hat{h}) + \beta p W(y', y, h, w(y', y, \hat{h})) + \beta(1-p) S(y', y, h-\varepsilon, w(y', y, \hat{h})) \end{aligned} \tag{54}$$

$S(y', y, h, w(y', y, \hat{h}))$ depends on the flow of income while in that state, $\gamma w(y', y, \hat{h})$, and on the continuation value. Note that $\gamma w(y', y, \hat{h})$ depends on the wage the individual received prior to the accident, $w(y', y, \hat{h})$, which is different than the wage they would get if there was no downward rigidity (the *market wage* associated to their human capital). The continuation value comes from the fact that with probability p the individual recovers and goes back to working, being paid the wage she had prior to the accident, $w(y', y, \hat{h})$, receiving value function $W(y', y, h, w(y', y, \hat{h}))$, and with probability $1-p$ she stays absent from work in which case her human capital decreases to $h - \varepsilon$.

The last remaining value function is $W(y', y, h, w(y', y, \hat{h}))$ for $h \neq \hat{h}$, defined by

$$\begin{aligned} W(y', y, h, w(y', y, \hat{h})) &= w(y', y, \hat{h}) \\ &+ \beta\psi \int_y^\infty \max\{W(y', y, h+\epsilon, w(y', y, \hat{h})), W_m(y, x, h+\epsilon)\} dF(x) \\ &+ \beta\psi \int_{y'}^y \max\{W(y', y, h+\epsilon, w(y', y, \hat{h})), W_m(x, y, h+\epsilon)\} dF(x) \\ +\beta\mu S(y', y, h, w(y', y, \hat{h})) &+ \beta\delta U(h) + \beta(1-\psi(1-F(y')) - \mu - \delta) W(y', y, h+\epsilon, w(y', y, \hat{h})). \end{aligned} \tag{55}$$

The value function $W(y', y, h, w(y', y, \hat{h}))$ of the worker depends on her flow income from employment, her *current wage paid*, $w(y', y, \hat{h})$, and her continuation value. With probability ψ the individual gets a new wage offer. If the offer is from an employer of higher productivity than her current employer, $x > y$, she chooses the maximum between what she gets by staying at her current employer, $W(y', y, h+\epsilon, w(y', y, \hat{h}))$, and what she gets if she moves to the competing employer, $W_m(y, x, h+\epsilon)$, where she will be paid her *market wage*, $w(y, x, h+\epsilon)$, associated to using her current employer y as outside option. If the offer is from an employer of lower productivity than her current current employer but better than her current outside option, $y' \leq x \leq y$, she chooses between the maximum between what she gets by staying with her current employer $W(y', y, h+\epsilon, w(y', y, \hat{h}))$, and what she gets if she renegotiates her wage with her current employer. If she renegotiates her wage with her current employer she is paid her *market wage*, $w(x, y, h+\epsilon)$ associated with the current employer, y, based on this new outside option, x, and so, gets $W_m(x, y, h+\epsilon)$. Finally, with probability μ she suffers an accident, and with probability δ she moves to unemployment.