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IZA DP No. 16312

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Márta Bisztray
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Anikó Bíró
Centre for Economic and Regional Studies

Márta Bisztray
Centre for Economic and Regional Studies

João G. da Fonseca
Université de Montréal

Tímea Laura Molnár
Central European University and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Accident-Induced Absence from Work and Wage Ladders*

How do temporary spells of absence from work affect individuals' labor trajectory? To answer this question, we augment a 'wage ladder' model, in which individuals receive alternative take-it-or-leave-it wage offers from firms and potentially suffer accidents which may push them into temporary absence from work. In such an environment, during absence, individuals do not have the opportunity to receive alternative wage offers that they would have received had they remained present. To test our model's predictions and to quantify the importance of foregone opportunities to climb the wage ladder, we use linked employer-employee administrative data from Hungary, that is linked to rich individual-level administrative health records. We use unexpected and mild accidents with arguably no permanent labor productivity losses, as exogenous drivers of short periods of absence. Difference-in-Differences results show that, relative to counterfactual outcomes in the case no accidents, (i) even short (3-12-months long) periods of absence due to accidents decrease individuals' wages for up to two years, by around 2.5 percent; and that (ii) individuals reallocate to lower-paying employers. The share of wage loss due to missed opportunities to switch employers is between 7-20 percent over a two-year period after returning to work, whereas at most 2 percent is due to occupation switches. Our results are robust to (a) instrumenting absence with having suffered an accident, (b) exploiting the random nature of the time of the accident, and (c) within-firm matching of individuals with and without an accident and subsequent absence spell.

JEL Classification: J22, J23, I10

Keywords: wage ladder, accidents, health shocks, temporary absence from work

Corresponding author:
Tímea Laura Molnár
Central European University
Quellenstraße 51
A-1100 Wien
Austria

E-mail: MolnarTL@ceu.edu

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1 Introduction

If indeed 80 percent of success is showing up,[1] then even temporary absence from work could have persistent detrimental effects on labor outcomes. Absence from work is not uncommon, yet it is difficult to identify its consequences on labor outcomes due to the endogenous nature of most absences (*e.g.*, quitting or being laid-off, parental leave, caregiving, *etc.*). In this paper, we quantify to what extent short periods of involuntary absence from work, induced by an exogenous shock (using unexpected, temporary and mild accidents), 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, competing, wage offers either from their current employer or competing employers. New internal wage offers lead to wage growth by allowing individuals to move to better-paying occupations at the same employer, while new external wage offers allow them to move to higher-paying employers and/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).[2] In the absence of shocks that prevent individuals from being present at work, they *climb the ladder*, and at each new position with a higher wage, they are more selective on the set of wage offers they accept.[3]

We formalize and estimate the importance of an overlooked mechanism related to wage ladders: *forgone opportunities to climb the wage ladder* during temporary absence from work, by missing out on wage offers. We formalize that involuntary periods of absence, even if temporary with no persistent labor productivity losses for individuals, force them to forgo wage offers that would have led to *faster wage growth* for them, *via* within-firm wage growth or switching to higher-paying firms. We estimate the resulting wage loss stemming from workers reallocating to lower-productivity, lower-paying employers and to lower-paying occupations.

On the theory side, we consider a wage ladder framework with accidents and temporary absence from work. In our model, individuals receive alternative wage offers from firms, use competing offers to renegotiate a higher wage, and potentially suffer accidents which may result in them being absent from work. In such an environment, wages grow *via* competing offers, allowing workers to either switch to higher-paying employers or to renegotiate a higher wage with their current employer. Upon suffering an accident, and while being temporarily absent, workers no longer receive competing wage offers. 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 theoretical framework buttresses the importance of distinguishing between the *past self* and the *counterfactual self*. Consistent with the notion that it is hard for firms to fire individuals after an accident, we consider individuals who are entitled to keep their jobs, who then, after recovery, are expected to either keep their positions on the wage ladder, or to move up. Our framework predicts that, upon return from absence, individuals have an equal or higher wage than the wage of their *past self*. At the same time, they are expected to have a lower wage relative to what they would have had otherwise in the case of no accident—*i.e.*, relative to the wage of their *counterfactual self*—due to the *forgone opportunities to climb the wage ladder* mechanism. Importantly, the aforementioned predictions differ from predictions that would arise through losses in productivity: if absence led to wage decreases *via* human capital depreciation or other forms of loss

[1] The quote is attributed to Woody Allen ("On Language; The Elision Fields," by William Safire, The New York Times, August 13, 1989; *available at* https://www.nytimes.com/1989/08/13/magazine/on-language-the-elision-fields.html).

[2] 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.

[3] *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.

in productivity, then, *upon return to work*, wages of individuals are predicted to be smaller than the wages of both their *counterfactual self* and their *past self.*

To empirically test the above strong predictions of the wage ladder framework, 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 (3–12-months long) absence. We define absence as not being attached to a firm, or not working for a valid pay, or being on sickness benefit. We choose unexpected and mild accidents with the possibility of full recovery and no permanent labor productivity losses, to avoid the contaminating effect of worse health or lower human capital on individual labor productivity. Importantly, 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 provides detailed information on labor market outcomes of a random 50 percent sample of the entire population of Hungary. 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 empirical aims are twofold. First, we test whether a short (3–12-months long) period of absence, due to a temporary, unexpected and mild accident, leads to lower wages for treated individuals, relative to control individuals (who have not suffered an accident). Second, we test whether it leads to lower wages for the treated, relative to their own pre-absence wages. In our Difference-in-Differences empirical strategy, we explicitly control for any level differences between treated and control individuals *prior* to the accident.

Our main results are: (i) even short (3–12-months long) periods of absence due to a temporary, 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 5 percent upon return to work and 2.5 percent even 1–2 years after; (ii) relative to the case of no accidents, *treated* workers reallocate to lower-paying employers (captured by the estimated firm-specific wage premium, following Abowd et al., 1999); (iii) treated workers' wage upon return to work is not significantly different (at the 5 percent level) from their *past self*'s pre-accident wage.

The observed wage losses of 2.5–5 percent relative to workers' *counterfactual self* are inconsistent with human capital depreciation and major productivity losses while being absent, given that treated workers' wages *upon return to work* are not smaller than their *past self*'s pre-accident wage. Instead, our main finding that workers' wages are smaller only relative to their *counterfactual self*'s but not relative to their *past self*'s wage, signals the importance of *forgone opportunities to climb the wage ladder* while being absent.

We present four further pieces of evidence indicating that our chosen accidents do not lead to persistent labor productivity losses. First, while there is an indication of worse health around the time of the accident—in terms of 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 a year. Second, there is no indication that individuals would work in less stressful and physically less demanding occupations in response to the 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 year and 2 years after returning to work are driven by young and white-collar workers, whose productivity is less likely to be affected by minor accidents than the productivity of 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, 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 trajectories of almost identical individuals who suffered the accident at least three years apart in time; then, individuals who have not yet suffered the accident, can serve as controls for those who have already suffered it. In (c), we compare the labor market 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.

As a falsification test, we show the estimates for workers who suffer an accident but have no subsequent absence spell. For them, our model predicts wage losses at most for the duration of recovery as the accident does not alter how selective they are on wage offers, and it predicts no impact on which (higher- or lower-paying) firms individuals work for in any of the periods. Our estimates confirm these predictions.

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 employers, and into a part that is due to relatively lower wages with the same employer. We find that upon return 7.36 percent of the wage drop for the treated stems from missed opportunities to switch to higher-paying employers, and this share increases to 20.14 percent two years after. Similarly, we decompose the wage loss for the treated into a part that stems from missed opportunities to switch to better-paying occupations, and into a part that is due to relatively lower wages within the same occupation. We find that upon return only 2.19 percent of the wage loss for the treated stems from missed opportunities to switch to better-paying occupations, and this share vanishes thereafter.

Our paper contributes to several strands of literature. First, job (or wage) ladders have been shown to be important for our understanding of important issues such as 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 tested the prediction on *forgone opportunities to climb the wage ladder*, arising from temporary absence from work without job loss. To the best of our knowledge, we are the first to use well-identified individual-level shocks to test 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 test the mechanism of *forgone opportunities to climb the wage ladder*, which we do, using accident-induced absence. A second generation of papers 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., 2022). 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 to climb the wage ladder*. 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 by decomposing how much the wage loss of the treated stems from missed opportunities to move to higher-paying firms *versus* missed opportunities to climb the wage ladder within the firm in other contexts and subsamples. 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 (worse employers). Although in the context of a different source of wage loss and decomposition exercise, their results are close to our finding that 20 percent of wage loss due to absence comes from relatively worse employers. Helm et al. (2022) focus on the manufacturing sector, where establishment premiums are higher, and document that half of the wage loss upon displacement is attributed to lower paying establishments. At the same time, they find that hardly any wage loss due to displacement comes from lower establishment premiums in the service sector.[4]

Third, our paper also relates to the growing literature studying the effects of career leave. By considering an environment where we see wages being higher or equal to what they were previously but lower than what they would have been, we distinguish our mechanism of interest—the *foregone opportunities to climb the wage ladder*—from the wage drop due to human capital depreciation (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.[5] But, while Johnsen et al. (2020) exploit exogenous paternal leave policy variation in worker's ranking within a contest, not *via* his own leave status but that of his competitors, we focus on the effect of workers' own absence induced by mild accidents, on wage trajectories and reallocation.

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 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). If we look at results from the US (with a typically short period (12 weeks) of unpaid maternity leave), the literature 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 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 an unexpected absence driven by an accident without long-term health consequences, which is different from the impacts 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

[4] 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.

[5] 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.

health shocks on employment and earnings, although the estimates vary, partly due to the different types of shocks analyzed. We contribute to this literature by using specific temporary unanticipated health shocks (accidents), instead of more drastic health shocks (such as the arrival of chronic conditions, a heart attack, or a stroke) to test the predictions of a wage ladder framework, with no long-term productivity losses.

In what follows, Section 2 describes the theoretical framework and the implied testable predictions. Section 3 describes the relevant institutional features of the Hungarian labor market, focusing on sickness benefits. Section 4 describes data and measurement. Section 5 provides motivating descriptive evidence, foreshadowing that mild accidents inducing short absence from work have permanent wage effects. Section 6 describes our empirical strategies, and Section 7 presents our estimates. To shed light on the underlying mechanisms, Section 8 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 9 discusses the main results, and Section 10 concludes.

2 Theoretical Framework

In this section, we provide a theoretical framework to study the wage effects of accident-induced absences from work. Importantly, this framework will also deliver us testable predictions to validate the mechanisms at play. For the sake of tractability, we consider a framework of identical workers (and then, in our empirical analysis, we control for a rich set of individual- and firm-level characteristics).

Consider the problem of an individual receiving alternative wage offers. Individuals can either be working (being attached to a firm with an employment contract, and supplying working hours for a given hourly wage), absent (temporarily not supplying working hours), or unemployed. Individuals are endowed with ϑ efficiency units, and there is no heterogeneity across workers in ϑ. Firms bargain with workers, both at the beginning of the employment relationship and when the worker receives a credible outside offer (Cahuc et al., 2006).[6] There is no cost to renegotiation, and renegotiation only occurs by mutual consent. Time is continuous and workers discount time according to parameter r. Workers and firms are risk neutral.

With probability ψ an individual receives a new wage offer. Employers differ by productivity $y \sim F(y)$, where $\infty > y \geq 0$. Define $w^u(y)$ as the wage an unemployed individual receives from a firm of productivity y. Assume $w^u(y) = h(y) < y$. Let $G(w)$ represent the wage offer distribution for unemployed workers, then, $G(h(y)) = F(y)$. With probability ψ the employed individual receives a new wage offer. Upon receiving a new wage offer, the current and potential employer of the worker compete for the worker. If the productivity y of the current employer is higher than that of the competing employer y', then, the worker stays with the current employer but uses the maximum she could have obtained from the competing employer, y', as outside option. If the previous outside option the worker had (from a previous competing offer) is smaller than y', the worker's wage increases, otherwise the wage is unchanged. If, instead, the competing employer productivity y' is higher than the current employer productivity y, the worker switches employer using the maximum she could have obtained from her employer, y, as outside option when negotiating with the new employer. In particular, a worker using y' as outside option when bargaining with an employer of productivity y receives wage $w(y', y)$ defined by[7]

$$w(y', y) = g(y', y), \text{with} \quad \frac{\partial g(y', y)}{\partial y'} \geq 0 \quad \text{and} \quad \frac{\partial g(y', y)}{\partial y} \geq 0. \tag{1}$$

[6] The model we consider here is a simplified version of Cahuc et al. (2006).

[7] See Cahuc et al. (2006) for a microfoundation of the properties of g.

The particular case in which workers have no bargaining power upon renegotiation would correspond to the above wage setting with the modified property $\frac{\partial g(y',y)}{\partial y} < 0$ (see Postel-Vinay and Robin, 2002)[8], and the particular case in which firms commit to the initial wages posted and never renegotiate is captured by $g(y', y) = w^u(y)$, as in Burdett and Mortensen (1998). Note that a person that has just got a job from unemployment and has not received any competing offers yet, has $y' = 0$. And so, they get $g(0, y)$ which must be equal to $w^u = h(y)$. Hence, for consistency, and to simplify notation, we define g such that $g(0, y) = h(y)$.

With probability δ the individual becomes unemployed. Once unemployed, the individual receives income b and receives a wage offer from $G(w)$ with probability ψ. With probability μ an individual suffers an accident. Let $S(y', y)$ be the value function of an individual following an accident who was working for an employer of productivity y with outside option y' when she suffered the accident. For those individuals who just got a job from unemployment and did not have any outside offers when they suffered the accident this corresponds to $S(0, y)$. Denote with $W^u(y)$ the value function of a worker with wage $w^u = h(y)$ who prior to the current employer was unemployed and did not yet receive a competing offer since working for y. Denote with $W(y', y)$ the value function of a worker with an outside option of y' and currently employed by a firm with productivity y. Finally, let U be the value of unemployment. Then,

$$\begin{aligned} rW^u(y) = \vartheta w^u(y) + \psi \int_y^\infty [W(y,x) - W^u(y)]dF(x) \\ + \psi \int_0^y [W(x,y) - W^u(y)]dF(x) + \delta(U - W^u(y)) + \mu(S(0,y) - W^u(y)). \end{aligned} \tag{2}$$

The value function for a worker employed by y since unemployment who has not received a competing offer yet, $W^u(y)$, depends on her flow income from employment, $\vartheta w^u(y)$, plus her continuation value. Her continuation value comes from the fact that with probability ψ workers get 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 negotiation with the new employer, x. As a result, she gets the value function of being employed by x using outside option y, $W(y, x)$, and loses what she currently has, $W^u(y)$. If the offer is from an employer of lower productivity than her current employer, $x \leq y$, she remains with her current employer but uses the competing offer from x to increase her wage with current employer to $w(x, y)$. Then, she gets the value of being employed by y using outside option x, $W(x, y)$, and loses what she currently has, $W^u(y)$. With probability δ she loses her job and moves to unemployment, getting U and losing $W^u(y)$. Finally, with probability μ she suffers an accident, getting $S(0, y)$ and losing $W^u(y)$.

Similar to the above, $W(y', y)$ is given by

$$\begin{aligned} rW(y',y) = \vartheta w(y',y) + \psi \int_y^\infty [W(y,x) - W(y',y)]dF(x) \\ + \psi \int_{y'}^y [W(x,y) - W(y',y)]dF(x) + \delta(U - W(y',y)) + \mu(S(y',y) - W(y',y)). \end{aligned} \tag{3}$$

The value function of the worker employed by y using outside option y' depends on her flow income from

[8] If workers have no bargaining power, high productivity employers are able to pay their workers less due to their workers' expectation of higher wages in the future. This corresponds to, all else equal, a wage decreasing in current employer productivity.

employment, $\vartheta w(y', y)$, 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 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 $W(y, x)$ and loses what she currently has $W(y', y)$. 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 $w(x, y)$. In that case, she gets $W(x, y)$ and loses what she currently has $W(y', y)$. Finally, with probability δ she moves to unemployment and with probability μ she suffers an accident.

By similar arguments we have

$$rU = b + \psi \int_0^\infty (\max\{W^u(x), U\} - U) dF(x). \tag{4}$$

The value function of the unemployed, U, depends on the flow of income from being 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 $F(x)$. Note that $W^u(x)$ is increasing in x and U is not. As a result, the optimal behavior of the unemployed is described by a threshold strategy. Let $\underline{y}$ be the lowest employer productivity the unemployed is willing to accept. Then,

$$\psi \int (\max\{W^u(x), U\} - U) dF(x) = \psi \int_0^{\underline{y}} (U - U) dF(x) + \int_{\underline{y}}^\infty (W^u(x) - U) dF(x) = \int_{\underline{y}}^\infty (W^u(x) - U) dF(x) \tag{5}$$

and so the expression for rU becomes

$$rU = b + \psi \int_{\underline{y}}^\infty (W^u(x) - U) dF(x). \tag{6}$$

Finally, define $\underline{w} = h(\underline{y})$.

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 $t = 0$. Next, define $E_0[w_j|\text{no accident}]$ as the expectation in $t = 0$ for wages j periods in the future, under the absence of accidents and unemployment in these j periods. Then,

$$E_0[w_1|\text{no accident}] = \int_{\underline{w}}^\infty w' dG(w') \tag{7}$$

Now, let us consider the wage at time $t = j$, w_j, the individual expects at $t = 0$, conditional on not receiving an accident from period 0 to j as $E_0[w_j|\text{no accident}]$. This brings us to Proposition 1 below[9]

Proposition 1

$$E_0[w_j|\text{no accident}] > E_0[w_{j-1}|\text{no accident}], \forall w_{j-1}. \tag{8}$$

It follows that, conditional on not receiving a health shock or slipping into the state of unemployment (U), wages are expected to grow *via* accumulation of wage offers (switching to a new employer or wage renegotiation with current employer). Proposition 1 does not depend on the value of being sick $S(y', y)$.

Next, let us consider two separate scenarios. In the first, we consider accidents that force the individual to be absent from work. Importantly, even if the individual is forced to be absent, she keeps her job and attachment to her current employer. In the second scenario, we consider accidents after which the individual

[9] Proofs of propositions can be found in Appendix A.

is able to stay present at work, but with a temporary decrease in efficiency units. Comparing these two scenarios will allow us to understand the difference between those cases, in which an accident does or does not lead to forgone opportunities to climb the wage ladder.

2.1 First Case: Accidents Inducing Periods of Absence

In this first case, we consider an accident that is strong enough to push the individuals into absence, in which they are unable to work temporarily and are unable to search for new jobs, but stay attached to their employer. We model the individuals as keeping their job, consistently 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 $w^s(y)$, where $w^s(y) \leq g(y', y), \forall y'$. Then, the value function associated with having had an accident, $S(y', y)$, is given by

$$rS(y',y) = w^s(y) + p\psi\Bigg(\int_y^\infty [W(y,x) - S(y',y)]dF(x) + \int_{y'}^{y} [W(x,y) - S(y',y)]dF(x) + \int_0^{y'} [W(y',y) - S(y',y)]dF(x) \Bigg) + p(1-\psi)(W(y',y) - S(y',y)). \quad (9)$$

The value function of having just had an accident, $S(y', y)$, depends on the flow of income while in that state, $w^s(y)$ and on the continuation value. The continuation value comes from the fact that with probability $p\psi$ the individual recovers and receives a new job opportunity and with probability $p(1-\psi)$ the individual recovers but does not receive a new job opportunity. If the individual receives an offer from a higher productivity employer $x > y$, she switches to this new employer and uses her current employer, y, as outside option when bargaining with the new employer. In this case, she gets $W(y, x)$ and loses $S(y', y)$. If the individual receives an offer from an employer less productive than her current one but more than her outside option, $y' < x < y$, she uses that offer as a new outside option. In that case she gets $W(x, y)$ and loses $S(y', y)$. If she recovers but gets an offer that is worse than her current outside option $x < y'$, she remains with her current employer and her wage reverts to its pre-accident value. In that case, she gets $W(y', y)$ and loses $S(y', y)$. Finally if she recovers but gets no offers she gets $W(y', y)$ and loses $S(y', y)$.

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 a health shock 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 a health shock between T and $T + j$. Then,

Proposition 2

$$E_0[y_{T+j}|\text{accident at } T] < E_0[y_{T+j}|\text{no accident}], \forall j. \quad (10)$$

and

$$E_0[w_{T+j}|\text{accident at } T] < E_0[w_{T+j}|\text{no accident}], \forall j. \quad (11)$$

Proposition 2 states that after an accident, the individual wage and employer productivity are both smaller than they would have been, in absence of the shock. Next, Proposition 3 states that the individual is not receiving less than she received before the shock.

Proposition 3

$$E_0[w_{T+j}|\text{accident at } T, w_{T-1} = \omega] \geq \omega = w_{T-1}. \tag{12}$$

Intuitively, after receiving the shock the individual goes back to her old job, avoiding any wage loss, relative to her *past self*. However, the wage she has is less than she would have had in the absence of the shock (*counterfactual self*). The reason for this difference is that the presence of the wage ladder implies growth in wages, conditional on no job loss (via job switching and wage renegotiation). As a result, simply not being able to search for jobs for temporary periods has repercussions for the worker.

Next, note that any decrease in wage leads to future decreases in wages by making individuals less selective on future offers than they would have been otherwise. It follows that

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

and

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

Hence, an accident without long-term health consequences has a permanent negative effect on wages only relative to the individual's *counterfactual self* and not relative to the individual's *past self*. This is an important point that allows us to distinguish these predictions from predictions arising from a human capital accumulation channel or losses in productivity more generally. In particular, if absence led to wage decreases via human capital depreciation or other forms of loss in productivity then the wage of the worker should be smaller relative to both her *counterfactual self* and her *past self upon return to work*.

We conclude that accidents without long-term health consequences which induce periods of absence lead to permanent falls in wages and employer productivity relative to the counterfactual of no accident but not relative to past values.

2.2 Second Case: Accidents Not Inducing Periods of Absence

We now consider the case in which the accident does not induce periods of absence. In particular, consider that upon suffering an accident the efficiency units of the individual falls from ϑ to $\vartheta' > 0$. Importantly, consider that since the individual is still able to work she is also still able to search for new jobs.[10] Finally, consider that at rate p the individual recovers. Then, the value functions for workers that had no previous competing offer are given by

$$\begin{aligned} rW^u(y,\vartheta) = h(y)\vartheta + \psi \int_y^\infty [W(y,x,\vartheta) - W^u(y,\vartheta)]dF(x) \\ + \psi \int_0^y [W(x,y,\vartheta) - W^u(y,\vartheta)]dF(x) + \delta(U - W^u(y,\vartheta)) + \mu(W^u(y,\vartheta') - W^u(y,\vartheta)), \end{aligned} \tag{15}$$

[10] Here we consider accidents do not alter the probability of receiving job offers while in recovery. Had that been the case, even accidents not leading to absence would have long run effects on wages and reallocation across employers, opposite to what we find empirically (see Section 7.5.4).

$$rW^u(y,\vartheta') = h(y)\vartheta' + \psi\int_y^{\infty}[W(y,x,\vartheta') - W^u(y,\vartheta')]dF(x)$$
$$+\psi\int_0^y[W(x,y,\vartheta') - W^u(y,\vartheta')]dF(x) + \delta(U - W^u(y,\vartheta')) + p(W^u(y,\vartheta) - W^u(y,\vartheta')). \quad (16)$$

Similarly, $W(y',y,\vartheta)$ and $W(y',y,\vartheta')$ are defined by

$$rW(y',y,\vartheta) = w(y',y)\vartheta + \psi\int_y^{\infty}[W(y,x,\vartheta) - W(y',y,\vartheta)]dF(x)$$
$$+\psi\int_{y'}^y[W(x,y,\vartheta) - W(y',y,\vartheta)]dF(x) + \delta(U - W(y',y,\vartheta)) + \mu(W(y',y,\vartheta') - W(y',y,\vartheta)), \quad (17)$$

and

$$rW(y',y,\vartheta') = w(y',y)\vartheta' + \psi\int_y^{\infty}[W(y,x,\vartheta') - W(y',y,\vartheta')]dF(x)$$
$$+\psi\int_{y'}^y[W(x,y,\vartheta') - W(y',y,\vartheta')]dF(x) + \delta(U - W(y',y,\vartheta')) + \mu(W(y',y,\vartheta) - W(y',y,\vartheta')). \quad (18)$$

We see that regardless of whether the individual has efficiency units ϑ and ϑ', to switch jobs the individual still requires to be paid at least the same wage per efficiency unit and can still search for competing wage offers. In other words, the individual does not change how selective she is on wage offers when ϑ decreases to ϑ'. As a result, in this case the accident affects wages only for the duration of the shock and does not impact the employer productivity the worker is expected to work for.

2.3 Summary

Our model predicts that accidents without long-term health consequences have a permanent effect on an individual's expected wage path and employer productivity if the shock induces periods of absence. If, on the other hand, the accident does not induce periods of absence, worker income decreases only for the duration of the shock and expected employer productivity is unaffected for all periods. We test both sets of predictions.

3 Institutional Background

Act LXXXIII of 1997 of the Hungarian Labor Code defines "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 the current employer later than the 1^{st} of January of that year.

After 15 days those who are still not able to work can receive sickness benefit. This benefit is paid by the state and is conditional on the individual having social insurance and paying her social security contribution. A person having an accident at the workplace is immediately entitled to sickness benefit. The maximum length is 1 year and the person can not be laid off during this period, 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 received corresponds to 50-60 percent of the salary, on average, and 100 percent in the case of an accident at the workplace.[11]

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 is also low in Hungary, it was around 10 percent in the analyzed period, 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, we note that a person with a permanent health deterioration resulting in limited or no ability to work is entitled to a disability benefit, which is 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.

4 Data, Sample Selection, and Measurement

4.1 Data Sources

Our main dataset 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 labor outcomes, also contains administrative data on medical records and drug expenditures, for years 2009–2017.[12]

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 counted in the length of service as a base for pension. Information on transfers is 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 stay and outpatient care with monthly number of healthcare days and disease codes, number of primary care visits, spending on prescribed medication by ATC codes) is available from 2009, originating from the *Hungarian National Health Insurance Fund Administration*.[13]

For each individual we observe gender, age, amount of unemployment benefits, amount of child-related and pension benefits, number of days on sickness benefit, education (for a subsample), number of days

[11] As a comparison, employees in Austria are entitled to 100 percent of their wage if they are on sick leave up to six to twelve 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%, from the fourth day on 55 percent of the reference wage is paid by the employer as a compensation, then the employee is entitled to a sickness benefit up to at most 52 weeks (Social Insurance Agency webpage). In the US, there are no federal regulations for a sick leave payment. Employees of those companies which fall under the Family and Medical Leave Act can have an unpaid sick leave for at most 12 weeks, conditional on fulfilling certain requirements (US Department of Labor).

[12] The administrative database used in this paper 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.

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

in hospital by month broken down by the cause of hospitalization (accidents, heart attacks, cancer, *etc.*), number of prescriptions by month, and monthly amount spent by drug code (antibiotics, antidepressants, respiratory tract drugs, *etc.*). To the extent the individual works in that month, we also observe monthly wage income, monthly hours worked, occupation, industry, and firm identifier.

On the firms' side, the data includes financial data, employment, industry, and foreign ownership data 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 and foremost, we observe individuals even for periods they are not attached to any employer—this is in large contrast to most employer-employee datasets in which workers can only be observed if they are employed. Observing such spells of absence or non-employment is crucial for any empirical test of the implications of the wage ladder framework. 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 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, in order to characterize occupations, we link to our data the publicly available Occupation Information Network Database (O*NET), describing the task content of occupations.[14]

4.2 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 3–12 months—absent from work, following an accident, with the counterfactual state of *no accident*.

First, we select the *treated* sample. The *treated* are defined as individuals who suffer the accidents listed below, are *employed* in the month of the accident or in the month prior, and then are *absent from work* for at least 3 and at most 12 months following the accident (but, they do return to work within a year, thus we exclude long-term unemployed from the analysis.)[15] We define *employed* as being attached to a firm with an employment contract,[16] having valid data on wage and working hours, and spending at most 5 days on sickness benefit in a given month.[17] We define being *absent from work* as not being employed.

For our definition of accidents, we choose the following unexpected, temporary, and mild accidents:[18] superficial injuries, open wounds, fractures, dislocations, sprains of joints and ligaments, injuries of nerves, injuries of muscle and tendon of selected body parts (ankle, foot, knee, lower leg, hip, thigh, wrist, hand, elbow, forearm, shoulder and upper arm), as well as dislocations, sprains of joints and ligaments of the head, the neck, the thorax and the upper lumbar spine. We exclude drastic fractures (*e.g.*, 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 consider only those people in the accident sample who have only one accident which might be

[14] For more information on this free data source, *see* `https://www.onetcenter.org/database.html`.

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

[16] 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. We only consider those with an employment contract as potentially being employed.

[17] 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. Note also that self-employment is not included in our employment definition.

[18] The ICD-10 "S" codes classify injuries related to single body parts.

a 'clustered' event as defined below.[19] Hospitalization events connected to accidents frequently occur in multiple consecutive months,[20] and because it is likely that these occurrences are the consequence of the same accident, we handle a sequence of at most four consecutive months with multiple accidents as a single 'clustered' event. This means, we take the last month before the first accident as the last month before the event, and the first month after the last clustered accident as the first month after the event.[21]

Second, we select the *control* sample as individuals who have never suffered any of the above-listed accidents, and are employed in the month of the event. For them, we choose both the time of the "event" and the time of the "return" after the event randomly, where their return also happens 3–12 months after.

In sum, our *main sample* includes (i) those with an accident and a short-term (3–12-months-long) period of absence thereafter, and (ii) those with no accident. In our Instrumental Variable strategy, described in Section 6.1.2 below, we use (i), (ii), as well as (iii) those with an accident but no short-term period of absence thereafter—we call this sample *IV sample*. In a robustness check described in Section 6.2 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 testing our model's predictions for those with an accident but no short-term period of absence thereafter, we use (ii) and (iii)—we call this *falsification sample*.

4.3 Measurement of the Outcome Variables

4.3.1 Hourly Deflated Log-Wage

We measure (the logarithm of) deflated hourly wage, for a given month, as:
$ln(w_{hour}) = ln\left(\frac{\text{wage income}_m}{\frac{\text{days in month}}{7}\text{working hours}_w}/\text{defl}_y\right)$, 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 being 2003). We censorize 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.

4.3.2 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, 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 equation for worker i at firm j at time t:

$$w_{ijt} = X'_{ijt}\beta + \vartheta_i + \phi_j + \varepsilon_{ijt}, \tag{19}$$

where w_{ijt} is the logarithmic wage of worker i at firm j at time t, X_{ijt} is a vector of time-varying observable characteristics with a $k \times 1$ vector of returns β, ϑ_i is time-invariant worker ability (or worker type), ϕ_j is the

[19] People with only one accident correspond to 83.5 percent of all the people ever having an accident.

[20] 67 percent of the people having an accident in our data have no other accident-related events. 12.8 percent have an additional accident-related ICD-10 code for in-patient care in the next month and additional 3.7 percent have further events—with or without gaps—within the next three months.

[21] As a result, event-month -1 and event-month +1 are 2-months apart for a simple non-clustered event, and these are 2-4 months apart for a clustered event.

time-invariant firm-specific wage premium, and ε_{ijt} is the time-varying error term.[22]

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/reallocation between firms is observable. This feature of the data is needed for identifying the time-invariant vectors of ϑ and ϕ in (19).

To get an estimate for time-invariant worker ability ϑ_i and firm-specific wage premium ϕ_j, we estimate the model in (19) 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, as well as a full set of year dummies, in X (but no firm-level variables).[23] After the estimation of the model in (19), we take the estimated coefficient vectors $\hat{\vartheta}$ and $\hat{\phi}$, and, in what follows, we call them "estimated AKM individual (fixed) effect" and "estimated AKM firm (fixed) effect", respectively.

4.3.3 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 (firm quality).

When doing so, we use the *prodest* STATA code of Rovigatti and Mollisi (2020) and apply the estimation procedure of Wooldridge (2009): we regress the logarithm of value added (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),[24] 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).

4.3.4 Measures of Occupation Characteristics (Stressful and Phisically Demanding Nature)

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[25] 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*.[26] 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 second variable. Both variables are standardized indices of O*NET task content values. Higher values indicate more stressful and more physically demanding occupations. We standardize the O*NET scores to have mean 0 and standard deviation of 1 in the relevant estimation samples.

[22] 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.

[23] 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.

[24] Ideally we would measure capital stock using tangible (and intangible) assets, but we do not observe these in the data.

[25] The Standard Occupational Classification (SOC) system is a system of classifying occupations used in the US.

[26] 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).

4.3.5 Estimated AKM Occupation Effect as a Measure of Occupation-Specific Wage Premium

As part of our analysis, we investigate how much of the estimated wage effects originate from transitions across occupations. To perform this exercise, we estimate occupation fixed effects of wages. We use two-digit occupation (ISCO) codes and estimate two alternative variants of equation (19): 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:

$$w_{ijot} = X'_{ijot}\beta + \vartheta_i + z_o + \varepsilon_{ijot}, \tag{20a}$$

$$w_{ijot} = X'_{ijot}\tilde{\beta} + \tilde{\vartheta}_i + \tilde{\phi}_j + \tilde{z}_o + \tilde{\varepsilon}_{ijot}, \tag{20b}$$

where 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 with a $k \times 1$ vector of returns β ($\tilde{\beta}$), 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 (20a), z_o captures the expected wage of a given occupation o (conditional on X_{ijot} and ϑ_i). In contrast, in (20b), $\tilde{z}_o$ 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 (20a) requires that there are switches/reallocation of individuals across occupations. The identification of $\tilde{\phi}_j$ and $\tilde{z}_o$ in (20b) requires switches/reallocation of individuals across firms, and across occupations within the same firm, respectively.

5 Descriptive Analysis

5.1 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 3-year-long window around the event (only partialling out individual fixed effects).

In Figure 1 we take all observations of treated individuals (*i.e.*, those suffering an accident, being employed at the time of the accident and being absent for 3–12 months thereafter) and control individuals (*i.e.*, those being employed in a given month without an accident) in a 3-year-long window around the event. Then we take the residual of deflated hourly log-wage on individual fixed effects and plot the average of those residuals, separately for treated and control individuals, for m=-36,...,0,...,36. $m = 0$ is the time of the event, and the average is normalized to 0 at $m = -12$. To ensure that the wage patterns of treated individuals are not driven by unobserved periods of sick leave (at most 15 days per year), we restrict the wage data of the treatment group in the first 12 months after the shock (when sickness absence is prevalent) to months at or after the first month of sickness benefit in the given calendar year. By that time the (unobserved) sick leave period is likely to be depleted because the payment received during sick leave is higher than the amount of sickness benefit. Thus, while sick leave is still available it should always be preferred to sickness benefit.

Note that in 1, $m = 0, 1, 2$ are not shown for the treated, as they are absent from work in those months. Then, the first red dot after the event (after $m = 0$) represents the average residual wage for individuals who have returned by the 3^{rd} month after the accident (*i.e.*, their wage upon return); similarly, the second red dot represents the average residual wage for individuals who have returned by the 4^{th} month after the accident; *i.e.*, those who return in the 3^{rd} and those who return in the 4^{th} month after, and so on (by the

12^{th} month after the event everyone is in the sample from which the corresponding averages are estimated).

The first observation from Figure 1 is that the average wage of the control individuals is gradually increasing over m, with no breaks or jumps around $m = 0$—which is as expected, for two reasons: first, $m = 0$ was randomly chosen for the control individuals, and second, they do not suffer an accident so their wages are expected to gradually grow *via climbing the wage ladder*. The second observation is that the average wage of the treated individuals falls at the time of the accident, and never catches up with the average wage of the controls, not even by 2 years after return to work.

The rest of the paper investigates (i) this raw relationship between post-accident absence and wages, after controlling for a rich set of firm-level characteristics, for all and by subgroups; (ii) the robustness of our results to restricting the sample only 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 for individuals who had an accident but no subsequent absence spell. Importantly, before investigating how the wage and firm quality of the treated evolves after the accident, we present evidence suggesting that our chosen accidents do not lead to long-term labor productivity drops.

5.2 Distribution of Those with Accidents, by Type and Body Parts Injured, and Length of Absence

Table 1 shows the most frequent accident types in our data, separately for those who were and were not absent from work after the accident. The table, 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 with absence after the accident, while one-fifth suffered a fracture in the knee/lower leg, 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.[27]

Table 2 shows the distribution of blue-collar and white-collar workers who suffered an accident and were absent thereafter, by accident type and body parts injured; there are no major differences in the distributions, except that injuries and fractures of the hand/fingers/wrist are over-represented among blue-collars, and injuries and dislocations of the knee/lower-leg are over-represented among white-collars.

Table 3 shows a similar distribution, by length of absence: almost 40 percent of individuals with an accident and absence thereafter were absent for 3 months and 85 percent were absent for at most 6 months; moreover, suffering fractures, hip-accidents and leg-accidents are positively associated with length of absence.

5.3 Event Study Figures of Health-Related Variables for Those with Accidents

To check whether our chosen accidents have indeed no persistent labor productivity loss implications, we provide supporting evidence that they have no permanent effects on workers' health, with outcomes as: sickness benefit uptake, hospitalization, and prescription drug spending (on all and on specific drug categories). We provide the evidence separately for those who (i) suffer an accident and have a short-term (3–12-months-long) period of absence thereafter, and who (ii) suffer an accident but have no absence spell.

[27]We show in Appendix Figure C1 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.

We estimate event study regressions, where the month of the accident is set to event time 0, the reference month is the month one year before the shock, 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 their time of the accident. The event study equation is the following (as in Schmidheiny and Siegloch, 2023):

$$\Omega_{it} = \sum_{k=-36}^{36} \delta_k d_{i,t-k} + \theta_t + \mu_i + \varepsilon_{it}, \tag{21}$$

where Ω_{it} is the outcome variable for individual i at monthly date t, $d_{i,t-k}$ is a set of indicators showing if individual i at time t is 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. In the following figures, we graph the $\hat{\delta}_k$'s, with their 95 percent confidence bands, for $k = -36, ..., 0, ...36$, to see if various indicators of sickness prevail before, around, and after the event (happening at $k = 0$).

Figure 2 shows that while there is an 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 6 months after the accident (*see* Panel (a) of Figure 2), while the average number of days spent on sickness benefits approaches zero 12 months after the accident (*see* Panel (b) of Figure 2), after which these averages are statistically indistinguishable from zero. Spending on antiinfectives and musculoskeletal drugs also tapers off after 2 and 6 months, respectively (*see* Panels (d) and (e) of Figure 2), while there is no spike in spending on psychoanaleptics neither around the accident, nor after (*see* Panel (f) of Figure 2).

Figure 2 also shows that there is no indication of sickness, in terms of hospitalization, uptake of sickness benefits, or drug spending, before the accident, supporting the unanticipated nature of our chosen accidents.[28]

5.4 Descriptive Statistics of Treated and Control Individuals

Table 4 shows the mean of various control variables, separately for those who suffered and who have not suffered an accident ($N = 27,487$ and $N = 522,108$, respectively), as well as separately for those who were and were not absent, following an accident ($N = 11,193$ and $N = 16,294$, respectively).

Table 4 reveals that individuals who suffered an accident are predominantly male and blue-collar workers (77 and 62 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. It also reveals that 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 likely to work at a foreign-owned firm, but tend to be employed at larger firms, with mixed patterns for health expenditures.

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 strategies in which we allow and control for any level differences between treated and control individuals prior to the accident.

[28]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.

6 Empirical Strategies

Next, we describe the empirical strategies with which, to the extent that the relevant identification requirements hold, we measure the labor effects of short-term absence, following an unexpected and mild accident.

We use three empirical strategies. In the first and main one, described in Section 6.1.1, we compare the labor trajectories of individuals who suffer a mild accident and are absent for 3–12 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 6.1.2, we employ an Instrumental Variable (IV) strategy, 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. 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 6.2). In this approach, we compare the labor trajectories 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 individuals who are prone to suffer an accident, and potentially would have worse labor trajectories even in the absence of an accident. Finally, to investigate if our results are driven by (unobserved) differences between the employers of treated and control individuals, we perform within-firm matching (described in Section 6.3).

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

6.1.1 Linear Model using Ordinary Least Squares (OLS)

In our main Difference-in-Differences (DiD) empirical strategy, we compare the trends of key labor market outcomes—of hourly deflated log-wages, estimated AKM firm effects, and other firm quality measures—upon return to work, 1 year after return to work, and 2 years after return to work, of

1. "treated" individuals who suffer an accident at a given point in time, while being employed, are absent for d periods thereafter, and return to work at $t + d$ within a year, with the labor trends of

2. "control" individuals with no accidents, who are employed in a given month.

There are 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 control groups of individuals; *i.e.*, control individuals do not switch in or out of treatment. As a result, our main empirical strategy only uses never-treated units as a control group, avoiding the problem of "*forbidden comparisons*" highlighted recently 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 control individuals can be attributed to absence following an accident.

Before spelling out and describing the linear DiD estimation equation, two remarks on notation and the control individuals. First, we refer to the event date e_i, using monthly observations, as the calendar date e at which the accident happens for individual i. Following this notation, the calendar date $e_i + d_i$ will denote the date at which the individual i returns to work after an accident that happens at calendar date e. Second, out of all time periods in which the selected control individuals are employed, we pick a random date, to assign it as the corresponding event date, e_i.[29] We also randomly pick a d_i from the integer uniform distribution between 3 and 12 for all control individuals, to be able to assign a post-event outcome for them.[30]

Let us 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 (3–12-months long) absent period thereafter (for d_i periods); this variable then 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 corresponding 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:

$$\Omega_{ijt} = \alpha_0 + \alpha_1 \mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} + \alpha_2 \mathbb{1}\{t \geq e_i + d_i\} + \alpha_3 \mathbb{1}\{t \geq e_i + d_i\} \cdot \mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} + \alpha_4 X_j^{pre} + \alpha_5 X_i^{pre} + \mu_t + \mu_d + \nu_{ijt}, \quad (22)$$

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, (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 (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). Finally, μ_t are event time fixed effects (capturing monthly calendar time of the event), μ_d are fixed effects for the duration of absence (in months), and ν_{ijt} is the unobserved error term.

The coefficients of interests are α_1, α_2 and α_3. α_1 captures pre-accident differences, conditional on X_j^{pre} and X_i^{pre}, between the treated and the control individuals. After controlling for observable characteristics, α_1 captures any remaining time-invariant differences between treated and 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 $ACC_i = 0$), for which our theoretical model has a clear prediction: in the absence of an accident, wages should increase *via* the mechanism of *climbing the wage ladder*, implying that $\alpha_2 > 0$.

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*. Our framework predicts $\alpha_3 < 0$, *via* the mechanism of *foregone opportunities*

[29] The health shock hits different individuals at different time periods. As a result, the shock (event) time is individual-specific.

[30] To see the importance of this random choice, consider a control individual A that works in January, February, March,..., December, 2010. Then, the January 2010 observation could serve as control for treated individuals returning to work in January 2010, the February 2010 observation could serve as control for treated individuals returning to work in February 2010, *etc.* That said, using both the January 2010 and the February 2010 observation for this particular control individual would artificially increase the number of control observations, and would artificially reduce the standard error of estimates.

to climb the wage ladder. 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.* Recall that our theoretical model predicts that, following a mild accident leading to temporary absence, with no labor productivity losses during accident-induced absence, the wage upon return should be equal or higher than the wage of their *past self*, *i.e.*, that $\alpha_2 + \alpha_3 \geq 0$. As a result, not only does our model impose restrictions on the sign of α_2 and α_3, but it also imposes restrictions on their relationship. On the contrary, if the wage of the individual suffering the accident decreases due to human capital depreciation or productivity losses during accident-induced absence, then, $\alpha_2 + \alpha_3 < 0$. Importantly, since $\alpha_2 + \alpha_3 \geq 0$ is inconsistent with the accident leading to permanent labor productivity losses, testing it may serve to validate the exclusion restriction that accidents are related to wages only through absence, and not directly through other channels, making ACC_i a candidate for an Instrumental Variable (IV) for $Absence_i$.

6.1.2 Linear Model using Instrumental Variables (IV)

Our slightly modified approach to the linear DiD model using OLS leverages the intuition that we are interested in *accident-induced absence from work* specifically, to test our theoretical predictions.

In this subsection, we explain how we use accidents as an IV for having been 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 on labor trends, 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. Note that 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 the state of absence. We provide numerous pieces of evidence suggesting that this is not the case.

First and foremost, we test the null hypothesis that $\alpha_2 + \alpha_3 \geq 0$ (against $\alpha_2 + \alpha_3 < 0$), which we cannot reject; *i.e.,* there is no indication that treated individuals' wages were lower upon return, relative to their *past selves.* Second, we look at whether our chosen accidents lead to permanent changes in health outcomes (hospitalizations, drug expenditures, *etc.*), which we saw in Section 5 that they do not. Third, 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.*, broken leg) could have an impact for that long on worker productivity. Fourth, we also verify our results are robust for both white-collar and blue-collar workers (with the idea that while it is possible that accidents lead to permanent changes in labor productivity for blue-collar workers, it is hard to argue that this is the case for white-collar workers). Our finding that our results persist when focusing on white-collar workers only is inconsistent with permanent drops in labor productivity fully explaining them. Fifth, we do not find any indication that individuals—either blue-collar or white-collar—would work in physically less demanding or less stressful occupations, 1-2 years after returning to work following an accident.

Finally, for the IV estimator to provide the LATE, we also need the monotonicity requirement to hold (the IV affecting 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 $Absence_i$ with ACC_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 it is for the *compliers*:

$$\Omega_{ijt} = \beta_0 + \beta_1 \mathbb{1}\{Absence_i = 1\}_i + \beta_2 \mathbb{1}\{t \geq e_i + d_i\} + \beta_3 \mathbb{1}\{t \geq e_i + d_i\} \cdot \mathbb{1}\{Absence_i = 1\}_i$$
$$+ \beta_4 X_j^{pre} + \beta_5 X_i^{pre} + \tau_t + \tau_d + \vartheta_{ijt}. \quad (23)$$

6.2 Robustness Check: Empirical Strategy Using Only Those with Accidents, Exploiting the Random Timing of Accidents

As a robustness check, and addressing any 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 outcomes of individuals who are sufficiently 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. Our estimation equation for individual i, suffering an accident at time e_i is given by

$$\Omega_{ijt} = \alpha_0 + \alpha_1 \tilde{\mathbb{1}}\{ACC_i = 1 \,\&\, Absence_i = 1\}_t + \alpha_2 \mathbb{1}\{t \geq e_i + d_i\} + \alpha_3 \mathbb{1}\{t \geq e_i + d_i\} \cdot \tilde{\mathbb{1}}\{ACC_i = 1 \,\&\, Absence_i = 1\}_t$$
$$+ \alpha_4 X_j^{pre} + \alpha_5 X_i^{pre} + \mu_t + \mu_d + \nu_{ijt}, \quad (24)$$

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 individual 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).

As shown by Goodman-Bacon (2021) and discussed in De Chaisemartin and D'Haultfoeuille (2022), when treatment is binary and groups can switch into treatment but not out (staggered design), then, identification of two-way fixed effects difference-in-differences specifications are problematic under dynamic treatment effects. For this reason, we do not consider two-way fixed effects difference-in-differences estimators.[31] Importantly, for our main strategy (using individuals that never had an accident as control group), since control individuals do not switch in or out of treatment, this concern does not apply.

6.3 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 the baseline *main sample*).

[31] In our case, two-way fixed effects would correspond to including dummies in year μ_t and individual μ_i.

We apply a kernel matching method, as described among others by Heckman et al. (1998) and Smith and Todd (2005).[32] We require exact matching on gender, firm identifier 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 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.[33]

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 (assumption (A-4) in Heckman et al., 1998).[34] 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 by Appendix Figure B1. We present the matching estimates of the Average Treatment Effect on the Treated (ATT).[35]

7 Estimation Results

7.1 Main Ordinary Least Squares (OLS) Estimates for Main Outcomes

Panel A of Table 5 shows the estimation result of our Difference-in-Differences OLS model, as specified in Section 6.1.1. Columns (1) and (2) show the results 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 the results 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 accident, wages of treated individuals are, on average, 1.57 (s.e. 0.17) percent larger than wages of the control group, which indicates a small positive selection prior to the event. The second row indicates that wages for the control group are 4.33 (s.e. 0.32) percent higher after the (random) event, consistent with individuals *climbing the wage ladder*, in the absence of events, such as accidents. The third row reports our estimate of the Average Treatment Effect (ATE) of being temporarily (for 3–12-months) absent following a mild and unexpected accident: wages are significantly, on average, 5.03 (s.e. 0.37) percent lower upon return, relative to what they would have been

[32]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 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.

[33]In Appendix B Table B1 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 can be observed for log health spending 3-12 months before the event, but even for that variable the standardized difference is only 0.118. Importantly, 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 observations and their matched control observations.

[34]This is different from the identifying assumption behind the DiD method, which is the common trend assumption.

[35]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).

in the absence of the event (*i.e.*, relative to the wage of the individual's *counterfactual self*).

To test our model's prediction that upon return from absence, individuals have an equal or higher wage than the wage of their *past self*, we formally test the $\alpha_2+\alpha_3=0$. The estimate for that sum is -0.007 and the corresponding *p-value* is 0.066. This latter result indicates that post-absence wages are only slightly below the pre-absence wages for the *treated* individuals, upon return to work following a mild accident, and they are not significantly different at the 5 percent level. Thus, the observed wage losses of 5 percent relative to workers' *counterfactual self* are inconsistent with human capital depreciation and major productivity losses during accident-induced absence, given that treated workers' wages upon return to work are not significantly smaller than their *past self*'s pre-accident wage at the 5 percent level.

Column (2) shows similar results for the estimated AKM firm effect, corresponding to firm productivity (firm quality): the result in the second row of column (2) shows that the estimated AKM firm effect is higher by 0.004 (s.e. 0.0002) for the control group after the (random) event (corresponding to a 0.4 percent higher firm-specific wage premium), being consistent with the wage ladder framework in which individuals experience wage growth by sequentially moving to better, higher-paying employers, *via climbing the ladder*. Our estimate of the ATE for the AKM firm effect is at -0.004 (s.e. 0.0012). The baseline average of the AKM firm effect at the control group is 0.025, with a standard deviation of 0.344, therefore the -0.004 point estimate on the AKM firm effect corresponds to a 1.16 percentage points decrease of a standard deviation.

Results in columns (3) and (4) show that the effects persist even 1 year after returning to work, following the event. In particular, we now see that an absence following an accident significantly decreases wages by 2.13 (s.e. 0.37) percent, on average, 1 year after returning to work, and it decreases the estimated AKM firm effect by 0.003 (s.e. 0.002), albeit the estimated effect on the AKM firm effect is not statistically significant. Finally, results in columns (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 -2.78 (s.e. 0.45) percent for wages and -0.006 (s.e. 0.003) for the firm effect.[36] [37]

7.2 Instrumental Variable (IV) Estimates for Main Outcomes

Panel B of Table 5 shows the estimation result of our Difference-in-Difference IV specification. The IV strategy allows us to estimate the Local Average Treatment Effect (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 sub-population 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 40

[36]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. Under the restricted sample, upon return to work, the sample size is 878,303, the treatment effect on log wage is -0.050 (s.e. 0.005) and on AKM firm effect is -0.004 (s.e. 0.001). Then, 1 year after the sample size is 865,056, the treatment effect on log wage is -0.021 (s.e. 0.004) and on AKM firm effect is -0.003 (s.e. 0.002). Two years after the sample size is 797,650, the treatment effect on log wage is -0.027 (s.e. 0.004) and on AKM firm effect is -0.005 (s.e. 0.003).

[37]Note, that our wage results are not driven by lower observed wages during sickness absence. Our wage definition excludes months with non-zero days on sickness benefit. However, we do not observe if someone is on sick leave (which is at most 15 days per year), which may affect the estimated wage effects upon return, as sickness absence is prevalent in the first year after the shock (Figure 2, panel (b)). To check the possible impact of unobserved sick leave on the wage estimates, we assume that if someone claims sickness benefit then she has already exhausted the 15 days of sick leave because the payment received during sick leave is higher than the amount of sickness benefit. Wages observed at or after the first month of sickness benefit in a year are unlikely to be affected by (unobserved) sick leave periods. Therefore, we restrict the wage data of the treatment group upon return to months at or after the first month of sickness benefit in the year of return. Under this restriction, the estimated treatment effect on log wage is -0.0386 (s.e. 0.0051), and the estimation sample size is 899,987.

percent of those with an accident have an absence-spell thereafter, the corresponding share of those with no accident is only 2 percent, implying a very strong 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 4.20 (s.e. 0.60) percent and the AKM firm effect by 0.004 (s.e. 0.002), relative to the outcomes of the individual's *counterfactual self*. Columns (3) and (4) show that the results persist in magnitude for both wages (-1.70 percent, s.e. 0.6) and AKM firm effect (-0.006, s.e. 0.003), 1 year later. Finally, Columns (5) and (6) show similar patterns for outcomes 2 years after return to work.

7.3 OLS Estimates for Other Firm Quality and Occupation Measures

In *Panel C* of Table 5 we show that the patterns for firm quality presented in section 7.1 continue to hold if we use the estimated TFP as another measure of firm quality.

The second row of *Panel C* of Table 5 confirms that employer TFP of control individuals is growing over time, consistent with workers *climbing the wage ladder via* switching to better firms. In the third row of *Panel C*, we see that short-term absences following a mild accident lead to 0.018 (s.e. 0.006) lower employer TFP upon return to work, 0.022 (s.e. 0.009) lower employer TFP 1 year later, and 0.015 (s.e. 0.013) 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 1.6, therefore the -0.02 effects correspond to 1.3 and 1.9 percentage points decrease in the standardized measure of employer TFP (that has mean zero and standard deviation of one). More generally, these results confirm that a short-term absence following a mild accident decreases employer quality for at least up to 2 years after returning post-accident, as measured by the employer's TFP.

Next, in Table 6 we check whether 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 the lower wages in the new occupations with better working conditions; or, it might also reflect long-term labor productivity losses, especially for blue-collars.

Panel A shows that upon return after an absence, individuals tend to have somewhat less stressful and less physically demanding occupations than they would have had without the absence. At the same time, the difference is small and it disappears in the longer run. For instance, treated individuals with an absence-spell following an accident work in an occupation with a 0.7 (s.e. 0.3) percent of a standard deviation lower physical score and a 0.5 (s.e. 0.4) percent of a standard deviation lower stress score upon return to work; that said, these difference cannot be detected 1 year and 2 years after return.

Panel B presents similar results for the sub-sample of white-collar workers, and *Panel C* does the same for blue-collar workers. Average patterns do not seem to be driven by the blue-collar sub-sample, and white-collar workers seem to switch to physically more demanding occupations in the longer run. White-collar workers have an occupation 1 year after return that has a 3.02 (s.e. 1.1) percent of a standard deviation higher score in the O*NET "physically demanding" occupation dimension, which increases to 6.42 (s.e. 1.67) percent of a standard deviation 2 years after return. 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 than the one pre-accident. Blue-collars have, on average, a 0.80 (s.e. 0.47) percent of a standard deviation less stressful occupation upon return, but this difference cannot be detected 1 year and 2 years after return.

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

7.4 Heterogeneity Analysis

In this section, we first check to what extent the aforementioned estimates—focusing on the interaction term $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} \times \mathbb{1}\{t \geq e_i + d_i\}$, stemming from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 6.1.1—differ by subgroups along the dimensions of age, occupation, tenure, and gender, type of injury and the specific body part injured, as well as pre-event wage and firm effect of the individual (if above/below the median in our sample). We also look at whether the coefficient estimate on the interaction term differs by length of absence (3, 4, $5-6$, and $7-12$ months), with the note that length of absence may be endogenous, and the estimates by length are only suggestive.

Before discussing the results, one note regarding heterogeneous effects in our model *versus* in the data: in our theoretical framework we consider identical individuals to keep the model purposely tractable. Nonetheless, in our theoretical framework, heterogeneity could be modeled by segregated markets with differing employer productivity distributions $F(y)$. These differing employer productivity distributions $F(y)$ lead to different wage distributions and, as a result, different effects of absence on the wage path of an individual.

7.4.1 By Tenure, Age, Occupation and Gender

Figure 3 shows the estimated coefficient, $\hat{\alpha}_3$, and its 95 percent Confidence Interval (CI), on the interaction term of $\mathbb{1}\{t \geq e_i + d_i\} \cdot \mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ in model (22), for the six main outcomes ($ln(w)$ and $\hat{\phi}_j$ upon return, 1 year after, and 2 years after, respectively), by tenure, age, occupation and gender.

The *top-left* panel of Figure 3 shows the estimated effect of absence post-accident on wages upon return to work: they are significant for all subgroups, and the confidence intervals typically overlap, except for the tenure and age heterogeneities, where the negative wage effect is stronger for the youngest ($20-30$ years old) age group and the one with less than 1 year of tenure at the firm (experiencing a large 8.3 and 9.4 percent wage loss upon return, relative to their *counterfactual self*'s, respectively).

The *top-right* panel shows the estimated impacts on the estimated AKM firm effect upon return to work: the estimated effects (in absolute value) are larger for those with less than 1 year of tenure, the youngest (20–29 years old), white-collar and female workers (with all significant estimates around $(-0.007)-(-0.009)$).

The two *bottom-left* and *bottom-right* panels show the estimated impacts for wages and firm effects 1 and 2 years after returning to work, respectively. The longer-term effects on wages are driven by short-tenured (< 1 year) and white-collar individuals, as well as by young in case of wages 1 year after return, while the longer-term effects on the estimated AKM firm effect are driven by the young and by females (although the CIs overlap, the point estimates of one subgroup typically do not fall into the CIs of the other subgroup).

In sum, there are no differences by gender for wages, only for the estimated AKM firm effects, and the persistent effects on wages and firm effects 1 year (and 2 years) after returning to work are driven by the short-tenured, the youngest ($20-29$ years old), and the white-collar individuals for the wage and females for the AKM firm effect.

7.4.2 By Type of Injury and Body Parts Injured

Figure 4 shows the estimated interaction coefficient and its 95 percent Confidence Interval, on the interaction term, for the 6 main outcomes we consider, 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). Overall, 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.

7.4.3 By Pre-Event Levels of Wage and Firm Effect, and Length of Absence

Figure 5 shows the estimated interaction coefficient and its 95 percent Confidence Interval, on the interaction term, for the 6 main outcomes, by pre-event levels of wage and firm effect, and by length of absence.

The point estimates for the groups by pre-event firm effect and pre-event wage are larger in absolute value for all outcomes, but (i) only for the wage upon return and 2 years after and for the AKM firm effect 1 and 2 years after are the point estimates for those with above median pre-event firm effect significantly higher in absolute value, and (ii) only for the longer term outcomes (1-2-years after return to work) are the point estimates for those with above median pre-event wage significantly higher in absolute value.

In sum, wage effects (at least upon return) are significantly negative in all groups by length of absence, and they are significantly negative 1 and 2 years after return for those who were absent for 4 months and 7–12 months; while the point estimates for relatively longer ($7-12$-months-long) absence are larger for all of the outcomes—consistent with longer time away in which the individual *missed opportunities to climb the wage ladder*—they seem to be significantly larger only for the immediate wage effect upon return.

7.5 Robustness and Falsification Checks for the Main OLS Estimates

7.5.1 Estimates for Robustness Check: Exploiting the Random Timing of Accidents

Table 7 shows the results of our robustness analysis in which we use individuals that suffer an accident 3 years or later in the future as controls, to address any remaining selection concern (*i.e.*, whether individuals select themselves into suffering an accident, even conditional on our rich set of control variables of labor and medical histories). Our results indicate that estimated labor losses of short-term absence are very similar to our baseline ones, when focusing on just the group of individuals who suffer an accident at some point.

Results in the first column and first row indicate that prior to the event the wages of treated individuals were 1.5 (s.e. 0.25) percent higher than that of the control group. Results in the second row indicate that wages for the control group are 3.97 (s.e. 0.25) percent higher after the event, consistent with the notion that individuals *climb the wage ladder*. The third row reports our estimate for the effect of being temporarily absent from work following an unexpected and mild accident: wages are 4.87 (s.e. 0.5) percent lower upon return to work, relative to what they would have been in the absence of the accident.

Column (2) shows similar results for our main measure of firm quality—the estimated AKM firm effect. Results in the the second row of column (2) indicate that the estimated AKM firm effect is 0.0089 (s.e. 0.0003) larger for the control group after the event, consistent with the prediction that, in the absence of the accident, individuals *climb the wage ladder* and experience wage growth by sequentially moving to higher-paying employers. The third row of column (2) indicates that temporary absence following a mild accident leads to individuals being employed by firms with a 0.0093 (s.e. 0.0017) lower firm quality, relative to the counterfactual of no accident (yet). This estimated effect corresponds to a 2.78 percentage points decrease in the standardized measure of AKM firm effect.

Columns (3) and (5) show that wages are still lower by 1.86 (s.e. 0.41) percent 1 year later and by 2.08 (s.e. 0.54) percent 2 years later, relative to the counterfactual of no accident. Finally, results in columns (4) and (6) confirm the persistence of the effect on firm quality (the AKM firm effect). Firm quality is still 0.011 lower 1 year later and 0.0131 lower 2 years later, relative to the counterfactual of no accident (yet).

In sum, our baseline results are not driven by selection into suffering an accident; *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.

7.5.2 Estimates for Robustness Check: Within-Firm Matching

Table 8 reports the matching 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).

The Average Treatment Effect on the Treated (ATT) estimate on wage is -5.1 (s.e. 0.38) percent upon return, which decreases to -2.3 (s.e. 0.37) percent 1 year later, and to -2.0 (s.e. 0.46) percent 2 years later. The ATT estimate on the AKM firm effect is -0.005 (s.e. 0.001) upon return, -0.008 (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 under the matching and OLS estimations. Thus, our estimated treatment effects are not driven by (potentially unobserved) differences between the employers of treated and control individuals.

7.5.3 Estimates When Using Alternative Measures of Hourly Wage and Wage Income

Table 9 presents further robustness checks, for the "Linear Model using OLS" (Section 6.1.1), using alternative definitions of the hourly wage and wage income, and shows estimates for weekly hours worked. In this table, each three consecutive columns refer to the same variable, in which the first presents estimates upon return, the second shows estimates 1 year after return, and the third shows estimates 2 years later.

Compared to the baseline hourly wage results (*Panel A* on the left), we adjust hourly wage with actual monthly days insured (*Panel B* on the right), and use monthly wage (*Panel B* on the left) which is not affected by potential differences in actual and reported (as of contract) weekly hours worked. We also adjust monthly wage with actual days insured (*Panel C* on the left) and actual days with an income that month (*Panel C* on the right). While there are some slight changes in the magnitude of the estimated coefficients, the main patterns stay the same, and none of these modifications affect our results: the immediate wage effect is between -6.43 and -3.52 percent upon return and between -2 and -3 percent 1 year and 2 years after return, no matter if actual days insured (and the corresponding wage income) are taken into account.

Estimates for the weekly hours worked (*Panel A* on the right) show a negative effect of absence after the accident, which remains even 2 years after return to work. Still, the magnitude is small (about 0.2 hour per week, corresponding to less than 15 minutes, on average), which suggests that decreasing labor supply in the form of hours or switch to part-time status is not a major mechanism behind the observed wage patterns.

7.5.4 Estimates for Falsificaton Test Using Individuals With Accident but No Absence

In this section, we present the test results of our model's predictions from Section 2.2 that individuals with an accident but no subsequent absence spell do not change how selective they are on wage offers, thus for them (i) an accident affects wages negatively only for the duration of the shock and (ii) does not impact the employer productivity either for the duration of the shock or in the longer-run.

In Table 10, 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. *Panels A* and *B* show 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. Furthermore, *Panels C* and *D* show, that apart from a weak negative effect 1 month after the accident (an estimated effect of $-0.0012(-0.0016)$ with s.e. $0.0005-0.0007$, 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. Thus, these individuals, consistently with our model's predictions, *do not incur any considerable foregone opportunities to climb the wage ladder.* At the same time, control individuals continue to *climb the wage ladder*, 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 $\hat{\phi}_j$.

8 Mechanisms: Decomposition of the Treatment Effect on Wages

8.1 Decomposition into Within- and Between-Firm Wage Loss

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 to climb the wage ladder at a given employer (within-firm wage loss).

Before addressing the question directly, we first look at the prevalence of firm switches and average wage changes. In a descriptive sense, we see from Table D1 that compared to control individuals, treated are by 0.18, 1.22 and 2.31 percentage points more likely to switch to a new employer, upon return, 1 year later, 2 years later, respectively. However, while the wage of the control individuals who stay and who switch grow by 4.4 and 5.4 percent, on average, respectively, the wage of the treated, upon return to work, grows only by 1 percent if they stay with their employer, and even decreases by 10 percent if they switch employers. Also 1 and 2 years after return to work the treated experience a smaller wage growth than the controls: their wage grows by 7.14 percent and 11.5 percent if they stay, and by 7.3 percent and 11.1 percent if they switch employers, while for the controls the same wage growth figures are 8.9, 13.3, 9.8 and 14.5, respectively. To address the question directly, we leverage the result in the proposition below.

Proposition 4 *Let $\Delta\log(x)$ represent the difference in $\log(x)$ 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 to climb the wage ladder at a given employer ("within-firm wage loss"). Then,*

$$\log(w) = \gamma_0 + \gamma_1 \log(y) + \epsilon \tag{25}$$

and

$$\Delta\log(w) = \gamma_1 \Delta\log(y) + \Delta w_{within}, \tag{26}$$

where γ_1 is a constant and $\Delta w_{within} = \Delta\epsilon$. See Appendix for proof.

Intuitively, the above proposition tells us that, 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": $\gamma_1 \Delta \log(y)$), and a second that is due to missed opportunities to climb the wage ladder at a given employer ("within-firm wage loss": Δw_{within}).

Next, note that $\gamma_1 \log(y)$, 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 accident-induced absence:

$$\frac{E[\Delta\gamma_1 \log(y)]}{E[\Delta \log(w)]}. \tag{27}$$

To form the sample analogue estimator for (27), first recall that ϕ_j in the AKM equation (19) is the time-invariant firm-specific wage premium (*i.e.,* $\phi_j = \gamma_1 \log(y)$); hence, for each individual, their employer's estimated AKM firm effect, $\hat{\phi}_j$, is the sample counterpart of $\gamma_1 \log(y)$. Second, for the denominator, the interaction estimates in the odd columns of *Panel A* in Table 5 are the ATE estimates for $\log(w)$, corresponding to the average estimated $\Delta \log(w)$ 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 $\Delta\gamma_1 \log(y)$.

Using the sample analogue estimator for (27) and estimates from Table 5, 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 and 2 of Table 5 *Panel A* imply that this share is 7.36 percent upon return to work, columns 3 and 4 imply that it is 13.15 percent 1 year later, and columns 5 and 6 imply that it is 20.14 percent 2 years later.[38] Irrespective of whether using the OLS or IV estimates, we find that the share of wage variation due to missed opportunities to switch employers is increasing with time.

8.2 Decomposition into Within- and Between-Occupation Wage Loss

Now, we decompose how much of the between-firm and within-firm wage loss stems from missed opportunities to move to better-paying occupations. In a descriptive sense we see that compared to control individuals, treated are by 0.72, 0.63 and 1.23 percentage points more likely to switch to a new occupation, upon return, 1 year later, 2 years later, respectively. However, while the wage of the control individuals who stay and who switch grow by 4.4 and 5.8 percent, on average, respectively, the wage of the treated, upon return to work, grows only by 0.3 percent if they stay in their current occupation, and even decreases by 7.85 percent if they switch to a new occupation. Also 1 and 2 years after return to work the treated experience a smaller wage growth than the controls: their wage grows by 7.14 percent and 11.7 percent if they stay, and by 7.41 percent and 10.5 percent if they change occupations, while for the controls the same wage growth figures are 9.1, 13.6, 9.5 and 14.03, respectively (Table D1). To do the decomposition formally, 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

$$\log(w) = \Sigma_0 + \Sigma_1 \log(\varkappa) + \zeta^G, \tag{28}$$

[38] Upon return: $0.0037/0.0503 = 7.36$ percent; the corresponding p-value for testing if this ratio is different from 0 is 0.002 (using the Delta Method). 1 year later: $0.0028/0.0213 = 13.15$ percent; the corresponding p-value is 0.081 (using the Delta Method). 2 years later: $0.0056/0.0278 = 20.14$ percent; the corresponding p-value is 0.008 (using the Delta Method). Our IV estimates from Table 5 *Panel B* imply higher estimates: they imply 9.05, 32.35, and 45.15 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. Importantly, while the OLS strategy identifies the effect of being absent following an accident, the IV strategy identifies the effect of *accident-induced absences* for the *compliers*.

where $\Sigma_1 \log(\varkappa)$ represents the occupation-specific wage premium workers receive from working in their current occupation;[39] *i.e.,* $\Sigma_1 \log(\varkappa)$ 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

$$\Delta \log(w) = \Sigma_1 \Delta \log(\varkappa) + \Delta\zeta^G, \tag{29}$$

where $\Sigma_1 \Delta \log(\varkappa)$ stems from missed opportunities to move to better-paying occupations, and $\Delta\zeta^G$ stems from missed opportunities to climb the wage ladder in the same occupation.

Second, we decompose $\Sigma_1 \Delta \log(\varkappa)$ into the part stemming from missed opportunities to move to better-paying occupations *with the same employer*, $\Phi\Delta \log(\varkappa)$, and into the part stemming from missed opportunities to move to better-paying occupations *with other employers*, $\Gamma\Delta \log(\varkappa)$. Similarly, we decompose $\Delta\zeta^G$ into the part stemming from missed opportunities to climb the wage ladder *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*, $\Delta\zeta^B$. Once we replace all these terms, we obtain

$$\Delta \log(w) = \Sigma_1 \Delta \log(\varkappa) + \Delta\zeta^G \equiv \Phi\Delta \log(\varkappa) + \Gamma\Delta \log(\varkappa) + \Delta\zeta^W + \Delta\zeta^B \tag{30}$$

where $\Sigma_1 \Delta \log(\varkappa) \equiv \Phi\Delta \log(\varkappa) + \Gamma\Delta \log(\varkappa)$, and $\Delta\zeta^G \equiv \Delta\zeta^W + \Delta\zeta^B$.

Third, note that $\Gamma\Delta \log(\varkappa)+\Delta\zeta^B$ 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. It follows that

$$\Gamma\Delta \log(\varkappa) + \Delta\zeta^B \equiv \gamma_1 \Delta \log(y), \tag{31}$$

where $\gamma_1 \log(y)$ represents the employer-specific wage premium workers receive from working for their current employer that has productivity (quality) y (as discussed above). Similarly, $\Phi\Delta \log(\varkappa) + \Delta\zeta^W$ 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 climb the wage ladder at a given employer. It follows that

$$\Phi\Delta \log(\varkappa) + \Delta\zeta^W \equiv \Delta w_{\text{within}}. \tag{32}$$

Then, from equations 31 and 32, it follows that

$$\Delta \log(w) = \Phi\Delta \log(\varkappa) + \Gamma\Delta \log(\varkappa) + \Delta\zeta^W + \Delta\zeta^B \equiv \gamma_1 \Delta \log(y) + \Delta w_{\text{within}}. \tag{33}$$

Now we are in a position to characterize shares of wage losses due to various missed opportunities. First, note that $(\Phi + \Gamma) \log(\varkappa) \equiv \Sigma_1 \log(\varkappa)$ 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

[39] As shall be clear in a few paragraphs, this assumed structure for wages is consistent with the wage formulation derived in Proposition 4. Furthermore, a wage structure without an occupation-specific premium is just a particular case of this equation, in which $\Sigma_1 = 0$.

premium, divided by the expected total wage loss, stemming from accident-induced absence:

$$\frac{E[\Delta(\Phi+\Gamma)\log(\varkappa)]}{E[\Delta\log(w)]}. \tag{34}$$

To form the sample analogue estimator for (34), first recall that z_o in the AKM equation, not controlling for firm fixed effects, (20a), is the time-invariant occupation-specific wage premium (*i.e.,* $z_o = (\Phi+\Gamma)\log(\varkappa)$); hence, for each individual, their occupation's estimated fixed effect, $\hat{z}_o$, is the sample counterpart of $(\Phi+\Gamma)\log(\varkappa)$. Then, for the numerator, the interaction estimates in the odd columns of Table 11 are the ATE estimates for $\hat{z}_o$, corresponding to the average estimated $\Delta(\Phi+\Gamma)\log(\varkappa)$ across individuals. (For the denominator, just as before, the interaction estimates in the odd columns of *Panel A* in Table 5 are the ATE estimates for $\log(w)$, corresponding to the average estimated $\Delta\log(w)$ across individuals.)

Second, note that $\Phi\Delta\log(\varkappa)$ 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:

$$\frac{E[\Delta\Phi\log(\varkappa)]}{E[\Delta\log(w)]}. \tag{35}$$

To form the sample analogue estimator for (35), first recall that $\tilde{z}_o$ in the AKM equation, controlling for firm fixed effects, (20b), is the time-invariant occupation-specific wage premium (*i.e.,* $\tilde{z}_o = \Phi\log(\varkappa)$); hence, for each individual, their occupation's estimated fixed effect, $\hat{\tilde{z}}_o$, is the sample counterpart of $\Phi\log(\varkappa)$. Second, for the numerator, the interaction estimates in the even columns of Table 11 are the ATE estimates for $\hat{z}_o$, corresponding to the average estimated $\Delta\Phi\log(\varkappa)$ across individuals.

Using the sample analogue estimator for (34) and estimates from Tables 5 and 11, 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 11 reports results of estimating our OLS model (22) for $\hat{z}_o$ (the *occupation fixed effect estimated from a wage regression not controlling for firm fixed effect*).[40] The second row tells us that $\hat{z}_o$ (*i.e.*, the occupation-specific wage premium) is higher by 0.0007 (s.e. 0.0001) for the control group after the (random) event – consistent with the wage ladder framework in which individuals experience wage growth by moving to better-paying occupations. Our ATE estimate for $\hat{z}_o$ is -0.0011 (s.e. 0.0004) upon return to work, which, given the baseline average of $\hat{z}_o$ for the control group is -0.010, corresponds to a 11 percentage point decrease. Dividing this ATE estimate for $\hat{z}_o$ by the ATE estimate for log-wages (from Column 1 of *Panel A* in Table 5) tells us that 2.19% of the wage drop is due to missed opportunities to move to better-paying occupations.[41]

Using the sample analogue estimator for (35) and estimates from Tables 5 and 11, 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 11 reports results of estimating our OLS model (22) for $\hat{\tilde{z}}_o$ (the *occupation fixed effect estimated from a wage regression controlling for firm fixed effect*). Our ATE estimate for $\hat{\tilde{z}}_o$ is -0.0008 (s.e. 0.0004) upon return to work; dividing this ATE estimate for $\hat{\tilde{z}}_o$ by the ATE estimate for log-wages (from Column 1 of *Panel A* in Table 5) tells us that 1.59% of the wage drop is due to missed opportunities to

[40] We restrict the samples in Table 11 to observations where the log wage and AKM firm effect are not missing.

[41] $0.0011/0.0503 = 0.0219$; the p-value for testing if this ratio is different from 0 is 0.007 (using the Delta Method).

move to better-paying occupations *with the same employer*.[42] These numbers imply that of the wage drop due to missed opportunities to move to higher paying occupations, 72.73% comes from missed opportunities to switches with the same employer.[43] 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.

9 Discussion of Results

Some discussion of our results is in order. First, our simple theoretical framework is kept tractable to keep the intuition clear and concise—with this in mind, we did not explore certain questions that would require adding more structure to the model; *e.g.*, we did not explicitly model how the wage gap between previously absent individuals and control individuals evolves over time. In our theoretical framework, the speed of catch-up depends on the characteristics of the distribution of employers, $F(x)$, in particular in the probability of extreme values (outliers). In our empirical analysis, we observe a small and slow catch-up: the difference in wages between treated and control upon return is of 4.2-5% (depending on if IV or OLS) which then drops to 2.1-1.7% 1 year later. In reality, the slow catch-up could also come from the employer distribution F being conditional on the current employer of the individual. In other words, the draw of employer productivity x could depend on the current employer the individual is working for, $y > y' \iff 1-F(x|y) > 1-F(x|y')$. In that case, individuals would climb up the wage ladder mechanically. Individuals would climb the wage ladder even in the absence of optimal employer switching. In order to focus on a framework in which job switching comes exclusively from individual optimal decisions to accept or reject competing offers, we restricted our case to a distribution F that is independent of the current employer of the individual, $F(x|y) = F(x|y')$.

Second, we did not explicitly model where the wage offers arise from. These offers can arise from several sources, such as: (i) when being present at work, workers 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 the ones that are getting a short-term task (not the colleague who is absent) in which they can either improve or impress their managers, which may lead to new wage offers, *etc.*

Third, it may be possible that the controls individuals not only climbed the wage ladder while the treated were absent, but also accumulated human capital; or, it may be possible that the controls' human capital will grow faster after the treated return. However, given the strong effects even for absences that last for only a few months, as well as for older, longer-tenured, and blue-collar workers (as in Table 5 and Figure 5), human capital accumulation is unlikely a key driver of the missed opportunities to receive new wage offers.

Finally, 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 -5.03 percent, -2.13 percent, and -2.78 percent, upon return, 1 year after, and 2 years after, respectively. The range of point estimates under the various specifications is between $(-4.2) - (-5.1)$ percent, $(-2.3) - (-1.7)$ percent, and $(-2.1) - (-2.0)$ 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 -0.0037, -0.0028, and -0.0056, upon return, 1 year after, and 2 years after, respectively. The range of point estimates under the various specifications is between $(-0.0093) - (-0.0037)$, $(-0.0110) - (-0.0055)$, and $(-0.0131) - (-0.0084)$, respectively.

[42] $0.0008/0.0503 = 0.0159$; the p-value for testing if this ratio is different from 0 is 0.02 (using the Delta Method).
[43] $1.59/2.19 = 0.7273$.

10 Conclusion

In this paper, we present new evidence that accident-induced periods of absence decrease individuals' wages for up to two years, by around 2.5 percent, relative to what they would have been in the absence of the accident, and that individuals reallocate to lower-quality employers. Our results suggest that even short-lived absences due to mild, unexpected accidents, with no persistent labor productivity losses, have persistent negative effects on labor trajectories, due to *foregone opportunities to climb the wage ladder*.

The persistent effects of *post-accident absence* on wages and firm fixed effects are stronger among young (20–29-years-old), short-tenure, and white-collar individuals, as well as among those who had higher-than-median wage and firm effect 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. Consistent with our model's predictions, 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, employers, or miss opportunities to climb the wage ladder at the same employer. Ending up at firms with relatively lower wage premium may have non-monetary consequences as well, since firms with lower wage premium 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 faced by the individual due to accident-induced absence from work.

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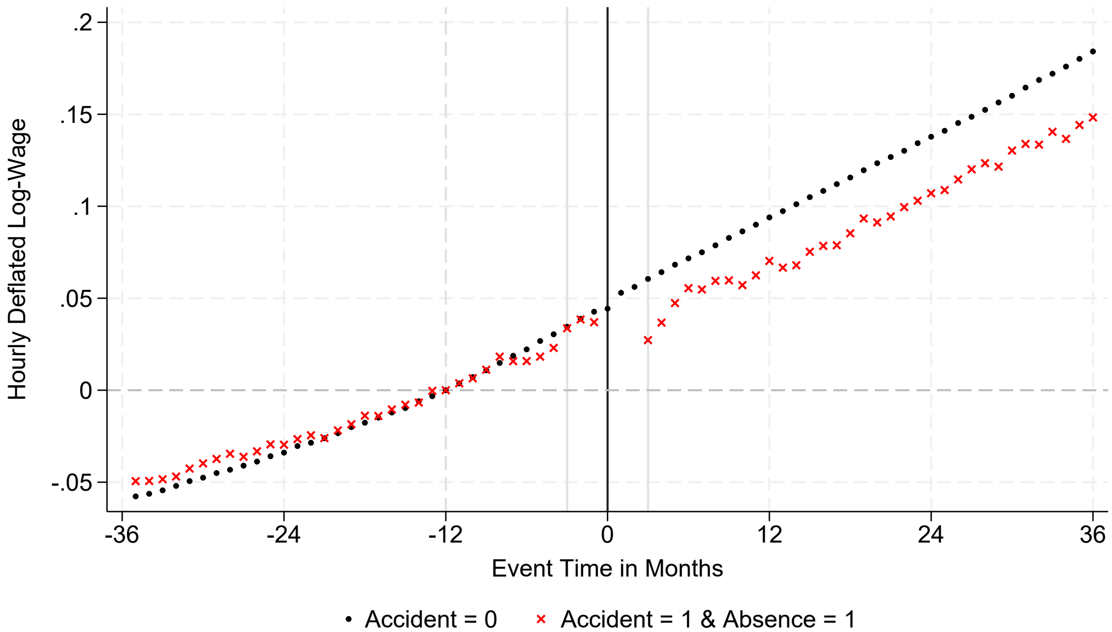
Figures

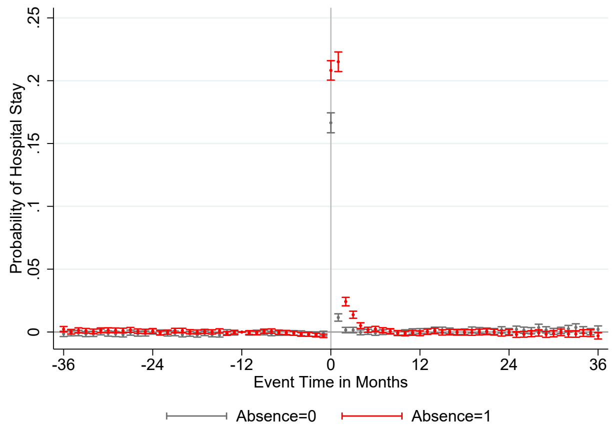
Figure 1: Wage Trajectory of Treated and Control Individuals Around the Time of the Event



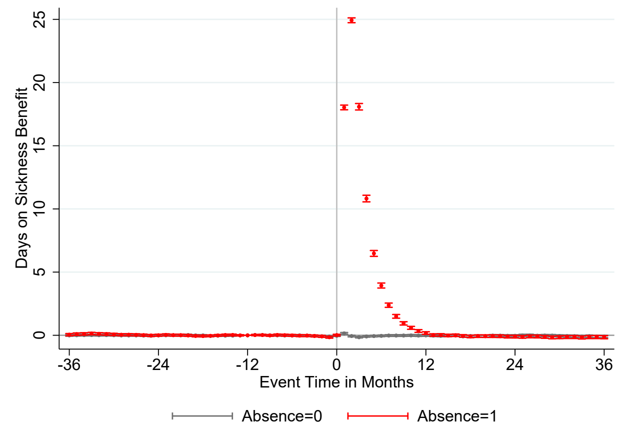
Notes: [1] The figure was constructed by (i) taking all observations of treated individuals (*i.e.*, those suffering an accident and being absent for 3–12 months thereafter) and control individuals (*i.e.*, those with no accidents) in the a 3-year-long window around the time of the event, (ii) taking the residual of deflated hourly log-wage on individual fixed effects and (iii) plotting the average of those residuals, separately for treated and control individuals, for m=-36,...,0,...,36, where $m = 0$ is the time of the event, and the average is normalized to 0 at $m = -12$ (averages for $m = 0, 1, 2$ not shown for the treated, as in those periods they are absent from work).
[2] The data of the treatment group in the first 12 months after the shock is restricted to months at or after the first month of sickness benefit in the given calendar year.
Data: Hungarian administrative matched employer-employee data, aged 20-50, employed at the month of the shock or a month before, 2009-2017; *main sample*.

Figure 2: Event Study Plots

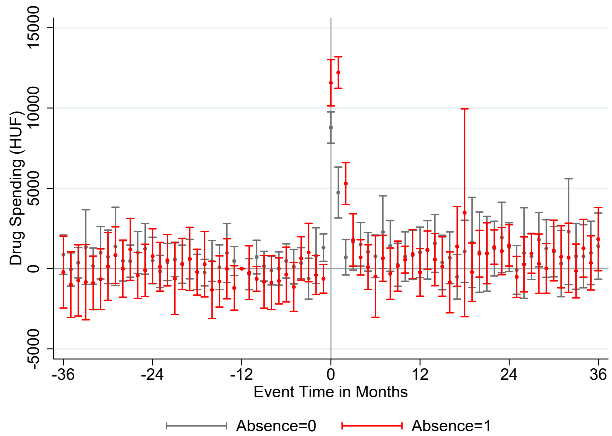
(a) At Least 5 Days per Month in Hospital



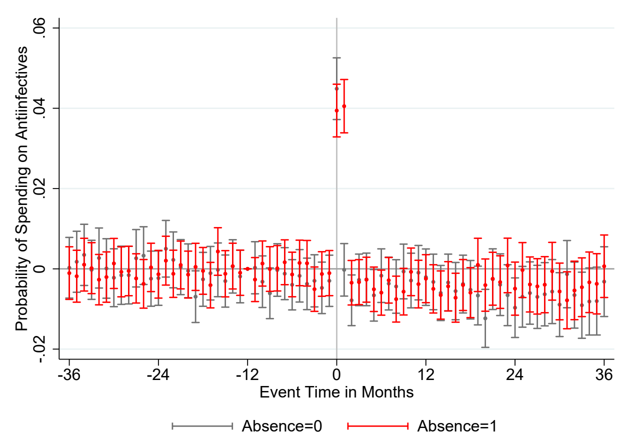
(b) Number of Days on Sickness Benefit



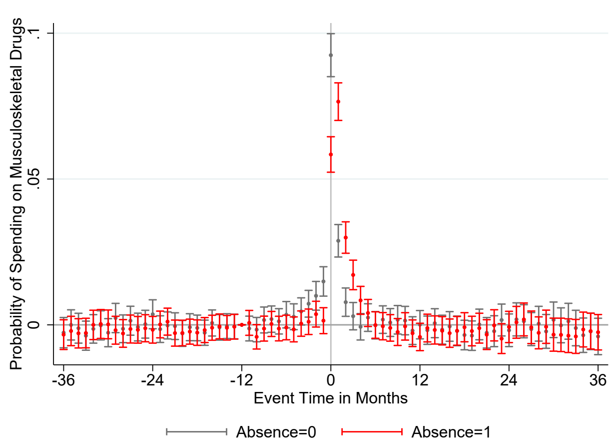
(c) Prescription Drug Spending (HUF)



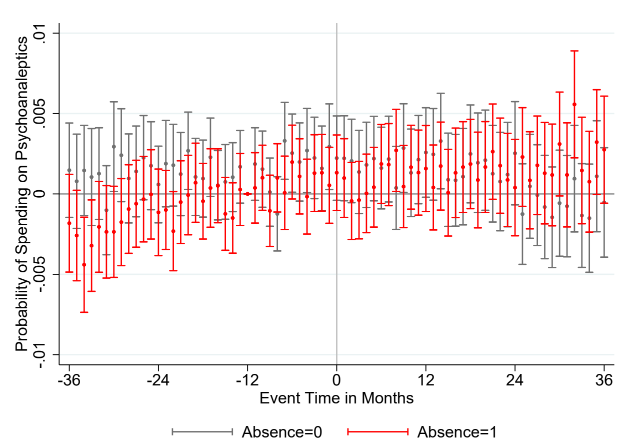
(d) Any Spending on Antiinfectives



(e) Any Spending on Musculoskeletal Drugs



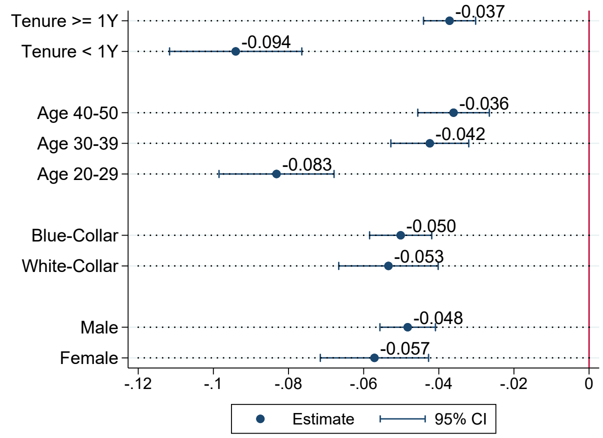
(f) Any Spending on Psychoanaleptics



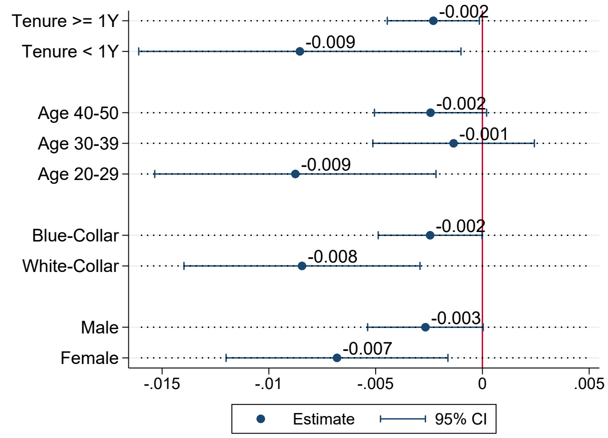
Notes: We graph the $\hat{\delta}_k$'s stemming from estimating equation (21) separately for the sub-sample of those who are absent after the accident and who are not, with their 95 percent Confidence Intervals, for $k = -36, ..., 0, ...36$, to see if various indicators of sickness prevail before, around, and after the event, $k = 0$, for individuals. 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).
Data: Hungarian administrative matched employer-employee data, aged 20-50, with accidents, employed at the month of the shock or a month before (control individuals employed at the month of the shock), 2009-2017.

Figure 3: Heterogeneity of the Main DiD (Interaction) Estimate on the Main Outcomes, by Tenure, Age, Occupation and Gender

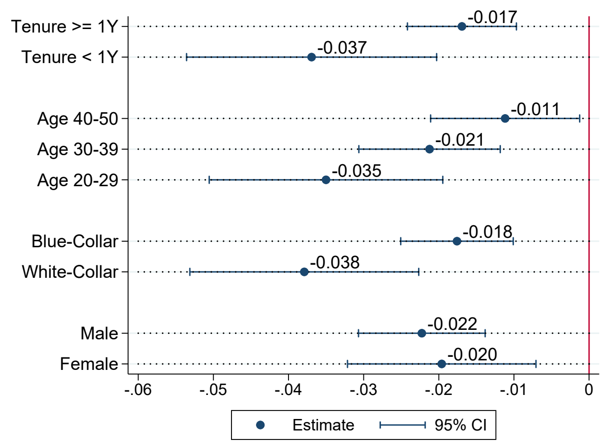
(a) Hourly Deflated Log-Wage, Upon Return



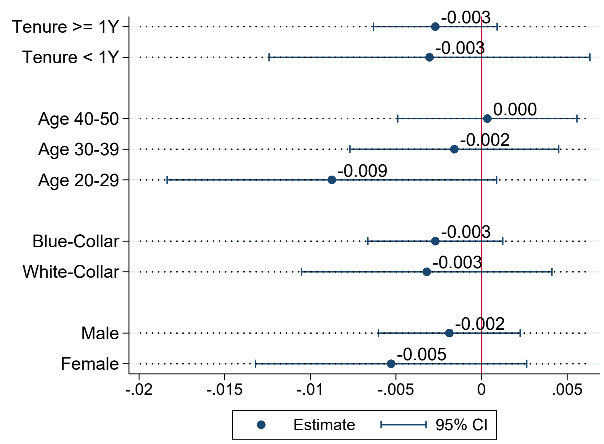
(b) AKM Firm Effect, Upon Return



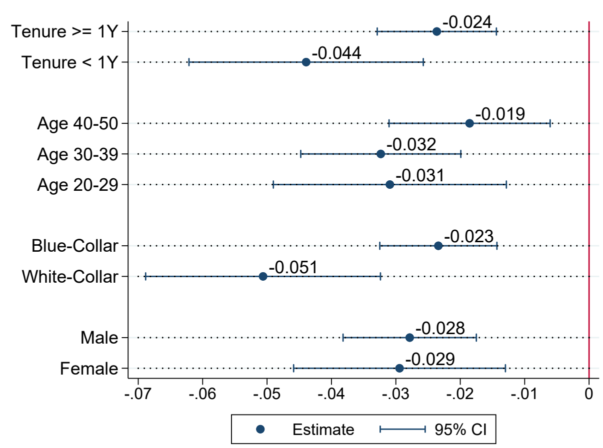
(c) Hourly Deflated Log-Wage, Upon Return + 1 Year



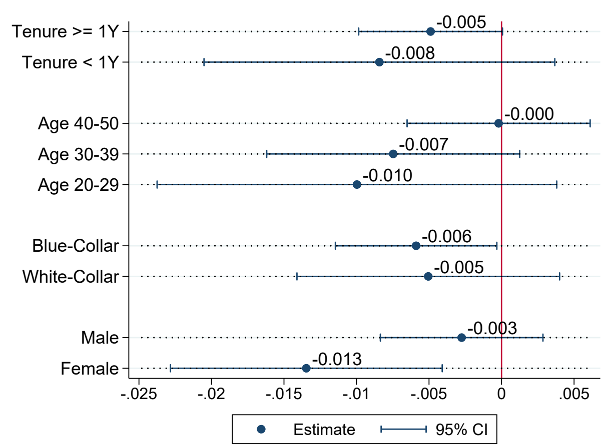
(d) AKM Firm Effect, Upon Return + 1 Year



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



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

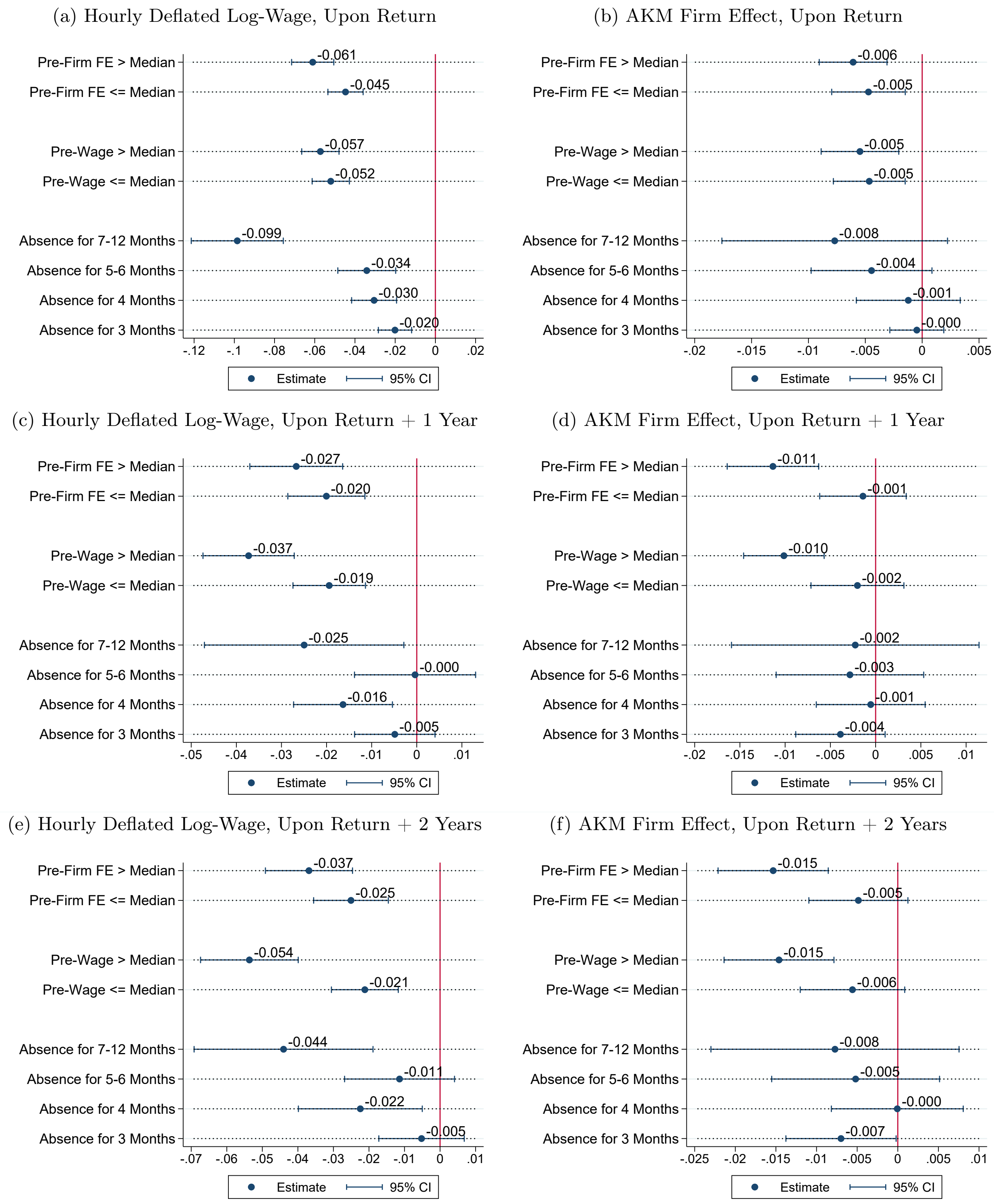


Note: These figures show the estimated coefficient and its 95 percent Confidence Interval, on the interaction term, stemming from estimating the "Linear Model using Ordinary Least Squares (OLS)", presented in Section 6.1.1, for the main six outcomes, using separate sub-samples as indicated on the vertical axis. The interaction term is $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} \times \mathbb{1}\{t \geq e_i + d_i\}$. 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 shock), 2009-2017; sub-groups within the *main sample*.

Figure 4: Heterogeneity of the Main DiD (Interaction) Estimate on the Main Outcomes, by Type of Injury and Body Part Injured

Note: 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 6.1.1, for the main six outcomes, using separate accident type sub-samples, as indicated on the vertical axis. The interaction term is $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} \times \mathbb{1}\{t \geq e_i + d_i\}$. 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 shock), 2009-2017, sub-groups within the *main sample*.

Figure 5: Heterogeneity of the Main DiD (Interaction) Estimate on the Main Outcomes, by Pre-Event Wage, Pre-Event Firm Effect, and Length of Absence



Note: 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 6.1.1, for the main six outcomes, using separate accident type sub-samples, as indicated on the vertical axis. The interaction term is $\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\} \times \mathbb{1}\{t \geq e_i + d_i\}$. 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 shock), 2009-2017; sub-groups within the *main sample*.

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	2.17	3.88	7.12	*13.17*	2.14	4.49	6.53	*13.16*
elbow, forearm	0.99	0.46	9.81	*11.26*	1.49	0.37	8.92	*10.78*
hand, fingers, wrist	8.97	0.48	9.68	*19.13*	11.07	0.64	11.08	*22.79*
hip, thigh	0.44	0.12	3.30	*3.86*	1.02	0.06	1.24	*2.32*
knee, lower-leg	7.23	12.83	23.89	*43.95*	5.09	24.67	9.91	*39.67*
ankle, foot, toes	0.21	1.52	5.00	*6.73*	0.31	1.56	2.66	*4.53*
head, neck, thorax, spine		1.91		*1.91*		6.75		*6.75*
total	*20.01*	*21.19*	*58.80*	*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, separately for those who were and were not absent, following an accident ($N = 11,193$ and $N = 16,294$, respectively).
Data: Hungarian administrative matched employer-employee data, aged 20-50, with accidents, employed at the month of the shock or a month before (control individuals employed at the month of the shock), 2009-2017.

Table 2: 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	2.30	4.05	6.66	*13.01*	1.73	3.27	8.75	*13.75*
elbow, forearm	1.17	0.48	9.86	*11.51*	0.36	0.36	9.64	*10.36*
hand, fingers, wrist	10.39	0.50	10.72	*21.61*	3.99	0.40	6.01	*10.4*
hip, thigh	0.42	0.11	3.19	*3.72*	0.48	0.12	3.67	*4.27*
knee, lower-leg	5.84	11.97	23.26	*41.07*	12.10	15.85	26.10	*54.05*
ankle, foot, toes	0.23	1.45	5.41	*7.09*	0.16	1.77	3.59	*5.52*
head, neck, thorax, spine		2.00		*2.00*		1.61		*1.61*
total	*20.35*	*20.56*	*59.09*	*100*	*18.84*	*23.40*	*57.77*	*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 ($N = 8,714$ and $N = 2,479$, 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 (control individuals employed at the month of the shock), 2009-2017.

Table 3: Distribution of Individuals with Accidents and Absence, and the Share of Fractures and Hip- and Leg-Accidents, by Length of Absence (in Months)

		share of accident type		
months	distribution (%)	fractures	hip-accidents	leg-accidents
3	38.94	0.524	0.020	0.469
4	24.21	0.593	0.033	0.513
5	13.24	0.618	0.050	0.545
6	8.24	0.650	0.064	0.561
7-12	15.37	0.683	0.072	0.531

Notes: The table shows the distribution of individuals with accidents and absence ($N = 11,193$), by length of absence (in months), and the share of individuals with a fracture of any body parts, or who suffered any type of accident affecting their hips and legs.
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 (control individuals employed at the month of the shock), 2009-2017.

Table 4: 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	0.774	0.764	0.560
age (in years)	37.003	36.038	36.446
blue-collar	0.779	0.511	0.585
in manufacturing	0.295	0.237	0.259
in trade	0.144	0.166	0.177
est. AKM individual FE	-0.078	0.093	-0.007
hourly log wage (lag 4)	6.340	6.577	6.470
est. AKM firm FE (lag 1)	-0.033	0.011	0.001
in foreign firm (lag 1)	0.272	0.304	0.324
n. of employees of the firm (lag 1)	1,587	1,211	1,679
log health expenditures (year prior)	6.712	6.915	6.667
inpatient expenditures (HUF, lag 1)	183,253	108,763	92,059
outpatient expenditures (HUF, lag 1)	4,849	4,898	4,222
social insurance support (HUF, lag 1)	6,957	5,937	5,523

Notes: The table shows the mean of various control variables, separately for those who suffered and who have not suffered an accident ($N = 27,487$ and $N = 522,108$ (in column (3)), respectively), as well as separately for those who were and were not absent, following an accident ($N = 11,193$ and $N = 16,294$, 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 shock), 2009-2017; *IV sample*.

Table 5: 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	
	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0157***	0.0005	0.0176***	-0.0003	0.0181***	-0.0001
	(0.0017)	(0.0008)	(0.0020)	(0.0008)	(0.0020)	(0.0009)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0433***	0.0040***	0.0900***	0.0039***	0.1355***	0.0050***
	(0.0032)	(0.0002)	(0.0056)	(0.0005)	(0.0076)	(0.0007)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	-0.0503***	-0.0037***	-0.0213***	-0.0028	-0.0278***	-0.0056**
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.0037)	(0.0012)	(0.0037)	(0.0018)	(0.0045)	(0.0025)
R^2	0.872	0.903	0.861	0.853	0.841	0.821
N	909,387	902,459	893,919	885,060	824,247	822,992

Panel B: IV Estimates	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$
$\mathbb{1}\{Absence_i = 1\}$	0.0172***	0.0009	0.0206***	-0.0001	0.0214***	-0.0000
	(0.0034)	(0.0012)	(0.0037)	(0.0013)	(0.0038)	(0.0014)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0439***	0.0040***	0.0901***	0.0040***	0.1357***	0.0051***
	(0.0032)	(0.0002)	(0.0056)	(0.0005)	(0.0076)	(0.0007)
$\mathbb{1}\{Absence_i = 1\}$	-0.0420***	-0.0038*	-0.0170***	-0.0055*	-0.0206***	-0.0093**
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.0060)	(0.0022)	(0.0065)	(0.0031)	(0.0079)	(0.0045)
N	937,075	930,070	921,308	912,209	849,521	848,273

Panel C: OLS Estimates Further Firm Quality Measure	(1) upon return	(2) after 1 year	(3) after 2 years
	TFP_j	TFP_j	TFP_j
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0231**	0.0194**	0.0199**
	(0.0091)	(0.0090)	(0.0089)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0172***	0.0153***	0.0242***
	(0.0017)	(0.0022)	(0.0029)
$\mathbb{1}\{Absence_i = 1\}$	-0.0179***	-0.0222**	-0.0154
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.0059)	(0.0086)	(0.0128)
R^2	0.643	0.606	0.590
N	787,379	779,243	720,700

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 6.1.1. 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 6.1.2.
[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 6.1.1 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 3-12 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 0.025, 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 6: 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*
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0685***	0.0664***	0.0715***	0.0679***	0.0698***	0.0692***
	(0.0064)	(0.0082)	(0.0064)	(0.0082)	(0.0065)	(0.0082)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0042**	0.0036**	0.0107***	0.0038	0.0164***	0.0032
	(0.0017)	(0.0015)	(0.0030)	(0.0023)	(0.0040)	(0.0029)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	-0.0073**	-0.0051	0.0002	0.0013	0.0002	-0.0099
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.0030)	(0.0039)	(0.0060)	(0.0079)	(0.0075)	(0.0099)
R^2	0.654	0.243	0.639	0.232	0.631	0.228
N	922,030	922,030	846,126	846,126	787,889	787,889

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*
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0614***	0.0703***	0.0551***	0.0725***	0.0497***	0.0742***
	(0.0144)	(0.0156)	(0.0145)	(0.0157)	(0.0147)	(0.0157)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0186***	0.0019	0.0459***	0.0031	0.0669***	0.0051
	(0.0020)	(0.0016)	(0.0031)	(0.0029)	(0.0041)	(0.0040)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	-0.0028	-0.0015	0.0302***	-0.0092	0.0642***	-0.0157
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.0065)	(0.0086)	(0.0110)	(0.0134)	(0.0167)	(0.0170)
R^2	0.213	0.294	0.210	0.277	0.208	0.268
N	383,295	383,295	351,567	351,567	327,059	327,059

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*
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0495***	0.0442***	0.0527***	0.0465***	0.0500***	0.0482***
	(0.0070)	(0.0098)	(0.0070)	(0.0098)	(0.0071)	(0.0099)
$\mathbb{1}\{t \geq e_i + d_i\}$	-0.0067***	0.0043	-0.0152***	0.0036	-0.0206***	0.0012
	(0.0017)	(0.0033)	(0.0031)	(0.0055)	(0.0041)	(0.0071)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	-0.0029	-0.0080*	0.0040	0.0001	0.0012	-0.0114
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.0035)	(0.0047)	(0.0068)	(0.0093)	(0.0086)	(0.0122)
R^2	0.354	0.233	0.334	0.224	0.324	0.220
N	538,546	538,546	494,280	494,280	460,587	460,587

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 6.1.1.
[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 6.1.1 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 3-12 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 4.3.4.
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 shock), 2009-2017, *main sample*.

Table 7: 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	
	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$
$\tilde{\mathbb{1}}\{ACC_i = 1\,\&\,Absence_i = 1\}$	0.0147***	0.0016	0.0120***	-0.0009	0.0039	-0.0032**
	(0.0025)	(0.0012)	(0.0026)	(0.0012)	(0.003)	(0.0013)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0397***	0.0089***	0.0825***	0.0099***	0.1299***	0.0116***
	(0.0025)	(0.0003)	(0.0024)	(0.0004)	(0.0037)	(0.0003)
$\tilde{\mathbb{1}}\{ACC_i = 1\,\&\,Absence_i = 1\}$	-0.0487***	-0.0093***	-0.0186***	-0.0110***	-0.0208***	-0.0131***
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.005)	(0.0017)	(0.0041)	(0.0026)	(0.0054)	(0.0035)
R^2	0.831	0.877	0.8	0.808	0.771	0.757
N	183,198	186,064	194,169	194,761	195,240	195,752

Notes: [1] The underlying Ordinary Least Squares (OLS) model is presented in Section 6.2.
[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 6.1.1 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 3-12 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 -0.0065, with a standard deviation of 0.334.
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; *accident sample, exploiting random timing.*

Table 8: Within-Firm Matching Estimation Results

	(1)	(2)	(3)	(4)	(5)	(6)
	upon return		after 1 year		after 2 years	
	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$	$ln(w_i)$	$\hat{\phi}_j$
$A\hat{T}T$	-0.0506***	-0.0053***	-0.0229***	-0.0077***	-0.0202***	-0.0084***
	(0.0038)	(0.0013)	(0.0037)	(0.0023)	(0.0046)	(0.0028)
N	6,397,451	6,497,881	5,834,230	5,977,185	4,831,866	5,116,940
Matched Treated Observations	6,581	7,471	6,641	6,732	5,408	5,602
Unmatched Treated Observations	2,585	2,654	2,518	2,417	2,063	2,067

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 6.1.1 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 3-12 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 0.025, 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; *main sample with full set of potential controls.*

Table 9: Estimation Results of the Linear Difference-in-Difference Model (OLS) on Wage Income, Labor Supply (Hours Worked), and Alternative Measures of the Hourly Deflated Log-Wage

Panel A: Baseline	(1)	(2)	(3)	(4)	(5)	(6)
		$ln(w_{hour}) - baseline$			*weekly hours worked*	
	upon return	+1 year	2 years	upon return	+1 year	2 years
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0157***	0.0176***	0.0181***	0.1835***	0.1720***	0.1726***
	(0.0017)	(0.0020)	(0.0020)	(0.0464)	(0.0464)	(0.0457)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0433***	0.0900***	0.1355***	0.1138***	0.0986***	0.1042***
	(0.0032)	(0.0056)	(0.0076)	(0.0117)	(0.0206)	(0.0289)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	-0.0503***	-0.0213***	-0.0278***	-0.1983***	-0.2243***	-0.2019***
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.0037)	(0.0037)	(0.0045)	(0.0404)	(0.0529)	(0.0618)
R^2	0.872	0.861	0.841	0.220	0.219	0.210
N	909,387	893,919	824,247	918,388	898,111	828,735

Panel B: Other Wage Measures	(1)	(2)	(3)	(4)	(5)	(6)
		$ln(w_{month})$		$ln(w_{hour})$ - *using monthly days insured*		
	upon return	+1 year	2 years	upon return	+1 year	2 years
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0203***	0.0208***	0.0209***	0.0158***	0.0177***	0.0184***
	(0.0023)	(0.0025)	(0.0026)	(0.0017)	(0.0019)	(0.0020)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0465***	0.0900***	0.1332***	0.0439***	0.0919***	0.1372***
	(0.0034)	(0.0061)	(0.0082)	(0.0032)	(0.0056)	(0.0075)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	-0.0643***	-0.0260***	-0.0302***	-0.0352***	-0.0194***	-0.0262***
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.0036)	(0.0045)	(0.0054)	(0.0035)	(0.0036)	(0.0046)
R^2	0.826	0.812	0.791	0.875	0.863	0.843
N	917,164	898,518	829,811	909,387	893,919	824,247

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
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0215***	0.0220***	0.0222***	0.0218***	0.0224***	0.0228***
	(0.0024)	(0.0026)	(0.0026)	(0.0023)	(0.0025)	(0.0025)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0474***	0.0924***	0.1362***	0.0472***	0.0926***	0.1362***
	(0.0034)	(0.0060)	(0.0081)	(0.0034)	(0.0060)	(0.0081)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	-0.0415***	-0.0244***	-0.0289***	-0.0393***	-0.0254***	-0.0294***
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(0.0037)	(0.0044)	(0.0056)	(0.0035)	(0.0043)	(0.0055)
R^2	0.829	0.813	0.793	0.830	0.814	0.793
N	910,303	895,926	826,971	910,269	895,880	826,921

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 6.1.1.
[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 6.1.1 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 3-12 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: $\log(w_{hour})$ denotes the hourly deflated log-wage. The baseline version is the same as $ln(w_i)$ in Table 5. $\log(w_{month})$ 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 10: 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
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(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
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(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
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(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
$\times\mathbb{1}\{t \geq e_i + d_i\}$	(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 6.1.1.
[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 6.1.1 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 absent 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 0.025, 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*.

Table 11: 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*
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$	0.0016***	-0.0002	0.0014***	-0.0004	0.0014***	-0.0002
	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0003)
$\mathbb{1}\{t \geq e_i + d_i\}$	0.0007***	0.0007***	0.0014***	0.0013***	0.0015***	0.0015***
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0002)
$\mathbb{1}\{ACC_i = 1 \,\&\, Absence_i = 1\}$ $\times\mathbb{1}\{t \geq e_i + d_i\}$	-0.0011***	-0.0008**	0.0004	0.0000	0.0003	0.0000
	(0.0004)	(0.0004)	(0.0006)	(0.0006)	(0.0008)	(0.0008)
R^2	0.820	0.897	0.764	0.842	0.746	0.823
N	878,218	878,218	864,972	864,972	797,567	797,567

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 6.1.1, 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 6.1.1 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 3-12 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 (20a)). The baseline average of this occupation fixed effect at the control group is -0.010, 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 (20b)). The baseline average of this occupation fixed effect at the control group is -0.009, 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*.

Appendix

A Proofs

Proof of Proposition 1.

Consider a wage in the previous period $w_{-1}(y', y) = g(y', y)$ dependent on the previous period employer y and the previous period outside option y'.

$$E_j[w_j|w_{-1}(y', y)] = \psi \int_y^\infty g(y, x)dF(x) + \psi \int_{y'}^{y} g(x, y)dF(x) + \psi \int_0^{y'} w_{-1}(y', y)dF(x)$$

$$+ (1 - \psi)w_{-1}(y', y) > w_{-1}(y', y), \forall y, y' \quad (36)$$

where the inequality follows from

$$g(y', x) = w(y', x) > w_{-1}(y', y), \forall x > y \quad \text{and} \quad g(x, y) = w(x, y) > w_{-1}(y', y), \forall y \geq x \geq y' \quad (37)$$

Hence, it follows that

$$E_j[w_j|w_{j-1}, \text{no accident}] > w_{j-1}, \forall w_{j-1}. \quad (38)$$

Now using the law of iterated expectations, we obtain

$$E_0[E_j[w_j|w_{j-1}]|\text{no accident}] = E_0[w_j|\text{no accident}]$$

$$> E_0[w_{j-1}|\text{no accident}], \forall w_{j-1}. \quad (39)$$

■

Proof of Proposition 2.

Consider the case of an individual that was paid a wage $w_{-1}(y', y_{-1})$ prior to the health shock. First, let's show that the employer productivity of the individual upon recovery is lower than it would have been otherwise

$$E_T[y_{T+j}|\text{accident at } T, y_{-1}] = \psi \int_{y_{-1}}^\infty x dF(x) + \psi \int_0^{y_{-1}} y_{-1} dF(x) + (1 - \psi)y_{-1}$$

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

$$< E_T[y_{T+3}|\text{no accident}, y_{-1}] < ... < E_T[y_{T+j}|\text{no accident}, y_{-1}], \forall y_{-1}. \quad (40)$$

Hence, it follows that

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

Now using the law of iterated expectations

$$E_0[y_{T+j}|\text{accident at } T] < E_0[y_{T+j}|\text{no accident}]. \quad (42)$$

Next, let us consider wages, conditional on not being laid-off.

$$E_T[w_{T+j}|\text{accident at } T, w_{-1}(y',y)] = \psi\int_y^\infty g(y,x)dF(x) + \psi\int_{y'}^{y} g(x,y)dF(x) + \psi\int_0^{y'} w_{-1}(y',y)dF(x)$$

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

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

$$< E_T[w_{T+3}|\text{no accident}, w_{-1}(y',y)] < ... < E_T[w_{T+j}|\text{no accident}, w_{-1}(y',y)], \forall w_{-1}(y',y). \quad (43)$$

Hence,

$$E_T[w_{T+j}|\text{accident at } T] < E_T[w_{T+j}|\text{no accident}] \quad (44)$$

Now using the law of iterated expectations

$$E_0[w_{T+j}|\text{accident at } T] = E_0[w_{T+1}|\text{no accident}] < E_0[w_{T+j}|\text{no accident}], \forall j. \quad (45)$$

■

Proof of Proposition 3.

$$E_0[w_{T+j}|\text{accident at } T, w_{-1}(y',y)] = \psi\int_y^\infty g(y,x)dF(x) + \psi\int_{y'}^{y} g(x,y)dF(x)$$

$$+ \psi\int_0^{y'} w_{-1}(y',y)dF(x) + (1-\psi)w_{-1}(y',y) > w_{-1}(y',y), \forall w_{-1}(y',y). \quad (46)$$

■

Proof of Proposition 4.

Recall that in our model the wage of an individual currently working for a firm of type y with outside option y' is given by

$$w(y',y) = g(y',y) \quad \text{with} \quad \frac{\partial g(y',y)}{\partial y'} > 0 \quad \text{and} \quad \frac{\partial g(y',y)}{\partial y} > 0 \quad (47)$$

Taking logs gives us

$$\log(w) = \log(g(y',y)). \quad (48)$$

Now take a first-order Taylor approximation to write $\log(w)$ as a function of $\log(y)$ and $\log(y')$ around the point $[\log(\overline{y}), \log(y^*)]$, which delivers

$$\log(w) = \log(w(\overline{y},y^*)) + \frac{\partial g(y',y)}{\partial y'}|_{y'=\overline{y},y=y^*}\overline{y}(\log(y') - \log(\overline{y})) + \frac{\partial g(y',y)}{\partial y}|_{y'=\overline{y},y=y^*}y^*(\log(y) - \log(y^*)) \quad (49)$$

where we have used the fact that

$$\frac{\partial f(x)}{\partial \log(x)} = \frac{\partial f(x)}{\partial x}\frac{\partial x}{\partial \log(x)} = \frac{\partial f(x)}{\partial x}x. \tag{50}$$

Next, note that

$$\Rightarrow \log(w) = \gamma_0 + \gamma_1 \log(y) + \gamma_2 \log(y') \tag{51}$$

where

$$\gamma_0 = \log(w(\overline{y}, y^*)) - \frac{\partial g(y', y)}{\partial y'}|_{y'=\overline{y}, y=y^*}\overline{y}\log(\overline{y}) - \frac{\partial g(y', y)}{\partial y}|_{y'=\overline{y}, y=y^*}y^*\log(y^*), \tag{52}$$

$$\gamma_1 = \frac{\partial g(y', y)}{\partial y}|_{y'=\overline{y}, y=y^*}y^* > 0, \tag{53}$$

and

$$\gamma_2 = \frac{\partial g(y', y)}{\partial y'}|_{y'=\overline{y}, y=y^*}\overline{y} > 0. \tag{54}$$

Define $\epsilon \equiv \gamma_2 \log(y')$ to obtain the first equation in the proposition in the text

$$\log(w) = \gamma_0 + \gamma_1 \log(y) + \epsilon. \tag{55}$$

Next, note that $\gamma_1 \log(y)$ is the wage premium an individual receives from working in their current employer y. In other words, it captures the part of the wage exclusively due to the current employer of the individual. As a result, this is the part of the wage that changes when individuals move to better, higher-paying, employers. In contrast, ϵ captures the part of the wage that makes individuals within a same firm (i.e, same y) get paid different amounts. In our model this is captured by different outside options, y', that different individuals with a same firm possess. In reality, it is more general, it captures any component of the wage that leads to individuals working for a same firm being paid differently.

Now define $\Delta \log(x)$ as the difference in $\log(x)$ between the actual and the counterfactual value of (any variable) x for the treated, due to the accident-induced absence. Then,

$$\Delta \log(w) = \gamma_1 \Delta \log(y) + \Delta\epsilon. \tag{56}$$

Since $\gamma_1 \log(y)$ captures the part of the wage that changes when individuals move to better, higher paying, employers, $\Delta \log(y)$ captures the difference between the actual and the counterfactual log-wages for the treated, due to the accident-induced absence, which stems from missed opportunities to move to better, higher-paying, employers. Similarly, since ϵ captures the wage component that leads to different individuals in a same firm being paid differently, $\Delta\epsilon$ captures the difference between the actual and the counterfactual log-wages for the treated, due to the accident-induced absence, which stems from missed opportunities to climb the wage ladder at a given employer ("within-employer"). Define $\Delta w_{\text{within}} \equiv \Delta\epsilon$ to get the desired result:

$$\Delta \log(w) = \gamma_1 \Delta \log(y) + \Delta w_{\text{within}}. \tag{57}$$

■

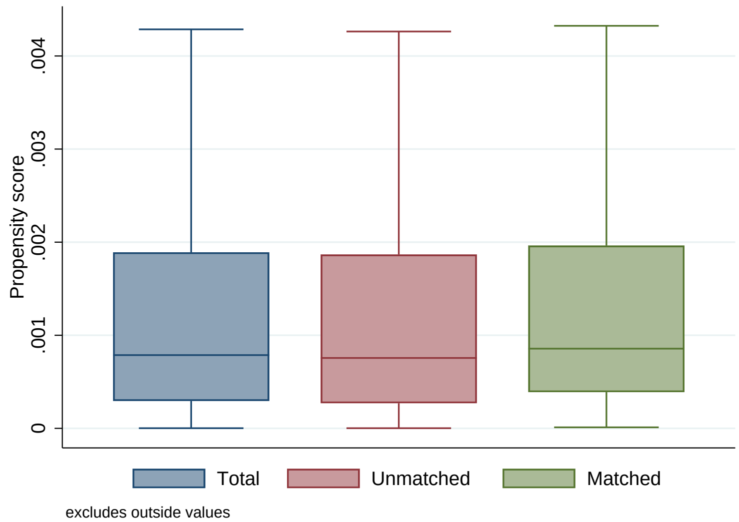
B Within-Firm Matching

Table B1: Balance Statistics

	Raw			Matched		
	Treated	Control	StdDif	Treated	Control	StdDif
Age						
20	0.011	0.004	0.074	0.010	0.007	0.036
21	0.014	0.008	0.054	0.014	0.013	0.014
22	0.018	0.012	0.053	0.018	0.017	0.013
23	0.023	0.015	0.062	0.022	0.021	0.002
24	0.024	0.016	0.055	0.024	0.023	0.004
25	0.024	0.018	0.046	0.023	0.024	-0.006
26	0.025	0.018	0.048	0.026	0.027	-0.004
27	0.023	0.019	0.033	0.024	0.026	-0.015
28	0.027	0.019	0.056	0.027	0.030	-0.020
29	0.027	0.019	0.052	0.026	0.029	-0.021
30	0.027	0.020	0.049	0.027	0.031	-0.023
31	0.029	0.021	0.048	0.029	0.034	-0.029
32	0.032	0.023	0.058	0.032	0.034	-0.013
33	0.032	0.025	0.042	0.033	0.036	-0.014
34	0.032	0.027	0.033	0.031	0.037	-0.039
35	0.039	0.028	0.059	0.039	0.040	-0.003
36	0.037	0.030	0.039	0.037	0.040	-0.018
37	0.040	0.031	0.049	0.040	0.043	-0.011
38	0.037	0.032	0.028	0.039	0.045	-0.034
39	0.041	0.033	0.039	0.040	0.040	0.003
40	0.042	0.034	0.041	0.042	0.042	-0.001
41	0.042	0.034	0.042	0.041	0.041	0.001
42	0.039	0.034	0.028	0.039	0.040	-0.008
43	0.040	0.033	0.040	0.040	0.038	0.014
44	0.037	0.032	0.025	0.035	0.037	-0.011
45	0.037	0.032	0.028	0.037	0.036	0.007
46	0.039	0.031	0.044	0.038	0.036	0.011
47	0.038	0.030	0.045	0.040	0.036	0.024
48	0.041	0.029	0.062	0.043	0.033	0.053
49	0.040	0.029	0.060	0.040	0.032	0.046
50	0.042	0.028	0.078	0.043	0.033	0.053
Gender	0.779	0.555	0.490	0.771	0.771	0.000
Occupation						
Manager	0.054	0.057	-0.013	0.030	0.033	-0.011
Professional	0.038	0.073	-0.155	0.035	0.045	-0.044
Other white collar	0.129	0.255	-0.326	0.121	0.149	-0.072
Skilled blue collar	0.380	0.322	0.123	0.376	0.357	0.040
Assembler, machine operator	0.222	0.188	0.086	0.263	0.258	0.013
Unskilled	0.177	0.105	0.207	0.174	0.158	0.046
Log mean wage 4-12 m before	6.337	6.632	-0.579	6.430	6.432	-0.004
Log health spending 3-12 m before	6.656	6.971	-0.073	6.694	6.184	0.118
Any drug spending 3-12 m before						
Alimentary tract, metabolism (ATC A)	0.120	0.151	-0.089	0.120	0.109	0.033
Blood, blood forming organs (ATC B)	0.050	0.061	-0.048	0.048	0.033	0.069
Cardiovascular (ATC C)	0.150	0.238	-0.223	0.159	0.147	0.030
Dermatologicals (ATC D)	0.071	0.082	-0.045	0.072	0.067	0.018
Genito-urinary system (ATC G)	0.021	0.036	-0.086	0.020	0.022	-0.010
Hormonal preparations (ATC H)	0.031	0.037	-0.036	0.030	0.024	0.033
Antiinfectives (ATC J)	0.280	0.286	-0.013	0.288	0.267	0.047
Antineoplastic, immudomodulating (ATC L)	0.002	0.004	-0.035	0.002	0.003	-0.013
Musculoskeletal system (ATC M)	0.164	0.168	-0.012	0.164	0.137	0.073
Nervous system (ATC N)	0.059	0.064	-0.019	0.059	0.048	0.046
Antiparasatic producucts (ATC P)	0.008	0.010	-0.022	0.007	0.007	-0.004
Respiratory system (ATC R)	0.106	0.118	-0.036	0.107	0.098	0.029
Sensory organs (ATC S)	0.046	0.046	0.003	0.045	0.043	0.009
Various (ATC V)	0.001	0.002	-0.012	0.001	0.001	0.003

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

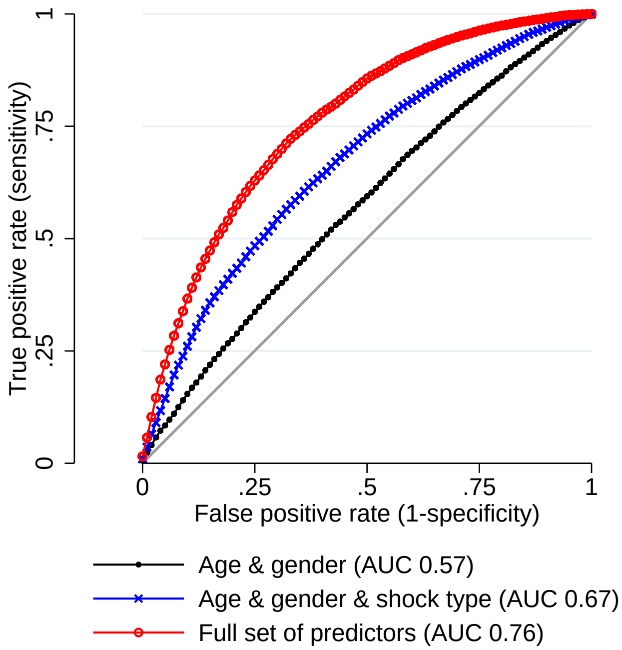
Figure B1: Common Support



Note: 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.

C Further Figures

Figure C1: ROC Curve of Absence Prediction After the Accident



Note: 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 (22).
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.

D Further Tables

Table D1: Distribution of and Average Log-Wage Changes of Treated and Control Individuals, By the Type of Switches (Firm- and/or Occupation-Switches), Relative to Before the Event

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Shares (%)	upon return		1 year after		2 years after	
	control	treated	control	treated	control	treated
same firm	87.54	87.36	71.62	70.4	61.8	59.49
new firm	12.46	12.64	28.38	29.6	38.2	40.51
same occupation	91.64	90.92	80.01	79.38	72.44	71.21
new occupation	8.36	9.08	19.99	20.62	27.56	28.79
same firm, same occup.	86.47	84.45	68.13	66.82	57.54	54.69
same firm, new occup.	2.99	2.14	6.41	5.89	8.34	8.09
new firm, same occup.	5.82	5.92	13.02	13.27	16.5	17.76
new firm, new occup.	4.72	7.49	12.44	14.02	17.62	19.47
same firm, same occup., wage loss	29.19	29.87	19.05	19.75	12.88	12.79
same firm, new occup., wage loss	1.13	0.94	2.14	1.93	2.36	2.42
new firm, same occup., wage loss	2.22	3.07	4.2	4.73	4.58	5.57
new firm, new occup., wage loss	1.98	4.2	4.69	5.84	5.92	7.21
same firm, same occup., wage gain	47.9	38.35	49.08	47.08	44.67	41.9
same firm, new occup., wage gain	1.79	1.1	4.27	3.96	5.98	5.67
new firm, same occup., wage gain	3.52	2.8	8.81	8.54	11.92	12.19
new firm, new occup., wage gain	2.72	3.26	7.75	8.18	11.7	12.26

Panel B	(1)	(2)	(3)	(4)	(5)	(6)
Avg. Change in Log-Wage	upon return		1 year after		2 years after	
	control	treated	control	treated	control	treated
same firm	0.0438	0.0099	0.0894	0.0714	0.1329	0.1155
new firm	0.0537	-0.1000	0.0977	0.0734	0.1447	0.1112
same occupation	0.0437	0.0031	0.0908	0.0714	0.1357	0.1171
new occupation	0.0581	-0.0785	0.0947	0.0741	0.1403	0.1055
same firm, same occup.	0.0432	0.0090	0.0892	0.0713	0.1336	0.1147
same firm, new occup.	0.0604	0.0456	0.0911	0.0727	0.1283	0.1211
new firm, same occup.	0.0512	-0.0824	0.0988	0.0720	0.1432	0.1246
new firm, new occup.	0.0566	-0.1139	0.0965	0.0748	0.1460	0.0990
same firm, same occup., wage loss	-0.1070	-0.1448	-0.1081	-0.1037	-0.1263	-0.1151
same firm, new occup., wage loss	-0.1177	-0.1665	-0.1172	-0.1253	-0.1389	-0.1093
new firm, same occup., wage loss	-0.2831	-0.3803	-0.2621	-0.2678	-0.2931	-0.2925
new firm, new occup., wage loss	-0.3967	-0.5058	-0.3618	-0.3501	-0.3870	-0.3861
same firm, same occup., wage gain	0.1431	0.1327	0.1658	0.1446	0.2085	0.1848
same firm, new occup., wage gain	0.1758	0.2314	0.1953	0.1693	0.2335	0.2195
new firm, same occup., wage gain	0.2628	0.2430	0.2710	0.2602	0.3107	0.3149
new firm, new occup., wage gain	0.3870	0.3903	0.3737	0.3779	0.4155	0.3843

Notes: [1] Panel A shows the distribution of treated and control individuals by (i) whether they switched to a new firm, (ii) whether they switched to a new occupation, (iii) whether they switched to a new firm and a new occupation, and (iv) whether they switched to new firm and a new occupation and experienced a wage loss, upon return/1 year later/2 years later.

[2] Panel B shows the average change in log-wages in the aforementioned cells.

[3] The sample contains only those individuals whose (i) firm identifier, (ii) occupation identifier, (iii) firm identifier and occupation identifier, (iv) firm identifier and occupation identifier and log-wage, can be observed both in the pre-period and upon return/1 year later/2 years later. The shares (in percent) of the original sample among the treated who can be included are: upon return: (i) 100, (ii) 99.9, (iii) 85, (iv) 85; 1 year after: (i) 90, (ii) 89, (iii) 85, (iv) 85; 2 years after: (i) 76, (ii) 74, (iii) 70, (iv) 70. The shares (in percent) of the original sample among the controls who can be included are: upon return: (i) 88, (ii) 86, (iii) 80, (iv) 80; 1 year after: (i) 85, (ii) 83, (iii) 77, (iv) 77; 2 years after: (i) 72, (ii) 70, (iii) 64, (iv) 64.

[4] Upon return, the shares of individuals who return with the same wage are 9.54 and 16.4 percent among the control and among the treated individuals, respectively, and they are not depicted in the table.

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.*