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THREE ESSAYS ON THE RELATIONSHIP BETWEEN HEALTH AND LABOR MARKET OUTCOMES

by

Kinga Marczell

Submitted in partial fulfillment of the requirements for
the degree of Doctor of Philosophy at
Central European University

Supervisor: Botond Kőszegi

Budapest, Hungary

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CENTRAL EUROPEAN UNIVERSITY
DEPARTMENT OF ECONOMICS AND BUSINESS

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I certify that I have read this dissertation and in my opinion it is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.
Advisor:

Botond Koszegi

I certify that I have read this dissertation and in my opinion it is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.
Internal Examiner:

Sergey Lychagin

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External Examiner:

Martin Halla

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Marton Csillag

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Author: Kinga Marczell
Title: Three Essays on the Relationship Between Health and Labor Market Outcomes
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DISCLOSURE OF CO-AUTHORS CONTRIBUTION

Title of paper: The Effect of Managers' Health Shocks on Employment Practices

Co-authors: Gergely Hajdu

The nature of the cooperation and the roles of the individual co-authors and approximate share of each co-author in the joint work: The paper was developed in close cooperation with Gergely Hajdu throughout all stages. We designed both the main question and the identification strategy of the paper together. We shared the tasks during the literature review, programming, regression analysis and writing as well.

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Abstract

This thesis encompasses three empirical studies from the intersection of labor economics, health economics and behavioral economics. The first two chapters are related to sick benefit schemes. Chapter 1 uses a legislative change in the sick benefit replacement rate to identify the effect of the level of sickness insurance on take-up, and the effect of sick benefit take-up on employees' and their colleagues' prospective health outcomes. Chapter 2 documents the empirical observation, that pregnant women spend less time on sick leave when their supervisor is a parent – a suggestive evidence for parent supervisors providing working conditions to mother-to-bes that incentivize them to keep working longer. The third chapter, co-authored with Gergely Hajdu, investigates the effect of a health shock on managers employment outcomes and employment practices.

Chapter 1: The Real Price of Saving on Sickness Benefits: Effects on Employees' and their Colleagues' Health Outcomes

Using a Hungarian linked employer-employee dataset containing health information, and a change in the sickness benefit scheme, I estimate the elasticity of sick leave take-up with respect to the benefit replacement rate. Using this as an instrument, I quantify the effect of sickness insurance on individuals' health outcomes, and on their co-workers' health outcomes. According to my results, benefit take-up decreased significantly as a result of the administrative decrease in replacement rates. The elasticity of sickness benefit take-up with respect to the replacement rate is 1.3 around the legislation change used for identification. However, I find no evidence for this decrease in sick benefit take-up raising prospective health expenditures, meaning that there is no evidence for an increase in presenteeism for the average worker. I find colleagues' health outcomes to be unaffected as well.

Chapter 2: You Make Me Sick: The Link Between Mother-to-bes' Sick Benefit Take-up and Their Employers' Parental Status

Using a linked employer-employee dataset, I show that pregnant women spend 11 days less on sick leave during their pregnancy if their superior is a parent. I interpret this result as a suggestive evidence of two phenomena. First, the length of sickness leave before giving birth is not entirely decided by health related factors, as it should be the case according to the legislation, but is, instead, heavily influenced by the mother-to-be's and her employer's

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decisions. Second, parent supervisors provide working conditions to mother-to-bes that incentivize them to keep working longer. To ensure that the results are not driven by a self-selection issue of employees with a tendency for higher sick leave take-up avoiding parent leaders, I carry out a placebo test, and find that the parental status of leaders has no effect on the sick leave take-up of male employees.

Chapter 3: The Effect of Managers' Health Shocks on Employment Practices

Co-author: Gergely Hajdu

We investigate the effect of managers' health shocks on the separation rate of their employees. Our hypothesis is that previous illness experience of division leaders at a company may affect their attitudes towards employees. To test our hypothesis, we measure changes in the separation rate of employees assigned to managers before and after the managers' illness episodes. Our results show that employee separation rate increases in the manager's employee pool after the manager's illness by 8%, a phenomenon mostly driven by an increase in the number of dismissals, as opposed to an increase in the number of employees leaving the firm voluntarily. We provide a descriptive analysis of managers' own employment outcomes as well. We find that adverse employment effects are present even four years after the illness episode. While 18.23% of previously ill managers has no job by this time, the corresponding ratio is only 13.02% for the control group, and this difference is almost entirely coming from the difference in their likelihood of having a manager position. Conditional on staying at the firm, managers' wage decreases by 13.4% following the year of illness, compared to the wage evolution of their matched healthy counterparts.

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Chapter 1

The Real Price of Saving on Sickness Benefits: Effects on Employees' and their Colleagues' Health Outcomes

1.1 Introduction and motivation

In most industrialized countries – and in virtually every European country – governments provide workers with a certain level of public insurance against the income loss associated with medical incapacity for work. Besides the welfare increase arising from risk sharing, justifications for sick leave policies involve the costs associated with *presenteeism*, that is, the practice of choosing to work while ill, in order to avoid the loss of earnings. This practice may result in the deterioration of the employee's health condition, imposing costs on both the individual and the public health care system. In addition to this, in case of contagious diseases, presenteeism causes negative externalities for co-workers and, in professions involving closer customer relationship, for customers as well. When designing the optimal level of insurance, policy makers must weigh these factors against the distortionary costs arising from a moral hazard problem: as sickness is a private information (or at least its monitoring is incomplete), some people may engage in *absenteeism*, meaning that they use sick leave for shirking.

These concepts are theoretically well understood, and are often invoked by policy makers, but empirical evidence of actual benefit schemes generating absenteeism, or influencing health outcomes by preventing presenteeism is scarce and mixed. We already have an understanding about the extent to which the replacement rate of sickness benefits influences take-up (e.g. Böckerman et al. (2014), Csillag (2016), among many others), but evidence about the effect on individual health outcomes is scarce and mixed (Puhani and Sonderhof (2010), Halla et al. (2017), Ziebarth and Karlsson (2014), Callison and Pesko (2016)). Concerning externalities affecting others' health, Barmby and Larguem (2009) find evidence of colleagues' sickness affecting one's absence probability using personnel data from a single firm, and Pichler and Ziebarth (2017) find evidence of sick leave take-up influencing influenza-like disease rates based on Google Flu data at the city level.

In this paper, I use an episode of exogenous replacement rate decrease in the Hungarian sick benefit system in 2009 to estimate the elasticity of sick benefit take-up. I find that benefit take-up decreased significantly as a result of the administrative decrease in replacement rates, and the elasticity of sickness benefit take-up with respect to the replacement rate is 1.3 around the legislation change used for identification. This reaction may be considered rather strong, and it is remarkably close to the one found by Böckerman et al. (2014) on Finnish data. In the spirit of Halla et al. (2017), I explore the effects of this replacement rate change, and the resulting decrease in take-up, on individual health outcomes proxied by inpatient, outpatient and medication expenditures, in order to determine, whether or not the average insuree is in the domain of presenteeism or absenteeism. According to their model, a negative relationship between sick benefit take-up and prospective health care costs is associated with the domain of presenteeism, reflecting the idea, that sick individuals going to work instead of resting take more time to recover, and may even be more likely to develop complications and get ill later on. I find no evidence of an adverse health effect of this policy induced decrease in the number of sick days, suggesting that the average worker in the database is not in the domain of presenteeism. Concerning the impact on colleagues' health outcomes, the data provides no strong evidence for decreased take-up having an adverse external effect on others through increased infection rates.

I aim to contribute to the literature in three ways. First, as evidence about the health effects of sickness insurance schemes is scarce, and based on data from only a handful of countries, I aim to increase our understanding of these phenomena by complementing this knowledge with experiences from a post-socialist country. Second, by using linked employer-employee level data complemented with various aspects of health outcomes, I am able to study the effect of sick leave take-up on co-workers' health outcomes directly, without having to resort to aggregated, state or city level health outcome measures. Also, having access to various variables describing healthcare system utilization, I can distinguish between serious conditions requiring hospital stay, and ordinary sickness episodes involving

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no inpatient costs. This allows me to restrict my attention to episodes that are more likely to correspond to contagious diseases.

The remainder of the paper is organized as follows. I describe the Hungarian sick leave insurance system in Section 1.2, and the dataset in Section 1.3. Identification strategy is discussed in Section 1.4; in Subsection 1.4.1 for the elasticity estimations, and in Subsection 1.4.2 for the estimations of the effect of sick leave take-up on individuals' and their colleagues' health outcomes. I explain necessary sample restrictions, define variables and provide descriptive statistics in Section 1.5. Results are presented in Section 1.6, and gender heterogeneity in results are covered in Subsection 1.6.3. Section 1.7 concludes.

1.2 Hungarian sick leave insurance system

The Hungarian sickness benefit scheme is universal in the sense that all employees are covered by the statutory health insurance scheme, making them eligible to a certain level of sickness insurance. Individuals receiving pensions, while working at the same time, constitute an exception under this rule, as they are not eligible for sick leave. Health insurance contributions are independent from individual risks. Medical incapacity to work has to be certified by the insuree's general practitioner. Parents of children under 12 years are also entitled to sick benefit when the child is in hospital and the parent is there with him, or when the child is cared for at home. The sickness benefit system comprises two phases. First, enrollees have access to a maximum of 15 days of firm-financed short-term sick leave annually, during which time their employer pays them 70% of their wage. After having exhausted this 15 day limit, employees become eligible to long-term sickness leave, which is co-financed by the state (2/3) and the employer (1/3). The maximum length, the basis and the replacement rate of long-term sick benefit depend on the employee's previous work history.

- The maximum length of a sickness spell is 365 days, if the insuree has been insured continuously for at least one year prior to the start of the sickness leave. In this context, continuous insurance time means that the insurance period has no gaps of 30 or more days length, gaps meaning periods of no or suspended insurance, that is, unpaid leave leave, unlicensed absences from work, unemployment, incarceration. For individuals not meeting this requirement, the maximum length of the sickness spell is equivalent to the length of their current continuous insurance spell. When assessing the maximum sick leave length one is entitled for, one has to substract the

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number of sick days already taken during the given calendar year.

- The basis of the benefit is, by default, the daily average wage income of the individual during the calendar year directly preceding the sickness leave. For those who do not have at least 180 income earning days during this period, the basis is calculated based on another reference period. This reference period has to be at least 180 days long, and continuous in the sense, that the insuree has had earnings throughout the entire period, without any gaps of 30 or more days. This reference period has to be chosen to be the latest one among those between the first day of the calendar year preceding the sickness spell, and the last day before the sickness spell starts. For employees without such an employment spell, the benefit base is defined by the statutory minimum wages. These employees are excluded from the analysis.

- Before August 2009, the benefit rate was 70% for employees with at least two years of uninterrupted insurance time. For those with lower insurance time, and those resorting to inpatient care, the benefit rate was 60%.

As of August 2009, the replacement rate of sickness benefit decreased from 70% to 60% of the insuree's income for employees with at least two years of uninterrupted insurance time. For those with lower insurance time, and those resorting to inpatient care, the ratio of the benefit and the wage income decreased from 60% to 50% at the same time. As a new element, the legislation also applied a cap on the amount of sickness benefit. The value of this benefit ceiling was equivalent to 400% of the minimum wage (150% for those who already have lost their job). The benefit scheme was further restriced in May 2011, when the cap was decreased to 200% of the minimum wage, and the sick pay eligibility of those who have already lost their jobs was discontinued.[1] Figure A.1 illustrates the changes by showing the amount of benefit as a function of the benefit base.[2]

1.3 Data

I use a large, longitudinal dataset linking administrative data from the Hungarian National Pension Insurance, the National Tax and Customs Administration, and the National Health

[1]For an analysis of the effects of the 2011 legislation change, see Csillag (2016).

[2]Though it is not shown on the graph, but it is worth mentioning that the nominal value of the benefit ceiling – as it is expressed as a function of the minimum wage in the law – was slightly altered two more times during the time period in question, alongside with the increase in nominal wage, as of the 1st January 2010, and as of the 1st January 2011, by 2.8% and 6.1%, respectively.

Insurance Fund, originally compiled for the Centre for Economic and Regional Studies of the Hungarian Academy of Sciences. The dataset contains a 50% random sample of Hungarian citizens of age 15-73 in 2003, covering the period 2003-2011.

The database contains information about the date of birth, gender, and the 2003 region of residence of individuals, alongside with monthly information about their employment status, labor income, and occupation code (ISCO). For people having multiple jobs at the same time, wage income is recorded separately for each employment relationship. I also observe an identifier of the company or companies individuals work at, allowing me to identify coworkers. Firm level information in the dataset include the number of employees, four digit NACE codes, pre-tax profits, and export. Individual level in- and outpatient care and medication costs are recorded separately.

Information on the number of long term sick-leave days is available at the exact spell level, while short-term sick leave — as it is entirely financed by firms — is not recorded. Benefit levels, replacement rates, and the maximum number of sick days one is eligible for, are calculated using employment and income history for each individual, for each day, regardless of whether or not he was on sick leave on that particular day.

1.4 Identification

1.4.1 Estimating the elasticity of benefit take-up

I estimate the elasticity of sick benefit take-up with respect to the replacement rate by regressing the number of sick days on the replacement rate, i.e. on the ratio between the benefit level and the income.

$$days_{i,t} = \alpha + \gamma rr_{i,t} + \beta X_{i,t} + \delta H_{i,t-1} + \zeta m_t + \phi_i + \epsilon_{i,t} \tag{1.1}$$

In Equation 1.1, $rr_{i,t}$ denotes the monthly average replacement rate corresponding to individual i in month t. Control variables $X_{i,t}$ include age, age squared, the previous three months' earnings, the length of uninterrupted insurance time, two-digit ISCO occupation dummies, and firm level controls: two-digit NACE sector dummies, firm size, export ratio and a profitability dummy. I also control for lagged health status by including the logged inpatient, outpatient and medication costs of the individual during the previous calendar year, $H_{i,t-1}$. Month dummies m_t control for within year seasonal patterns, and ϕ_i denotes the individual fixed effects. Time fixed effects are not included, as the over time variation

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in $rr_{i,t}$ and $days_{i,t}$ induced by the legislation change is used for identifying the coefficient of primary interest, γ.

To gain a better understanding of this identification method, one needs to consider all the possible sources of variation in the replacement rate. As described above, before August 2009, replacement rates take on the value of 60 or 70% for each individual, depending on previous work history. As of August 2009, this decreases to 50 and 60%, respectively, and gradually decreases further as a function of income for those above the benefit ceiling. As of May 2011, the benefit cap was decreased, resulting in another exogenous decrease of the replacement rate for individuals whose calculated benefit levels are in-between the previous and the new benefit caps. Since the two legislation changes affected different income groups, and also because I only use the first one for identification in my IV-estimations, it is worth estimating the elasticity separately for the 2007 – April 2011 and the August 2009 – December 2011 periods. While these two periods overlap, the first one only includes the 2009 legislation change, and the second one only includes the 2011 legislation change. Although it is much smaller in effect, but the two nominal cap increases — associated with increases in the minimum wage — generated some exogenous variance in the replacement rate as well.

Although the most important source of identifying variation in the replacement rate are the legislation changes, this is complemented by two other sources. As the replacement rate is a function of the individual's previous work history, it may change over time for individual reasons as well: when someone accumulates two years of uninterrupted insurance time, he moves from the 50% replacement rate bracket to the 60% replacement rate bracket, or the other way round, whenever he interrupts his insurance spell for more than 30 days. Also, one may get above/below the benefit ceiling over time by increasing/decreasing his revenue. As this source of variation in the replacement rate may be related to unobserved individual characteristics that may also directly affect sick benefit take-up, I carry out the analysis for restricted samples as well. After excluding those, whose income rises above or sinks below the benefit ceiling over the time horizon, I chose to explore two ways of eliminating the variation coming from moving between the two replacement rate brackets. First, I exclude everyone, who switches between the brackets over time. The disadvantage of this method is the huge loss in sample size, and, presumably, in representativeness. Thus, in order to get a comprehensive picture of the effect I am after, I also estimate the models without leaving out bracket switchers, but, instead, splitting the observations disjunctly between the high and the low replacement rate groups. That is, I use only observations

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falling into the low replacement rate bracket in one estimation, and only the ones falling into the high replacement rate bracket in another. The estimated effects are, in either case, local average treatment effects.

1.4.2 Estimating the effect on own and on colleagues' health outcomes

As health cost variables are only available at a yearly frequency, these estimations are carried out in a cross-sectional form. I use the comparison between time periods August - December 2008 and August - December 2009 to measure the change in the replacement rate, and the resulting change in sick leave take-up. This allows me to regard the difference between the 2008 and 2010 health costs as a proxy for the change in individuals' health status.

In the following model, I estimate how the change in the number of sick days induced by the legislation change affected the evolution of individuals' health costs on the medium run.

$$\Delta health_i = \alpha + \beta \Delta days_i + \gamma X_i + \epsilon_i \tag{1.2}$$

In Equation 1.2, the dependent variable, $\Delta health_i$, is the change in logged health costs from 2008 to 2010, and $\Delta days_i$ is the change in sick benefit take-up from the period August - December 2008 to the period August - December 2009. $X_{i,T}$ contains the same control variables as Equation 1.1. Earnings and the length of uninterrupted insurance time are included as differences from 2008 August – December to 2009 August – December. Other control variables (occupation dummies, and firm level variables: size, industrial sector, profitability, export ratio, ownership dummies) are taken at their December 2009 value. When estimating the effect on colleagues' health outcomes, I also control for the previous health status of the employee by including his 2008 logged inpatient, outpatient, and medication expenditures in the estimation. Note, that this is not necessary for the regressions with the own health outcomes as the dependent variable, as it is defined as the change from 2008 to 2010.

As one's health cost and sick benefit take-up are both influenced by an underlying, unobserved factor, namely the health status, this estimation would be biased without using an appropriate instrumental variable. In this model, this role is played by the replacement rate, which has a substantial effect on individuals' sick leave decisions, but has no direct effect on their health status. Specifically, when estimating Equation (1.2) by two-stage

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least squares, the first stage equation is:

$$\Delta days_i = \rho + \nu \Delta rr_i + \kappa X_i + \varepsilon_i, \tag{1.3}$$

where Δrr_i is the change in the average replacement rate from the period August - December 2008 to the period August - December 2009. The results are valid under the assumption that the legislative changes in sick benefit replacement rates are randomly assigned to individuals conditional on the control variables, and they affect health outcomes only through changes in sick benefit take-up. While these assumptions are not directly testable, they are nevertheless reasonable. The estimated effects are local average treatment effects.

When estimating the effect on colleagues' health outcomes, I use the same model with the outcome variable being the change in the average health cost of the individual's colleagues from 2008 to 2010.

1.5 Sample restrictions, variable definitions and descriptive statistics

1.5.1 Models estimating the elasticity of benefit take-up

Although data is available from 2003, in order to better concentrate on individuals' behavior around the legislation changes, and to mitigate biases arising from changing attitudes over time, I only use the 2007–2011 period for estimation. Out of the 78,819,395 individual months corresponding to 2,036,365 employees eligible for sick leave, and having a reference income during this time, 5% are excluded from the estimation sample for the following reasons. As described in Section 1.2, the number of sick days one is eligible for, depends on his previous work history. This limit makes the number of actual sick days censored at individually different censoring points. Luckily, this censoring is only effective for a tiny fraction of individuals, therefore I simply exclude them from the estimation sample. Individual entrepreneurs are excluded, as they have different incentives regarding sick benefits. Individual months with incomplete insurance time are also excluded. This estimation sample contains 74,658,654 individual months, corresponding to 2,011,320 individuals.

The *dependent variable* of these models is the sick benefit take-up, which is measured by the number of sick days aggregated at a monthly level. Partly due to the existence of the 15 days of short-term sick leave – information about which is excluded from the dataset I use – the number of sick leave days shows a strong seasonal pattern, as illustrated by Figure A.2 in the Appendix. Observing the data at a yearly frequency, as plotted on Figure A.3 in the Appendix, shows an important decrease in take-up following both legislation changes. Instead of calculating yearly averages based on the calendar years, I cut the years between July and August so that the pre- and post-treatment periods of the 2009 legislation change are clearly distinguishable. Between the time periods August 2008 - July 2009, and August 2009 - July 2010, the average number of yearly sick leaves decreased by cca. 18%, from 7.2 to 5.9, and it further decreased to 4.8 by August 2010 - July 2011. These figures are equivalent to 0.6, 0.5 and 0.4 days in monthly terms, respectively. In cross-sections, individuals with higher income tend to take less sick days. See the 2008 values on Figure A.5 in the Appendix. This pattern is very similar to the one found by Böckerman et al. (2014) on Finnish data.

The *key explanatory variable* is the replacement rate. For each individual and each day, based on individual work history, I determine the reference period used for calculating the benefit level he is entitled to, as defined by the legislation. Using the income during the reference period, and the legislated replacement rates and caps, I calculate the benefit level. I call the ratio between the benefit level and the income during the reference period the replacement rate. Figure A.6 in the Appendix shows the evolution of these this variable over time. Up until August 2009, the replacement rate is in fact a simple weighted average of 60% and 70% values: 81-87% of people are eligible for the higher, and 13-19% for the lower replacement rate. The sharp drop in August 2009 indicates the effect of the uniform 10 percentage point cuts in these values, plus the effect of the introduction of the cap. The effect of the cap decrease in May 2011 is also clearly visible on the graph. The smaller shifts at the beginning of each calendar year are the result of the definition of the reference period, which, in many cases, is the previous calendar year. Also, as the benefit cap is linked to the minimum wage, there are two slight increases in replacement rates associated with two minimum wage increases between the two legislation changes. Concerning the distribution of these replacement rate changes across income groups, it is important to note, that while the 10 percentage points decrease in the replacement rate of August 2009 was universal, the benefit cap introduced at the same time affected only the top 5% of the income distribution. See figure A.4 in the Appendix for illustration.

Control variables include the gender and the age of the individual, the 2003 region of residence, average daily earnings during the three previous months, the length of uninterrupted insurance time, occupation groups based on two-digit ISCO codes, industrial sector based on two-digit NACE codes, firm ownership dummies (foreign, domestic, state or municipality owned), an export dummy taking on the value 1 if the firm has any export sales, a profit dummy taking on the value 1 if the firm has positive pre-tax profits and 0 with no profit or loss, and firm size. As firm level information is missing for public administration, I completed the data by assuming zero export sales for these employers, and creating a separate category for them in terms of ownership, profitability and industrial sector. For the same reason, instead of using the firm-reported employment data, I measure firm size by (the logarithm of) the total number of individuals working at the firm observed in the database in the given month. As the data is a 50% random sample of individuals, this should represent firm size without any systematic bias.

Descriptive statistics of the entire pool of insurees having a reference income, and of the estimation sample are provided in column (1) and (2) in Table A.1 in the Appendix, respectively. The two groups are very similar by all listed characteristics, which is a good sign regarding the external validity of the estimations carried out on this sample. The gender composition is perfectly balanced, and the average age is 4.6 years. Excluding individuals who get below from above, or above from below the benefit ceiling over time results in a decrease of sample size from 74.7 million to 63.5 million, but it does not cause any meaningful change in the value of either descriptive variable. The more substantial step in sample restrictions is the exclusion of the variance in relation with replacement rate group switches. The sample containing only non-switchers (column (4)) contains only 26.2 million observations, 76% of which belong to individuals never leaving the high bracket. This sample differs from the larger samples in one aspect: the average monthly sick benefit take-up is only 0.31, which is substantially smaller than the corresponding values of approximately 0.5 for the larger samples described above. As this group mostly contain individuals, who stayed in the high bracket continuously for a very long time, one explanation for the low sick benefit take-up could be an under-representation of individuals with frequent health problems, as they often cannot maintain their continuous employment history, thus fall out from the high bracket from time to time. Using the samples that split observations between high and low replacement groups at the observation level (described in column (5) and (6)) overcomes this problem in the sense that they jointly represent all the 63.5 million observations, yet do not identify from between-group switches. Note, that there are much more observations in the high replacement rate group than in the low replacement rate group (52.2 million and 10.9 million, respectively), and many individuals are represented in both groups, as the distinction is not at the individual level, but rather at the level of observations. Members of the low replacement group are, on average, somewhat younger (35.7 years as opposed to 41.5 years in the high replacement group), and there are slightly more males among them (53% as opposed to 48% in the other group). Quite obviously, they have 10 percentage points lower replacement rate, and, a possibly related fact, that they have a lower sick benefit take-up: 0.41 days per month, compared to 0.56 days per month for their high replacement rate peers.

Table A.2 and Table A.3 in the Appendix present the same descriptive statistics for the time period around the first and the second legislation change, respectively. The figures describing the time period January 2007 - April 2011 — the one that is more relevant to my question —, are very similar to the previously discussed ones in all regards. Descriptives describing the time period around the 2011 legislation change are also similar, with the notable difference that average replacement rates are lower by about 6 percentage points, due to the legislation changes.

Models estimating the own and colleagues' health outcomes

For the analyses regarding the effect on own and colleagues' health outcomes, I use cross-sectional models in which the main explanatory variable (the number of sick leave days), and the instrumental variable (the replacement rate) are defined as changes in the average values between two time periods: 2008 August – December and 2009 August – December. In order to have comparable values across individuals, I included only individuals who are eligible for sick leave on every single day within both of the above mentioned time intervals, and satisfy all requirements defined in the previous section in all of these months (i.e. not an individual entrepreneur, and the number of sick days he is eligible for is not an effective constraint), and an additional one: has a daily wage of at least 1 euro. As implausibly low wages may be a result of misrecording, and may not even indicate an employment relationship that provides the individual with sick benefit eligibility, it may be misleading to compare pre and post legislation change take-ups for individuals who only have a valid employment relationship in one of the periods.[3] The resulting sample contains 865,028 individuals, 50.4% of whom are men, the average age is 42.6. The average change in the monthly number of sick days is -0.012, which is equivalent to a 6.9% decrease compared to the baseline value. Replacement rates decreased by 11 percentage points on average;

[3]Qualitative results are found to be unaffected by this additional sample restriction.

a weighted average of the -10 percentage points for employees below the threshold, and the larger decreases for the few above. This is equivalent to a 15.7% decrease. When estimating the effect on own health outcomes, the dependent variables are the change in logged inpatient, outpatient, and medication expenditures from 2008 to 2010. I do not use 2011 health outcomes, as those may already be affected by the 2011 legislation change as well. Expenditure categories are estimated separately. Average nominal inpatient and outpatient costs increased by 26% and 10%, respectively, and medication costs decreased by 2.6% from 2008 to 2010. When restricting the sample to those who do not switch between replacement rate groups and do not get above/below the cap during either August – December 2008 or during August – December 2009, the sample size becomes 823,587. Descriptive statistics cited above remain virtually unchanged.

Regressions involving the health cost variables of individuals' colleagues are subject to further sample restrictions. For this analysis, the sample is restricted to small firms in order to increase the probability of personal interactions between individuals and their colleagues. Specifically, I use employees for firms for which we observe not more than 10 employees during each month of the period 2008-2010. As the database is a 50% random sample from the adult population, this corresponds to firms having 2-20 employees in expected value. To further increase the interaction between colleagues, I only include employees who spend the entire estimation period at the same firm. When calculating the average inpatient, outpatient, and medication cost corresponding to all colleagues of the given individual, that is, all employees working at the same firm at the same time, except for the individual, I only include colleagues, who also spend the entire time period at the given firm. This, altogether, reduces the sample size to 147,671 individuals.

1.6 Results

1.6.1 Elasticity of benefit take-up

Table A.4 in the Appendix presents the estimated elasticity of benefit take-up with respect to the replacement rate. As described above, both the replacement rate and the sick benefit take-up are associated with income levels, therefore controlling for income and the length of uninterrupted insurance time is an important part of the estimation. For this reason, I report estimation results both without (column (1)) and with (column (2)) these controls. Column (3) presents estimation results obtained by controlling for lagged health costs as well. In all of these three regressions, the estimated coefficients are statistically significant at the 1% level, and are also economically high: one percentage point increase in the replacement rate is associated with 0.93-1.1 extra days of sick leave per month. As the average monthly number of sick days is 0.48, and the average replacement rate is 0.63, these coefficients imply take-up elasticities of 1.2-1.5.

While these estimations use all variance in the replacement rate, in the next column I present estimates from the subsample containing only individuals who do not rise above from below, or sink below from above the benefit ceiling during the entire estimation period of 2007–2011, neither do they switch between the low and the high replacement rate groups. Here, both the estimated coefficient and the corresponding elasticity is much lower, the latter being 0.7, meaning that one percent increase in the replacement rate is associated with a 0.7 percent increase in take-up. Results for the subsamples containing only individual months corresponding to the low replacement rate group, and to the high replacement rate group are presented in the last two columns. The estimated elasticity is 1 for the high replacement rate group, and only 0.3 for the low replacement rate group. The difference in the elasticities is due to the fact, that they measure two very different local average treatment effects, as both the mean replacement rate and the demographic characteristics are different in the two groups. However, the fact that the weighted average of the two figures are very similar to the 0.7 elasticity estimate for the no switcher group lends credibility to these estimates, and suggests that both ways of eliminating variance from switching between replacement rate groups lead to similar results.

Results presented so far were based on the 2007–2011 time period, therefore encompass the effects of both the August 2009 and the May 2011 legislation changes. As the two legislation changes affected people at different parts of the income distribution, and – due to data constraints – only the former legislation change is used for estimating the effect of benefit take-up on health outcomes, it is necessary to separate the two events during the estimation. Table A.5 and Table A.6 in the Appendix present estimation results from two subperiods separately. The January 2007 – April 2011 period includes the 2009 legislation change, but ends just before May 2011, the time of the second change in replacement rates; while the August 2009 – December 2011 only includes this second event. During the period around the 2009 legislation change, which is of our primary interest in this study, elasticities were 1.7 in the entire sample, and 1.5 and 0.7 in the high and the low replacement groups, respectively. Again, the weighted average of these latter two figures is close to the estimated coefficient of 1.3 for the sub-sample of those, who never switch between replacement rate groups. Elasticities estimated in the neighborhood of the 2011 legislation change (Table

A.6 in the Appendix) are smaller than the ones estimated around the 2009 legislation change, discussed above: 0.6 for the entire sample, and 0.5 when estimated only for non-switchers. I find this somewhat surprising, as the 2011 cap increase affected people with lower income levels than the 2009 change, and I expected lower income people to react more sensitively to income losses arising from sickness, than the richer. However, one must bear in mind, that one percent change in the replacement rate means more in terms of nominal income loss for the high income people, than for the low income people. Thus if people make decisions based on nominal money losses as opposed to relative changes in their income, or if the costs of avoiding or minimizing sickness absence is linked to the price of certain products or services[4], then it is rational for richer people to react more to an income decrease. It is also possible, that lower income people are already less likely to use sick leave unless absolutely necessary, and therefore have less room for maneuver to react to decreases in the replacement rate. It is also curious, that the order between the elasticity of the high and the low replacement rate groups is changed: while it is higher for the high replacement rate group in the January 2007 – April 2011 period, it is the low replacement group that has the bigger figure in August 2009 – December 2011.

1.6.2 The effect of benefit levels on own and colleagues' health outcomes

Table A.7 in the Appendix presents the results from the first stage estimations. Column (1) and (2) correspond to the regressions in which the dependent variables are the individuals' own health outcomes, using the entire and the restricted sample, respectively.[5] The coefficients are statistically significant at all conventional levels, and have the expected sign and magnitude, and the F-statistics are significant as well, although they are below 10. Column (3) and (4) provide the same regressions on the sample corresponding to the regressions with colleagues' health outcomes as the dependent variables. Concerning the entire sample, the situation is analogous to the one described above: coefficients are sta-

[4]If people can ease the discomfort of working when ill e.g. by taking a taxi instead of commuting, hiring someone for house work to gain time to rest, etc., then they have to compare the price of these services to the income loss associated with staying at home. As the nominal income difference induced by one percentage point change in the replacement rate is higher for high income people, it is more likely that they lose more than the price of these services than their lower income colleagues.

[5]Note, that estimations using the three outcome variables — log inpatient, log outpatient and log medication costs — are equivalent in all other terms, thus the same first stage regression corresponds to all three of them.

tistically significant at all conventional levels, have the expected sign and magnitude, and the F-statistics are significant as well, although they are below 10. However, the coefficient for the restricted sample is not significant, potentially due to the low sample size — this means that the results of the corresponding second stage equations should be interpreted cautiously.

Table A.8 in the Appendix presents the estimates from Equation (1.2) for all health cost types (inpatient, outpatient and medication), both for the entire sample and for the restricted one. The entire sample consists of 865,028 individuals. The restricted sample, again, contains individuals satisfying two criteria. First, they are either below or above the benefit ceiling one each day during the August 2009 – December 2009 period, and do not switch between the two. Second, they do not switch between the high and the low replacement rate groups during either in August 2008 – December 2008 or August 2009 – December 2009. Low and high replacement rate individuals are pooled in a single regression, comprising 823,587 observations. When estimated on the entire sample (columns (1)-(3) in Table A.8), the change in the number of sick days seems to affect medication costs on the medium term. However, the sign of the estimated coefficient is not in line with the expectations. One extra day of sick leave increase is associated with a 282% increase in medication costs. As the average number of sick days per months is around 0.2 in this sample, one extra day means, in other words, a 500% increase in sick days. This being necessary to induce a 282% decrease in medication costs translates to an elasticity of 0.56. However, when restricting the sample to non-switchers, this effect becomes insignificant at the 5% level. As the decrease in the sample size is only 4.7%, it is unlikely that this would simply be an effect of losing power. Overall, I conclude that the data does not support the hypothesis, that the 2009 sick benefit cut would have had an important adverse health effect on the affected individuals. However, as size of the confidence interval is rather large, this only means a lack of evidence for an effect, as opposed to a strong evidence for the effect being a precise zero.

Estimation results concerning the effect on colleagues' health outcomes exhibit similar patterns. As presented in Table A.9 in the Appendix, when estimated on the entire sample, the coefficients are positive and statistically significant for all cost categories. However, when estimated on the restricted sample, the coefficients are not statistically different from zero. A plausible explanation for the unexpected positive sign of the coefficients estimated on the full sample may be that the health burden imposed to colleagues by the presenteeism of sick employees is overwhelmed by another force. When employees are on sick benefit

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— whether or not they are actually ill —, their colleagues may have to do their tasks as well, partially or entirely. This extra workload may have an adverse health effect, causing a positive relationship between the number of sick days taken by an employee, and his colleagues' health costs. These two, contradictory effects are not fundamentally impossible to distinguish, as the former is only present in case of sick leave take-up associated with contagious diseases. Sick leave days due to infectious diseases constitute only a fraction of all sick days[6], and are not distinguished in the database I use. However, as infectious diseases, like influenza, are rarely associated with hospitalization, I re-estimated the model with excluding all individuals who have positive inpatient costs in 2009. This reduces the sample size by 6-7%, but qualitatively has no effect on the coefficients, only increases the uncertainty of the estimates.[7]

1.6.3 Gender heterogeneity

As men and women differ substantially in terms of their labor market outcomes, it is worth investigating separately the effect of the cut in sick benefits on the behavior, and the subsequent health outcomes of each gender. Table A.10 and Table A.12 in the Appendix present elasticity estimations for the time period around the first legislation change, for males and females, respectively. Women spend more time on sick leave on average than men (0.35-0.62 and 0.27-0.39 days monthly, respectively, depending on the exact subsample), and they also seem to exhibit a more pronounced reaction to changes in the replacement rate in the January 2007–April 2011 period: while men show an elasticity of 1.1 without taking into account those, who switch between replacement rate groups (or, for the low and the high replacement rate groups combined), the corresponding figure for women is 1.6. Results regarding the August 2009–December 2011 period do not show such a clear pattern, and are less precisely estimated (although statistically significant at all conventional levels).

I investigated potential gender heterogeneities in the effect on own health outcomes, and on colleagues' health outcomes as well. Despite the differences in sick benefit take-up and elasticities, I found no consistent and convincing evidence of adverse health effect for either gender.[8]

[6] According to (Pichler and Ziebarth, 2017), analyzing German data from 1994–2004, 8.2% of sick days were classified as "infectious diseases", and another 35.4% as "respiratory diseases". Approximately 80% of cases in this latter group correspond to "bronchitis", "influenza", or "acute upper respiratory infections".

[7] Results are therefore not reported, but available upon request.

[8] Therefore these estimation results are not included in the present paper, but are available upon request.

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1.7 Conclusion and directions for further research

Using an episode of exogenous replacement rate decrease in the Hungarian sick benefit system in 2009 for identification, I find that the elasticity of sickness benefit take-up with respect to the replacement rate is 1.3 around the legislation change used for identification. In the spirit of Halla et al. (2017), I explore the effects of this replacement rate change, and the resulting decrease in take-up, on individual health outcomes proxied by inpatient, outpatient and medication expenditures. Neither of these health proxies are affected adversely by the policy induced decrease in the number of sick days, suggesting that the average worker in the database is not in the domain of presenteeism. Colleagues' health outcomes are also not affected significantly. This means that the state bears no losses arising from increased health care costs that would counterbalance its monetary gains realized from decreasing sick benefit replacement rates and take-up. However, one reason behind these results may be that — according to anecdotal evidence[9] — employees who can't afford to go on sick leave, tend to use their annual leave days when getting ill. This essentially means that they avoid presenteeism on the expense of their leisure time, which is clearly a factor of equity that the policy maker shall take into account when designing the sick benefit system.

Should data about firms' or employee's geographical residence become available, by analyzing the relationship between the estimated effects and the local unemployment rate, I could get a hint about the way these phenomena work during times of economic expansions and recessions, similarly to Halla et al. (2017). This may be especially relevant in the current case, as the 2008 economic crisis may have affected sick leave take-up around the time period I am investigating. Also, when data becomes available for further years, the health effects of the 2011 legislation change may provide an even deeper understanding of these phenomena.

[9] Source in Hungarian: https://www.penzcentrum.hu/biztositas/problemas-tappenz-trukkel-mentik-penzuket-a-magyar-csaladok.1060641.html, downloaded in January 2018

Chapter 2

You Make Me Sick: The Link Between Mother-to-bes' Sick Benefit Take-up and Their Employers' Parental Status

2.1 Motivation

Even though gender differences at the workplace are widely studied, the intersection of work and pregnancy is still considered as a relatively understudied area (Jones (2017)). According to survey evidence, most women perceive workplace pregnancy and maternity leaves as stressful and conflictual (Buzzanell and Liu (2007), Liu and Buzzanell (2004)), and pregnancy-related discrimination at the workplace is prevalent (Charlesworth and Macdonald (2007), McDonald, Dear and Backstrom (2008)).

In this study, I use a linked employer-employee dataset to test the hypothesis that women spend less time on sick leave during their pregnancy when they work under the supervision of someone who is a parent. My results support the hypothesis: I find that women spend 11.2 days less on sick leave during their pregnancy on average if their supervisor is a parent, an equivalent to a 10.1% change in sick leave days on average.

This contribution is important for two reasons. First, this sheds light on a new aspect of the misuse of the sick benefit system. By definition, sick leave is intended to provide financial support when the employee is forced to halt working due to medical conditions. Using it to avoid work related inconveniences from the mother's side, or to avoid necessary

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adjustments of working conditions from the employer's side is a misuse of the system. This overuse of insurance service is also a market failure, as in most cases, it would be cheaper for society to provide appropriate working conditions to pregnant women instead of inducing them to stay at home even when they are medically capable of working. The tendency of people to use sick leave for shirking (a phenomenon called *absenteeism*) has been studied both theoretically and empirically by Puhani and Sonderhof (2010), Halla et al. (2017), Ziebarth and Karlsson (2014), and Callison and Pesko (2016). However, despite the fact that descriptive analyses of sick leave take-up patterns of pregnant women has already been described (e.g. by Rieck and Telle (2013)), I am not aware of any studies considering the possibility of a potential misuse of the sick leave benefit system by pregnant women and/or their employers.

Second, the fact that parent supervisors are successful in keeping their pregnant employees at the firm much longer than others, indicates that there is a substantial room for maneuver in ameliorating women's working conditions and affinity to work during pregnancy. This is important, because while losing valuable working days at a pivotal point in one's career is a loss for the mother and for the society in itself, the list of damages does not stop here. The way pregnant women are treated at the workplace, and the amount of time they spend out of the labor force influences their prospective decision of returning to work (Houston and Marks (2003), Judiesch and Lyness (1999)). Also, as Salihu, Myers and August (2012) find in their meta-study, the impact of work culture experienced by the employee during her pregnancy can have profound implications not only for mothers' intentions to return to work after childbirth, but for their psychosocial health as well. Although the phenomenon of pregnant women's employment choices being influenced by the employers' and colleagues' attitude has been described in numerous studies based on survey information (Buzzanell and Liu (2007), Liu and Buzzanell (2004), Charlesworth and Macdonald (2007), McDonald, Dear and Backstrom (2008), Salihu, Myers and August (2012), among others), I am not aware of any previous evidence based on administrative datasets, that are not prone to the biases characterising self-assessment questionnaires. The role of the immediate supervisor has also been found to be important for the work-related experiences of pregnant women in survey studies (e.g. Mäkelä (2012)), but I am not aware of any evidence linking the work-related decisions of pregnant women to their supervisors' characteristics based on administrative databases. Therefore, the findings of this paper give additional credit to claims based on previous survey evidence, as it shows that pregnant women do not only complain about their treatment at the workplace, but

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also reveal their dissatisfaction by choosing to go on sick leave early on.

Although I do not have direct evidence on why it is specifically parents who facilitate work by parents-to-be, a natural hypothesis is that they understand better what pregnant women need, they care more about whether these needs are met, and also they may have a stronger preference for keeping these women at the company. In this sense, my research is also related to the literature on homophily at the workplace (McPherson, Smith-Lovin and Cook (2001)). Whether female managers are relatively more supportive towards female employees than male managers has already been investigated (e.g. Maume (2011)), and the empirical results are ambiguous. However, I am not aware of any studies addressing the same questions concerning parents. Watanabe (2015) examine non-work related social interactions in workplaces (faculties), and find that parent homophily exists in friendship networks together with a gender divide. Whether these stronger friendship ties translate to better professional relations, especially in supervisor - subordinate relationships, has not been addressed yet.

The effect of supervisors' parental status on the sick benefit take-up of pregnant women estimated in this study is not necessarily causal though. The documented phenomena might also — partially or entirely — be arising from some unobserved difference between firms. For example, workplaces with flexible working hours may employ more parents in manager positions that similar firms with fixed working hours. At the same time, flexible working hours allow pregnant employees to attend all necessary medical examinations without having to go on sick leave unnecessarily early. Nevertheless, unless this underlying cause that makes parent supervisors more likely to work at a certain firm also worsens the health condition of pregnant women, this does not invalidate my conclusions regarding the misuse of the sick benefit system, and the room for improvement in pregnant women's working conditions. To rule out the presence of an unobserved factor that attracts parent supervisors and makes employees more healthy at the same time, I designed a placebo test involving male employees working at the same firms, at the same time, and in the same occupations as women in my study. I find no difference in the sick benefit take-up of these men depending on whether or not their matched supervisors are parents. I also check, whether the lower number of sick days used by pregnant women with parent supervisors has an adverse effect on their post-birth health outcomes, to rule out any interpretations involving parent supervisors prohibiting their employees from taking medically necessary sick leaves. The data provides no evidence of such an adverse effect, supporting my hypothesis that the difference between the number of sick days between pregnant employees with non parent supervisors and that of pregnant employees with parent supervisors is arising from the overuse of sick leave by the former group, and not from the underuse of sick leaves by the latter group. This finding is in a similar vein as the findings of Ahammer, Halla and Schneeweis (2018). They show that increasing the time of mandatory prenatal leave from six to eight weeks in Austria in 1974 has had no significant positive effect on children's health outcomes, and thus conclude that six weeks of mandatory prenatal leave has proven to be sufficient in their context.

I describe the database in Section 2.2, and the Hungarian parental and sick benefit system in Section 2.3. Identification issues are discussed in Section 2.4. The definition and the descriptive statistics of the variables used to estimate the previously described model are presented in Section 2.5. Section 2.6 covers the estimation results. Section 2.7 discusses measurement errors and provides robustness checks: subsection 2.7.1 explores estimation results under various sample restrictions, Subsection 2.7.2 presents estimates using observations from firms that happen to be relatively well-covered by the data, and Subsection 2.7.3 re-estimates the model by only using female supervisors. In Section 2.8, I analyze potential selection issues and investigate alternative explanations for the results: Subsection 2.8.1 searches for an association between male employees' sick benefit take-up and their supervisors' parental status, and Subsection 2.8.2 investigates, whether the lower number of sick days used by pregnant women with parent supervisors has an adverse effect on their health outcomes. Section 2.9 concludes and draws policy lessons from the findings.

2.2 Data

I use a large, longitudinal dataset linking administrative data from the Hungarian National Pension Insurance, the National Tax and Customs Administration, and the National Health Insurance Fund, originally compiled for the Centre for Economic and Regional Studies of the Hungarian Academy of Sciences. The dataset contains a 50% random sample of Hungarian citizens of age 15-73 in 2003, covering the period 2003-2011.

The database contains information about the date of birth, gender, and the 2003 region of residence of individuals, alongside with monthly information about their employment status and labor income. I also observe an identifier of the company or companies the individual works at, allowing me to identify coworkers. Four digit occupation codes are also observed, and are used for matching employees to their supervisors.[1] The dataset comprises information on individual health costs covered by the National Health Insurance Fund. In- and outpatient care and medication costs are recorded separately, including co-payments for the latter.[2] As private health care played a marginal role on the Hungarian health care market during the time period covered by the data, except for a handful of professional areas[3], these variables capture individual health costs rather accurately.

2.3 Parental and sick benefit system in Hungary

Women with at least 180 (or, as of the 1st May 2010, 365) days of previous employment within two years of the birth of a child are eligible to *pregnancy and confinement benefit*[4] for a period of six months, which may be initiated no earlier than four weeks before their due date. The amount of the pregnancy and confinement benefit is 70% of average daily earnings. After these six months are exhausted, these mothers become eligible for *child care fee*[5] until the child is two years old. The eligibility criteria and the amount of the child care fee are the same as those of the confinement benefit, except for it being capped at the 70% of the double of the minimum wage. Women who are not eligible for the pregnancy and confinement benefit, or who have children above two years, receive a much lower amount of subsidy, named *child care allowance*[6] until the child turns 3. Parents of twins and parents of chronically ill and disabled children are eligible for the benefit for a longer time.[7] The amount of the child care allowance is equal to the minimum old-age pension. Parents of children above three years old are eligible to *child raising support*[8] if they have at least three children, from the date when the youngest child reaches the age of 3 until that child reaches the age of 8, if the parent does not engage in paid employment for more than 30 hours a week, or works at home. Information on individuals' parental

[1]The Hungarian Standard Classification of Occupations (HSCO) follows the structure of its international counterpart, the ISCO. For the years 2003 - 2010, the HSCO-93 classification is used, which is similar to the ISCO-88, while for 2011 the HSCO-08 is used, which mirrors the ISCO-08.

[2]There are no co-payments in Hungary for in- and outpatient care services.

[3]Dental care and gynecology are exceptions.

[4]terhességi-gyermekágyi segély, TGYÁS

[5]gyermekgondozási díj, GYED

[6]gyermekgondozási segély, GYES

[7]For twins, up to the age of 6. For chronically ill and disabled children, up to the age of 14 until 2006, and as of the 1st January 2006, up to the age of 8.

[8]gyermeknevelési támogatás, GYET

benefit take-up is used for two purposes in this paper. First, I use the pregnancy and confinement benefit spells to identify pregnant women and uncover the date of childbirth. I only consider working women who are eligible for the pregnancy and confinement benefit, because women without a continuous working history are often not eligible for sick benefit either. Second, I find parents among supervisors by checking whether they received any sort of parental benefits any time before the employee's pregnancy, during the time period covered by the dataset. For this latter purpose, all of the parental benefit schemes described above are used.

The Hungarian sickness benefit scheme is universal in the sense that all employees are covered by the statutory health insurance scheme, making them eligible to a certain level of sickness insurance. The sickness benefit system comprises two phases. First, enrollees have access to a maximum of 15 days of firm-financed short-term sick leave annually, during which time their employer pays them 70% of their wage. After having exhausted this 15 day limit, employees become eligible to long-term sickness leave, which is co-financed by the state (2/3) and the employer (1/3). The maximum length, the basis and the replacement rate of long-term sick benefit depend on the employee's previous work history. The amount of sick benefit changed twice during the period of observation.[9] These changes in the sick benefit system need to be controlled for by including time dummies, otherwise they should not affect my question of investigation.

Based on the above, when a pregnant woman becomes unable to work due to health issues, she has the following choices. First, she can use her annual 15 days of employer-paid short term sick leave. After exhausting this, she can go on sick leave. Alternatively, she can start her pregnancy and confinement benefit at most four weeks earlier than the time of birth, but this is a suboptimal choice for those who reach the (rather low) benefit cap of the child care fee. Theoretically, women can also choose to use their regular annual leave for staying at home, although it is financially not optimal for most of them.[10]

Medial incapacity to work (illness or high-risk pregnancy) has to be certified by the insuree's general practitioner, or, in case of pregnant women, their gynecologist. However, there is anecdotal evidence, that most gynecologists provide this certification for any

[9] As of August 2009, the replacement rate of sickness benefit decreased uniformly by 10 percentage points (from 70% to 60% and for 60% to 50% depending on previous work history), and a benefit ceiling was introduced at 400% of the minimum wage. The cap was decreased to 200% of the minimum wage in May 2011.

[10] These annual leave days can be used when getting back to work after maternity leave, or get paid for by the employer.

pregnant woman asking for it, and some of them offer the certification during prenatal care even to low-risk and healthy pregnant women. The reason for this — according to gynecologists' newspaper interviews[11] — is risk avoidance from their part: while issuing the certificate entails no cost for the gynecologist, refusing to issue it involves a risk of being held responsible in case of any medical issues arising later during the pregnancy or childbirth.

2.4 Identification

I estimate the following linear regression model, in which the level of observations is a pregnancy spell.

$$leave_{itfo} = \alpha + \beta parent_{tfo} + \gamma X_{itfo} + \theta Z_{tf} + \eta_t + \epsilon_{itfo} \tag{2.1}$$

where $leave_{itfe}$ is the number of sick leave days during the pregnancy of mother i, giving birth in month t, working at firm f with an occupation o. β is the coefficient of primary interest, belonging to $parent_{tfo}$, the share of parents among supervisors of all employees working at firm f, in managerial occupations that are linked to the employee occupation code o, at time t. (The exact way of linking managers to employees is discussed in Section 2.5.) X_{itfo} are individual level control variables: age, region, employment type, occupation code dummies, previous year's health costs and earnings. Controlling for earnings is important for multiple reasons. Besides proxying socio-economic background, earnings also influence the level of sick benefit the individual is eligible for, both through the benefit basis and through the benefit rate. The lagged health variables intend to capture the pre-birth health status of the woman, and only partially include the costs associated with medical interventions during pregnancy: for those giving birth in the early months of a year, most of their pregnancy-related costs are represented in the previous year's record, while those who give birth in the last quarter, these costs are completely excluded. Medical costs during the year of giving birth are not used as control variables, because they contain the costs related to giving birth, which may itself be affected by the individual's choice about how long to work before childbirth. Sick benefit take-up during the 365 days preceding the pregnancy are also used as control variables in order to capture any differences in women's

[11]E.g. one source in Hungarian: https://24.hu/kozelet/2018/04/27/veszelyeztetett-terhes-tappenz/, downloaded in April 2018

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general medical status and attitude towards sick leave. Z_{tf} are firm level controls: size, export status, domestic/foreign/public ownership, profitability, and industrial sector dummies. η_t are time dummies at a monthly frequency, which take care of legislative changes in the sick benefit level, and also seasonality.

As employees are not randomly assigned to supervisors, the relationship grasped by the above regression is not necessarily causal. Any estimated correspondence between the number of sick days during pregnancy, and the share of parents among matched supervisor may potentially be arising from some unobserved difference between individuals and/or firms. How this possibility affects the interpretation of my results, is discussed in Section 2.8.

2.5 Variables

2.5.1 Variable definitions

Dependent variable

The dependent variable is the aggregated number of sick leave days during pregnancy. As neither the starting day of pregnancy, nor the exact day of giving birth is recorded in the dataset, I refer to the 270 days' period directly preceding the starting date of the mother's maternity leave as the time of pregnancy.[12] All sick leave days within this period are added up in the dependent variable, even if the sick days are followed by working days. I decided to use this measure as opposed to using the length of only the last, uninterrupted sick leave spell directly preceding the birth, because any decision regarding sick leave during pregnancy may be related to the employer's attitude just as much as the length of the last spell.

Main explanatory variable

The main explanatory variable is the parental status of the leader(s). To measure this, I have to infer two pieces of information from the data: the identity of potential supervisors corresponding to a given mother-to-be, and the parental status of these leaders.

[12] Legally, the maternity leave may be initiated at most four weeks before the baby is due. However, for women who are eligible for sick pay, it is financially not worth using up their valuable maternity leave days before giving birth instead of going on sick leave. In any ways, as sick leave during pregnancy is mostly concentrated on the second half of pregnancies (see B.2 in the Appendix, discussed in Subsection 2.5.2), it does not really matter where exactly the beginning of the time period of observation is.

The Hungarian Standard Classification of Occupations (HSCO) distinguishes heads of units and other managers from subordinates in many fields, giving me a hint about employee hierarchies. Similarly to Caliendo et al. (2015), I base my analysis on the companies' hierarchical layers defined by occupation codes, but extend their concept by defining finer subgroups within the layers instead of regarding the entire layer of supervisors as potential leaders of each employee at the lower levels as they do. For example, an accountancy and finance manager is likely to be the superior of auditors and accountants of the same firm, but is unlikely to be responsible for software developers. I use this information to assign employees to their superiors in two steps. First, I compile a list of supervisory occupational codes and identify all the employee occupational codes that likely represent subordinates of these supervisors. In some cases, more than one supervisor occupation codes are connected to an employee occupation code. The 14 supervisor and 130 subordinate occupation codes together generate 207 supervisor-employee occupation code pairs.[13] For a full list of these pairs, see the Appendix. Then, based on the list of supervisor-employee occupation code pairs, for each mother, I identify all the supervisors working at the same company at the same time who may potentially be her leader. Occupational codes do not provide sufficient information to uncover the pool of subordinates of a given supervisor with certainty. Often there are more than one supervisors with the same occupational code at the same firm, and also there are many occupation categories that may be supervised by more than one type of supervisors. In case there are more than one supervisors matched to a pregnant woman, I use the share of parents among supervisors as the main explanatory variable. Moreover, the data covers only half of the population, thus there are potential hidden supervisors and employees in any firm in our sample. Altogether, the supervisor-employee assignments should be treated as a proxy.

Observing child related benefits (pregnancy and confinement benefit, child care fee, child care allowance, and the child raising support) allows me to infer information regarding the parenthood status of supervisors. Therefore, I base the dummy variable proxying parenthood on these: it takes on the value one in a given time period if the individual has received any type of parental benefit any time before that, and zero otherwise.[14] Note,

[13]For the year 2011, occupations are recorded in the HSCO-08 system in the data. In this classification system, I used 18 supervisor, 129 employee codes and 146 pairs.

[14]This method leads to an increase in the ratio of parents over the time horizon, as the longer the observation period, the more likely it is to find any parental benefit in the individual's history. In 2004, I only recognize someone as a parent, if he or she received parental benefits some time during 2003 or 2004, while in 2011, I can check the entire period 2003-2011 for parental benefits. This is one more reason

that as the variable definition is based on benefits received during the preceding few years, this rather reflects recent parenthood, i.e. being a parent of small children, as opposed to parenthood in general.

Controls

There are three groups of individual level control variables in the model. Demographic variables are age at giving birth and the region of residence in 2003. Health status variables are the sick benefit-take-up during the year before the pregnancy, health fund covered inpatient and outpatient costs, and all prescription medication costs corresponding to the individual during the year before giving birth. Job related variables are the four digit occupation code, the type of employment[15], and logged earnings during the year before giving birth.

Firm level control variables are ownership dummies (foreign, domestic, state or municipality owned); an export dummy taking on the value 1 if the firm has any export sales; a profit dummy taking on the value 1 if the firm has positive pre-tax profits and 0 with no profit or loss; two-digit NACE codes and firm size. As firm level information is missing for public administration, I completed the data by assuming zero export sales for these employers, and creating a separate category for them in terms of ownership, profitability and industrial sector. For the same reason, instead of using the firm-reported employment data, I measure firm size by (the logarithm of) the total number of individuals working at the firm observed in the database in the given month. As the data is a 50% random sample of individuals, this should represent firm size without any systematic bias.

In some specifications, I also include the (average) age and the (ratio of) sex(es) of leader(s) as controls.

2.5.2 Descriptives

There are 169,064 pregnancy and confinement benefit spells starting between the beginning of October 2004 and the end of December 2011 in the database. Because of the restriction in the eligibility criteria mentioned in Section 2.3, spells starting in 2010 or 2011 constitute only 11% of the sample. I restrict my attention to those 122,000 spells, that belong to mothers having an employment relationship throughout their entire pregnancy. Among

making the inclusion of time dummies absolutely necessary.

[15]private employment contract, civil servant, public employee, public worker, employed by the armed forces or the state judicial system, employed in a co-operative

them, 54,813 have an occupation code that I am able to pair with a supervisor occupation code. I could find potential supervisors operating at their firm at the relevant time period for 28,207 of them. Out of them, 4,073 have a single matched supervisor, and another 3,067 have 2-5 potential supervisors. Note that even having a single matched supervisor does not necessarily mean a perfectly sure match between them, as the database itself is a 50% random sample of the population. However, the quality of supervisor assignment is potentially better when the number of matched supervisors is lower, and/or when the firm is smaller. For that reason, I exclude very large firms from my baseline estimations, and investigate the robustness of the results for various sample selection criteria along these lines in Section 2.7. Excluding pregnancy spells corresponding to women working at companies with more than 10,000 employees in my database[16] leaves me with a sample size of 18,936, out of which 3,755 have a single assigned supervisor, and another 2,993 have 2-5 supervisors. The average number of supervisors corresponding to a pregnant woman in this subsample is 57. This sample of 18,936 pregnancy spells represent 18,243 women (96% of women have a single pregnancy spell during this almost eight year long observational period, and the highest number of pregnancies belonging to one woman is 3) and 2,820 firms. 90 different occupations are represented in the sample, and the distribution of women across occupations is fairly dispersed.[17] The most frequent occupations are presented in Table B.1 in the Appendix.

While the age range of the mothers is wide (from 18 to 57), 80% of them is between 25 and 35 years old, both the median and the average is 30.[18] The distribution of mothers' previous years' earnings is lognormal (see histogram on Figure B.3 in the Appendix).

Graph B.1 is the histogram of the dependent variable, that is, the number of sick leave days during pregnancy in the above described subsample of women. 12.5% of pregnant women have zero sick leave take-up. They may work until the last day of their pregnancy (which is probably more likely in cases when the baby is born earlier than their due date, or when the mother works from home), or use their short term sick leave days, and/or their annual leave to cover any time they may spend out of work. Also, they may start their

[16]As the database is a 50% random sample of individuals, it means that the firm size limit is 20,000 employees in expected terms.

[17]In 2011, using the HSCO-08 classification system, the number of different occupations represented in the data is 87.

[18]The average age of the entire set of 169,064 pregnancies starting between the beginning of October 2004 and the end of December 2011 is also 30 years, thus this subsample can be considered representative in this regard.

pregnancy and confinement benefit earlier than giving birth. The non-zero values are fairly evenly distributed between 1 and about 230, with a few higher values. The mean is 111 days, and the median is 112 days. While these figures include all sick leave days during the pregnancy, Figure B.2 shows how these sick days are distributed over the time of pregnancy. Not surprisingly, sick days tend to concentrate at the end of the pregnancy spells: 270 days before giving birth, 1.8% of women are on sick leave, and this ratio gradually increases up to 80.8% for the last day.

Concerning the main explanatory variable, only 1.2% of women have exclusively parent supervisors, 51.8% have only supervisors who are not parents, and the remaining 47% have both types of supervisors. For them, the value of the explanatory variable is between 0 and 1, indicating the ratio of parents among the assigned supervisors. As for the gender of the supervisors, 13.3% of pregnant women in the sample have male supervisor(s), 16.8% have female supervisors(s), and the others have a mixed supervisor pool. As the take-up of parental benefits is much higher among women than among men, these variables are interrelated. The average ratio of parent supervisors is much higher for those pregnant women, who have exclusively female supervisors, than for those, whose supervisors are all males; 9% and 0.6%, respectively. The average age of women's supervisors is 45.3.

The distribution of non-zero values of all three health control variables are close to log-normal. The ratio of zero values is 74% for inpatient costs, 3% for outpatient costs, and 9% for medical costs. Regarding firm level controls, 16% of women work for domestic private companies, 21% for foreign owned firms, 4% for state or government-owned companies, and 59% in the public sector. 20% of pregnant women in the sample work for companies having positive export sales, and 30% for firms that have positive pretax profit. The distribution of firms according to the number of observed employees in the dataset (my firm size proxy) is presented on Figure B.4 in the Appendix.

2.6 Results

Estimation results are presented in Table B.2 in the Appendix. The sample is restricted to mothers with an employment relationship throughout their pregnancy, and working at a firm with not more than 10,000 observed employees in my database. In the first specification, there are no control variables, only the time fixed effects at a monthly frequency. The estimated effect is substantial: all else held equal, an employee whose all matched supervisors are parents, stays at work almost 10 days more compared to someone with only non parent supervisors. The second specification includes individual demographic, work related and health controls. Comparing to the first one, here the R^2 goes up dramatically (to 0.27), but the coefficient estimate remains stable and highly significant. This holds true for the third specification as well, which differs by the inclusion of firm level control variables. In the fourth, preferred specification I added two other variables describing the supervisor(s) of the mother-to-be. The first is a dummy variable indicating whether the supervisor is a male, or, in case of multiple matched supervisors, the ratio of males among them. The second is the (average) age of the supervisor(s). Neither of these two factors seem to play a statistically significant role in pregnant women's sick leave take-up, nor do they substantially alter the estimated effect regarding the parental status of the supervisor. This proves that the estimation results are not driven by the fact that women's parenthood is more often observed in the data than men's. To sum up, the results of the preferred specification show, that women who have exclusively parent matched supervisors spend 11.2 days less on sick leave during their pregnancy than women with exclusively non parent supervisors, all other factors being equal. Compared to the mean value of 111, this means a difference of 10.1%.

There is no evidence in the data for substantial treatment effect heterogeneities across geographical regions, neither are there statistically significant differences in the treatment effects of women in jobs requiring different education levels. This lack of evidence for treatment effect heterogeneities may however be a result of sample size constraints.

2.7 Measurement error and robustness checks

Supervisors' parental status and supervisor - employee relationships are described by proxies in the data, creating a source of measurement error. The way I measure supervisors' parental status is based on the previous few years' benefit history, thus fails to recognize parents who do not receive any child-related state benefits. As some benefits used in the process are universal in the sense that all parents are entitled to it, the measurement error only arises from the fact that these benefits can be taken by only one of the parents. As gender roles are very traditional in Hungary, the number of mothers who do not receive any child-related benefit during the first years after giving birth is most likely negligible. Therefore, this measurement error is only prevalent among male supervisors, that is, potentially many fathers with young children remain hidden in the data and get classified an non parents. In order to check, whether this is a major concern, I repeat the analysis using only female supervisors in Subsection 2.7.3.

Concerning supervisor - employee relationships, there are three sources of potential misallocation of employees to supervisors. First, the data I use is a 50% random sample from the population, therefore there are supervisors, who remain hidden for me. Second, when there are multiple potential supervisors with the same occupation code, I can only calculate the expected value of the real supervisor's parental status by using a mean value of their parental status. Third, it is possible that, in certain cases, employees have supervisors with a different occupation code, than the one matched to them by my pre-defined list occupation code pairs, e.g. an HR manager oversees an IT employee. Unfortunately, as this latter component of the measurement error is impossible to quantify, I cannot calculate the extent of the overall measurement error either. Nevertheless, I perform two exercises to get an idea about the importance of the first two sources of errors. In Subsection 2.7.1, I investigate whether the results are similar for subsamples of pregnant women having homogeneous supervisor groups (i.e. only parent or only non parent supervisors), and in Subsection 2.7.2, I re-estimate the model using only women working at firms that happen to be relatively well-covered by the 50% random sample, based on a comparison of the number of employees observed in the database and the firm reported number of employees.

Overall, due to the measurement error, my estimates are subject to an attenuation bias and shall be treated as a lower bound of the real effect.

2.7.1 Sample restrictions based on firm size and number of supervisors

As the assignment process between pregnant women and their supervisors is supposed to be more uncertain in large firms and in case of many assigned supervisors, I excluded large firms from my baseline estimations presented in Section 2.6. In this section, I explore how this restriction affects estimation results, and consider some alternative restrictions along these lines. The results of these alternative models are presented in Table B.3. Each of them includes all the control variables of the preferred baseline specification from Section 2.6, they only differ by the sample restrictions. Therefore, the results should be compared to the coefficient estimate of -11.2 presented in the last column of Table B.2.

In the first specification, I remove the size limit on firms, that is, I include all pregnancy spells regardless of the size of the firm or the number of potential supervisors matched to the mother-to-be. Despite the sample being larger by about one third, the coefficient estimate is almost unchanged (-12.5), and highly significant. The second column in Table

B.3 presents the estimates from a sample, where, for each pregnancy spell, the group of the mother's assigned supervisors is homogeneous in terms of parental status. That is, only those women are included in the regression, who either have a single supervisor, or their potential supervisors are either all parents, or none of them has a history of parental benefits in the database. This restriction reduces the sample size importantly, to 9,655, while the number of explanatory variables is 333. Thus, although the coefficient estimate remains qualitatively very similar (-7.3), it is not statistically significant. Increasing the degrees of freedom by excluding occupation dummies from this specification yields a significant coefficient of value -10.5, as presented in column (3).

These results demonstrate that the estimated effect is stable not only across specifications with different sets of control variables, but it is also robust to the exclusion of women with a large and/or heterogenous group of matched supervisors.

2.7.2 Well covered firms

As discussed above, the fact that I am working with a 50% sample of the population, gives rise to a measurement error from hidden supervisors. To get an idea about the extent of this error, I calculated a firm level measure of data coverage by dividing the number of observed employees in the dataset by the number of employees reported by the firm. The distribution of pregnancy spells according to the data coverage of the firm the pregnant woman works at is depicted on Figure B.5 in the Appendix. Unfortunately, this information is not available for public sector institutions, as the official number of employees is only provided by firms in this data. As pregnancy spell observations corresponding to better covered firms are less prone to measurement error, I compare estimates for the below 0.5 and the above 0.5 data coverage observations. As the number of observations is much lower than in the entire sample, I limit the set of control variables. Column (1) of Table B.4 in the Appendix shows, that this limitation has no important effect on the baseline coefficient estimate: it is -13.6 days when estimated on the entire sample of 21 thousand pregnancy spells. However, as the majority of our observations are coming from the public sector, I am only left with less than 4 thousand observations, for which I observe the data coverage measure, approximately half of which are above, and the other half are below 50% coverage. I re-estimated the model for these subsamples, to see whether the results are stronger for well covered firms. While the coefficient estimate for the entire four thousand observations (column (2)), and the one estimated for the relatively weakly covered firms (column (3)) are not statistically different from zero, the coefficient for women working at firms that are well covered in the dataset (column (4)) is higher than for the entire sample (-22.5) and significant. Therefore, one can conclude, that the attenuation bias coming from not observing everyone in the firm is indeed strong, the true effect is probably even stronger than my baseline estimations.

2.7.3 Female supervisors

As mothers receive child related subsidies more often than fathers, my parental status proxy is more accurate for female supervisors than for men supervisors. Also, the large difference between the average parental status of female and male supervisors may raise concerns that the effect I am capturing is in fact due to gender differences as opposed to parental status differences. Although this second concern is addressed by including the supervisor pools' gender composition as a control variable in the estimation models, in this subsection I re-estimate the models using only female supervisors.

The regression results are presented in Table B.5, and robustness checks are presented in Table B.6 in the Appendix. The models presented in these tables are analogous to the ones featured in Table B.2 and Table B.3, therefore the coefficient estimates are directly comparable. The results are virtually unchanged, the estimated effect in the preferred specification is 11.5 days.

2.8 Tests of alternative explanations

2.8.1 Selection into jobs and pregnancy

My results may potentially be arising from some unobserved heterogeneity across individuals and/or firms. This possibility may or may not invalidate my conclusions, depending on the nature of this heterogeneity. It is possible, that certain firms have better work-family culture than others, allowing parents to work as supervisors, and providing appropriate working condition for pregnant women at the same time. However, this does not invalidate the main conclusions of the paper.

On the other hand, it would be problematic, if the estimated negative coefficient was caused by a self-selection issue, where employees who are less likely to go on sick leave — either because they have a relatively good health condition, and/or because they tend to avoid going on sick leave unless it is absolutely necessary — self-select into departments where the supervisor is a parent. Even though this possibility sounds counter-intuitive (healthy employees with a strong affinity for work seek competitive workplaces, and competitive workplaces with low tolerance towards employee's absence likely disfavor parents as managers), it is theoretically possible, and needs to be investigated. Similarly, one might argue, that parent supervisors are constrained by their parental duties, thus are more likely to be absent from work, which may result in a higher pressure on their employees about not to go on sick leave. To exclude these interpretations, I designed a placebo test. First, I collected all male employees working at the same time, at the same firms, with the same occupation codes as women in my original sample. Then I checked whether these male employees have higher sick benefit take-up when their leaders are parents, by estimating Equation 2.1, using the monthly sick benefit take-up as the dependent variable.

Table B.7 presents the estimation results. As both the sample restrictions and the specifications are the same, the results can be compared to those reported in Table B.2. The average value of the outcome variable, i.e. the monthly average number of sick days, is 0.25 in this sample. As in most firms there are more male employees than pregnant women employees, the sample size is more than thirty times larger here than in the regression concerning pregnant women's sick leave length. In spite of the much larger sample size, there seems to be no statistically significant relationship between the family status of the supervisor and the sick leave take-up of male employees with either set of control variables.

As, similarly to employment, pregnancy is not a random event either, I also need to consider, whether my results are caused by selection into pregnancy. Note, that in order to influence the results of my estimations, a simple difference in the likelihood of pregnancy between women working under the supervision of parent supervisors, and those working under the supervision of non parents, would not be sufficient. Rather, in order to generate these estimation results, a more complicated selection procedure would be needed: those women, who have the type of going on long sick leave during pregnancy need to postpone or cancel child bearing when working in companies with parent supervisors, and/or those women, who have the type of not going on long sick leave during pregnancy should postpone or cancel child bearing when working in companies with non parent supervisors. Although the presence of this mechanism is non-testable, the inclusion of variables representing pre-existing health status and the attitude towards going on sick leave (lagged inpatient, outpatient and medication costs, and sick leave take-up before pregnancy) as controls should partially take care of these kind of selection issues, should they arise. Also, the fact that the inclusion of these controls has no important effect on the estimation results, that is, there is no such selection going on along the lines of the observable part of being the type of going on long sick leave during pregnancy, hints that it is unlikely to have an important selection issue on the unobservable part of it either.

2.8.2 Presenteeism of pregnant women?

In this subsection, I carry out another test to check whether the "extra" sick benefit days that are taken by pregnant women not working under the supervision of parent supervisors would have been medically necessary for women with parent supervisors as well. In other words, I would like to know, whether parent supervisors provoke presenteeism of their pregnant employees. To check this, I test, whether the lower sick leave take-up during pregnancy associated with parent supervisors causes a deterioration of employees' health outcomes. The proxy variables for future health status of the employee are the inpatient, outpatient and medical costs during the year after the baby is born. The analysis is carried out by regressing the health outcomes on the number of sick leave days during pregnancy, while using the ratio of parents among supervisors as an instrument. Thus, the first stage equation is Equation (2.1), as presented in Section 2.4, and the second stage equation is as follows.

$$health_i^{t+1} = \alpha + \beta * leave_i + \gamma X_i + \epsilon, \tag{2.2}$$

where $health_i^{t+1}$ is the logged inpatient, outpatient, or medical cost in the year after giving birth. Other notations and control variables are the same as in Equation (2.1).

The estimation results are presented in Table B.8 in the Appendix. Coefficients corresponding to future values of all three health cost categories are statistically zero. Thus, there is no evidence for underuse of sick benefits by pregnant employees with parent supervisors, which is in favor of my hypothesis about an unnecessary overuse of insurance by women with non parent supervisors.

2.9 Interpretation and future research

Women's sick leave take-up during their pregnancy is 11.2 days shorter if their supervisors are parents. This relationship between pregnant women's sick leave take-up and their supervisors' parental status proved to be robust for various changes in the set of control variables and sample restrictions. Also, no similar relationship was detected between sick leave take-up of male employees and their supervisors' parental status.

One mechanism behind this effect can be that parents are better supervisors of pregnant

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women in the sense that they prevent them from resorting to sick leave unnecessarily early. This is in line with the theory of homophily at the workplace. Alternatively, or at the same time, some firms may have a more supporting attitude towards pregnant women and mothers in general, making parents become leaders and pregnant women stay longer at work than other firms operating in the same sector, with same characteristics in terms of size, ownership, profitability, export status. In this case, the parental status of the supervisor is in fact a measure of the employer's attitude towards employees with children. Another specific channel of this mechanism can be that pregnant women, who witness other parents holding supervisory positions at their firm interpret this as a positive sign of their prospective career outcomes. This may increase the anticipated return on their efforts, inducing them to work more during their pregnancy, and, potentially also the likelihood of them returning to work after childbirth (Houston and Marks (2003), Judiesch and Lyness (1999)). All of these mechanisms deliver the conclusion that employers have a major room for improvement in increasing women's labor market participation. Creating procedures to maintain the commitment of pregnant women, educating employees about dealing with the special needs of pregnant co-workers and subordinates, and encouraging them to be supportive and flexible with them during pregnancy would prove to be a win-win game for all stake-holders: the mother, the firm, and society as a whole. Carefully monitoring the complaints of pregnant women regarding their work environment and relationship with co-workers may prove to be useful in identifying the firm specific actions that need to be implemented.

Chapter 3

The Effect of Managers' Health Shocks on Employment Practices

Co-author: Gergely Hajdu

3.1 Introduction

A past experience can have a long-lasting effect on one's preferences and economic decision making, as demonstrated by Malmendier and Nagel (2011) using data on financial investment decisions of individuals experiencing macroeconomic shocks decades earlier. There is a recent literature focusing on personal traits and experiences that affect managerial decisions, and the future performance of a company (Kaplan et al. (2012), Bragaw and Misangyi (2013), Nguyen (2015)). Nevertheless, little is known about the effect of personal experiences on the interpersonal relationships of managers. Dahl et al. (2012) measure the effect of an exogenous event in a CEO's life, namely, the gender of his newborn child, on the wage of his employees. Our paper contributes to the literature by exploring the effect of a manager's temporary health shock on the separation rate of the manager's employees. As traumatic events have been shown to influence the development of a pessimistic explanatory style (as reviewed by Peterson and Steen (2002)), and to impair one's ability to exercise self-control and delay gratification (Simmen-Janevska et al. (2015)), both being potentially important factors in leadership, we hypothesize that health shocks have the potential to change managers' preferences regarding their working relationships, leading to an adjustment in their working pools. Specifically, we expect managers to part with more of their employees as they would have done without this experience. If this hypothesis is true, this should result in an increased exit rate within managers' employee pool following their illness episode.

We test this hypothesis using an administrative panel dataset linking employers and employees, that contains, alongside with labor market variables, inpatient health care costs. Although we do not observe firms' reporting hierarchies, we proxy manager - employee relationships using a fine resolution of occupation codes. Similarly to Caliendo et al. (2015), we make use of the hierarchical coding of occupations, but, unlike them, we do not only differentiate between hierarchical layers, but attempt to specify separate employee pools for each type of managers based on precise occupations. Besides covering a wider range of managers as opposed to using information solely on CEOs, this approach has the advantage of investigating close leader-employee relationships within a firm. This way we use information about the very level where the decision about the separation between manager and employee is really made. After identifying suspected manager-employee relationships in the data, we calculate the average monthly exit rate within the employee pool of each manager. Using this as an outcome variable, we estimate the average treatment effect on the treated of a temporary illness episode — which is defined as a single, temporary peak in a manager's inpatient health cost history. To minimize concerns about the validity of our results due to potential selection bias, that there may be factors that affect both health and employee relations, we choose the shortest possible illness episodes and limit our attention to people who have zero inpatient cost in all other time periods on our horizon, getting as close to the concept of a randomly assigned health shock as possible in similar datasets.

Besides analysing the effect of a temporary illness episode on the separation rate within managers' employee pools, we provide a descriptive analysis of their own employment outcomes, contributing to the small literature on the effect of temporary leaves from work on managers' career paths. Compared to Judiesch and Lyness (1999), who use data from a single firm for exploring this issue, we have the advantage of using a large database including managers from all sectors of the economy.

We use a matched control group including managers who never experience health care shocks throughout our observation period. We find that even four years are not enough for managers to entirely leave the effects of their illness episode behind: while 18.23% of them has no job by this time, the corresponding ratio is only 13.02% for the control group, and this difference is almost entirely coming from the difference in their likelihood of having a manager position. Conditional on staying in their job, managers' wage decreases by 13.4% following the year of illness, compared to the wage evolution of their matched

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healthy counterparts. This resonates with the findings of Judiesch and Lyness (1999), who claim that leaves of absence of managers – regardless of whether they result from family responsibilities or illness – are associated with significantly fewer subsequent promotions and smaller salary increases. Our results regarding the evolution of the separation rates in the employee pool are in line with our expectations, as we find an 8% increase in the employee separation rates following the illness of the manager. Re-estimating our model only for separations that are likely initiated by the employee, and also for the ones that likely correspond to a dismissal yields the results that it is mostly an increase in dismissals that seem to drive this phenomenon. We find no corresponding effect in the size of treated managers' employee pool, suggesting that it is the restructuring of the employee pool that causes the increase in separation rates, and not department downsizing.

This paper aims to contribute to the early steps of the research agenda of understanding the effect of personal experiences on economic decisions, and, specifically, on the way they influence managers' employment policies. Adverse dynamics in workplace relationships and dismissals are costly for the firm, cause earnings losses for the employees, and harm their well-being and future employment prospects as well. Whether these costs are surpassed by the potential benefits arising from better employer-employee matches on the longer term, is yet to be investigated. An agenda for the future is to decide, whether changes in managers' social preferences following a health shock are transitory or permanent, and what exactly drives the adverse employment consequences; whether it is something that can be avoided by better corporate policies, or simply by improved mediation techniques. If these changes are temporary, providing help and assistance to managers after important health shocks in their lives may prevent unnecessary dismissals. On the other hand, if the changes prove to be permanent, awareness of the phenomenon from the part of managers, employees, and the firm, may help assessing the necessity, and minimizing the costs of necessary adjustments in the employee pool. Concerning the adverse employment and wage effect of health shocks we found for the managers, it needs to be understood, whether they are linked to permanent decline in performance, or rather driven by a corporate culture that penalizes leave. In the latter case, increasing awareness of the issue may help in improving firms' decisions in this regard.

The remainder of the paper is organized as follows. Section 3.2 describes the dataset, Section 3.3 presents the identification strategy and provides definitions of the variables used during the estimation. Section 3.4 shows descriptive statistics, Section 3.5 presents and discusses the estimation results. Section 3.6 points out potential directions for future

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research.

3.2 Data and institutional settings

We use a large, longitudinal dataset linking administrative data from the Hungarian National Pension Insurance, the National Tax and Customs Administration, and the National Health Insurance Fund, originally compiled for the Centre for Economic and Regional Studies of the Hungarian Academy of Sciences. The original dataset contains a 50% random sample of Hungarian citizens of age 15-73 in 2003 covering the period 2003-2011. Due to a change in the occupation classification system, we omit the year 2011. Employee hierarchies are not included in the data, thus we proxy leader-employee relationships using occupational categories. The database contains information about the date of birth, gender, and the 2003 region of residence of individuals, alongside with monthly information about their employment status and labor income. In case an individual was employed in a given month, we observe an identifier of the company he or she worked at, allowing us to identify coworkers.

The National Health Insurance Fund provides information about public health care spending corresponding to individuals at a yearly frequency. In- and outpatient care and medication costs are recorded separately, medication costs are split to an out-of-pocket and a publicly financed part. This information should represent total health expenditures fairly accurately, as co-payments are not important in Hungary, and private health care providers only represented a meaningful market share in a handful of areas, such as gynecology or dentistry during the time period covered by the dataset. According to the calculations of the Hungarian Statistical Office[1] the share of government expenditures in total health expenditures was 69% in 2008.[2] Even out of the remaining 31%, paid by households and NGO's, We do observe out-of-pocket medication costs, amounting to at least 6 percentage points out of the 31%[3]. The remaining (maximum of) 25% of expenditures that we do not observe, contain estimated values of gratuities, which are tightly linked to state-financed health care interventions. (Gratuities are informal — and often large — payments made by

[1] in line with international statistical methodologies developed by WHO, OECD and Eurostat

[2] Data source: https://www.ksh.hu/docs/hun/xftp/stattukor/eukiadasok1015.pdf, downloaded on 30 Oct, 2018.

[3] Calculated by adding up all out-of-pocket medication costs in the sample, and doubling it, as my sample is a 50% random sample. Note, that this is an underestimation, as children are not covered by the dataset.

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patients to doctors and other health care employees when receiving state-financed health care services, a phenomenon widely present in the Hungarian health care system.) All in all, the public health care costs recorded in the database should constitute a close proxy for overall health expenditures.

3.2.1 Subsample of managers and employees

The International Standard Classification of Occupations (ISCO) distinguishes heads of units and other managers from subordinates in many fields, giving us a hint about employee hierarchies. Similarly to Caliendo et al. (2015), we base our analysis on the companies' hierarchical layers defined by occupation codes, but extend their concept by defining finer subgroups within the layers, instead of regarding the entire layer of supervisors as potential leaders of each employee at the lower levels. For example, an "accounting and financial services branch manager" is likely to be the superior of auditors and accountants of the same firm, but is unlikely to be responsible for "trade organizers". We use this information to assign employees to managers in two steps. First, we compile a list of managerial occupational codes and identify all the employee occupational codes that likely represent subordinates of these managers. This assignment is not a one-to-one relationship though, as e.g. department managers in construction and supervisors in construction may both supervise construction technicians or building block assemblers as well. By using a very fine resolution of job categories, we are able to have a closer look at the actual manager-employee relationships where individual layoff and exit decisions are most likely to be made, as opposed to focusing only on the top managers. Thus, our 34 managerial and 376 employee occupation codes generate 1127 manager-employee occupation code pairs in the actual data. In other words, managers have 33 types of employees on average, and each employee code is assigned to 3 type of leaders. For a full list of these pairs, see the Appendix. Second, based on the list of manager-employee occupation code pairs, for each employee, we identify all the managers working at the same company at the same time, who may potentially be his or her leader. Any observed decision regarding the potential dismissal of a given employee is then assigned to these managers.

Obviously, occupational codes do not provide sufficient information to uncover the pool of subordinates of a given manager with certainty. Often there are more than one managers with the same occupational code at the same firm, and also there are many occupation categories that may be supervised by more than one type of managers. Moreover, the data covers only half of the population, thus there are potential hidden leaders and employees in any firm in our sample. Altogether, our leader-employee assignments should be treated as a proxy. During the analysis of managerial decisions, we collect all employees assigned to a given manager and calculate their average exit rate. When more than one potential managers are observed for a given employee, we assume that each of them have equal responsibility for the employee, that is, they influence the probability of the employee leaving the firm to the same extent, in expected terms. Therefore, when calculating these averages, we weight the employees by the inverse of the number of managers assigned to them. [4]

3.2.2 Defining health shocks

The number of manager months with identified employees in our data is 6,758,534, representing the decisions of 131 thousand managers working at 62 thousand different firms. However, as we are after the causal effect of a one-time health shock affecting the manager's decisions, we created a subsample of managers with zero health costs in general, and a single year of high health expenditure. This leaves us with 9,896 individuals, working at a total of 8,107 firms. Altogether, this makes up for 517,612 observed manager months corresponding to managers with health shock. The gender composition is somewhat unbalanced, 58% of the managers being men. Our health cost category contains the total cost of inpatient medical care paid by the National Healthcare Fund for a given individual. The distribution of this variable is close to exponential, thus has many zeros, and many small values. When identifying healthy periods and illness episodes in the managers' history, individuals with zero health cost are considered healthy, and those with a health cost above the 95th percentile in the distribution of real inpatient care costs are regarded as ill. From all individual-months belonging to managers, for whom we observe at least one employee in at least one month in our dataset, 90% have a corresponding health expenditure of zero, and 4.5% belong to individuals who were considered substantially ill at that moment in time. Concerning individual health cost histories, 52% of our population of managers have never had positive health expenditures during these years, i.e. we can consider them

[4]As an example, imagine two managers in our dataset, who both work at the same firm during the same time period as an employee, and, based on their occupation code, they both may possibly be the leaders of this employee. In this case, we "divide" the employee between them, by assigning him a weight of 0.5. After assigning these weights to each employee based on the number of their potential leaders, we turn to the managers, and count their employees by summing up these fractions. In our example, both of our managers are assumed to have 0.5 employees, in case this employee is the only person at the firm at that time with an occupation code linked to the occupation code of these managers.

always healthy. We are mostly interested in managers, who experience a single one-year episode above the high cut-off, and have zero inpatient cost otherwise. They account for appr. 7.5% of our sample. The remaining 40.4% have different health cost histories, most of them have more than one year of non-zero inpatient cost.

3.3 Research design

3.3.1 Identification

Identifying the causal effect of medical condition on managerial decision making is not a straightforward exercise. The ideal thought experiment would randomly assign health shocks to managers, and monitor their prospective human resource decisions. Quite obviously, illness in real life is not assigned randomly, rather there are personality traits that are unobservable to the researcher, but do simultaneously affect one's health status, his or her employment possibilities and choices, and also his or her attitude towards their colleagues. An approximation of a random, unexpected illness episode in an administrative database similar to ours is to look for individuals who appear to be healthy throughout our entire observational period, apart from a single, short episode of illness. Specifically, all managers in our sample experience a health shock in a single year between 2005 and 2008. Shortness of this episode is important, because the time scope of the effect we are after is not ex ante known, thus we need clearly distinguished pre- and post-treatment phases.

As a first step, we estimate the average treatment effect on the treated using a fixed effects model, in which identification hinges on the exact timing of this shock. Each manager experiences the health shock only once, but not at the same time, and this time variation in the event makes it feasible to identify from cross-sectional variation after controlling for macro trends in separation rate. Similarly to Jacobson et al. (1993), we investigate the evolution of our outcome variable in each of the years following the treatment separately. That is, we define a set of dummy variables D_{it}^{k}, each of which takes up the value 1 only in the kth year following the treatment. Using these dummies we are able to capture, how larger or smaller the separation rate becomes in the treatment year, and in the first, second, ..., kth year following the treatment, compared to the pre-treatment average. We estimate the models with the inclusion of a control group including managers who never experience health care shocks throughout our observation period.

$$y_{it} = \zeta_i + m_t + \sum_{t \leq 0} D_{it}^k \delta_k + \alpha T_i + \sum_{t \leq 0} T_i * D_{it}^k \omega_k + \beta X_{it} + \epsilon_{it} \tag{3.1}$$

In Equation 3.1, y_{it} is the average monthly separation rate of manager i's employees conditional on not having separated before, in month t. To account for seasonality in separation rates, we include month dummies m_t. $T_i = 1$ for the treated and 0 for the control group, and the ω parameters capture the effect of treatment 1, 2, ..., k years after the treatment. X_{it} are control variables, including the log average in-patient, out-patient and medication costs for all employees of the given firm, except for the given manager himself. These variables aim to account for any over-time changes in firms, that may cause the illness of the manager, or at least influence its likelihood, and, potentially also influence the separation rate of employees.

We also estimate the model in a more parsimonious form, using an after dummy D_{it} for differentiating between the pre and post treatment periods, instead of separate time periods.

$$y_{it} = \zeta_i + m_t + \delta D_t + \alpha T_i + \omega D_t * T_i + \beta X_{it} + \epsilon_{it} \tag{3.2}$$

In both specifications, our identifying assumption is that the average change in the separation rate of the control group represents the counterfactual change in the treatment group if they had not experienced an illness episode. While this assumption is not directly testable, we believe that is reasonable given all the sample restrictions and the matching procedure we apply.

Matching procedure

We have already discussed the possibility of a bias arising from selection into illness, and the ways we intend to minimize this concern during the definition of our treatment group. In this difference in differences specification, there is another possible source of bias that may potentially corrupt our estimates. Individuals in our treated group may be affected by two sources of selection, neither of which are present in case of the control group. First, they may be selected in the sense that they were able to continue their careers as managers after suffering a bad health shock, which may not be true for the entire population of managers. Second, the time needed to recover from an illness may also be affected by unobservable differences. Therefore, by limiting our attention to managers who recovered from their illness by the second year, we introduce another sort of selection. Meanwhile, members in our control groups cannot be selected along these lines, and thus the two groups may be different in unobservable personality traits, such as perseverance and grit. Hence, in order to make control managers as similar to treated managers as possible, motivated by King and Nielsen (2016), we used coarsened exact matching (Iacus et al., 2012). This method stratifies the characteristic space and does exact matching on the cells calculating weights based on the number of observations in each cell. Stratas are formed based on the sex and the age of the manager at the time of the treatment, on the year of treatment – in order to tackle any potential time trend in separation rates originating in the labor market – and also on previous managerial decisions, namely, the separation rate in their employee pool during the two years preceding the health shock.

To prepare a matched sample, placebo illness years have to be assigned to the always healthy managers as the prior separation rates are used in the matching procedure. As the final sample of treated managers is restricted to those being managers in their prior two years to their illness year and having observable manager years after the illness year as well, we also discard the always healthy managers who — with respect to their assigned placebo illness year — do not satisfy these criteria. If a potential control manager has more than one possible placebo treatment year satisfying the above requirements, we allow the matching algorithm to choose from all of these manager – placebo treatment year combinations.

3.3.2 Sample restrictions

During the estimation, we restrict the sample for those managers for whom we observe separation rates for at least two years before, and at least one year after the health shock. Hence, managers who experience a health shock, but will not go back to a manager position during our observed period, where we could follow their behavior are not in our sample. Managers who start maternity benefit right after their health episode are excepted as well.

One adjustment that we needed to do in all of our specifications is the exclusion of the last month of all managerial employment spells. As shown on Figure C.1 in the Appendix, employee separation rates tend to be relatively high in the last year of managers' tenure in their job, possibly because the end of the managerial spell often goes hand in hand with the closure of the department or the firm, in which case most or all employees are dismissed over a short course of time. In order to avoid this to affect our results, we needed to eliminate this last moment of managers' tenure. As dropping the entire last years would decrease our sample size importantly, we decided to drop only the last month of spells. This small correction has proved to be enough to eliminate the phenomenon from the data.

3.3.3 Variables

Our outcome variable of interest is the separation rate of the manager's employees from one month to another, conditional on not having separated before:

$$\frac{n_{im}}{N_{im}} \tag{3.3}$$

where N_{im} is the number of employees in manager i's employee pool in month m, n_{im} is the number of employees leaving the firm by the next month. When calculating the denominator of the separation rate, that is, the size of the employee pool, we need to count all employees who work at the same firm, at the same time, as the given manager, and — based on his occupation code — supposedly at the same department. Also, when there are more than one managers, who could be associated to a given pool of employees, the pool is divided between them. Therefore, N_{im} — or N_{imc}, by adding a c index for the company, where manager i works in month m — is computed as

$$N_{imc} = \sum_{e} \frac{N_{emc}}{\sum_{l} N_{lmc}}$$

where N_{emc} is the number of employees working at company c in month m, with an occupation code e — the summation going over all employee occupation code e's that are considered as potential employees of manager i, based on our predefined manager-employee occupation code pairs. N_{lmc} is the number of managers working at company c in month m, with an occupation code l — the summation going over all leader occupation code l's that are considered as potential leaders of an employee with occupation code e, again, based on our predefined manager-employee occupation code pairs. The number of employees leaving the firm by the next month, n_{imc} is calculated analogously to N_{imc}, taking into account only the employees who will not work at the company by month $t + 1$. Note, that managers working at the same company, in the same position, at the same time, have the same employee separation rate, as we have no means to differentiate between their employees.

We chose manager-months to be our unit of measurement as opposed to using employee level hazard rates, because we intend to focus on the average effect on managerial decision making as opposed to the average impact of this phenomenon on employees. Information about the employee pool of managers who are about to leave the company themselves by the next month, are excluded, in order to avoid classifying the simultaneous dismissal of managers and employees as the manager's decision of dismissing his employee. As health information is only available at a yearly frequency, we aggregated separation rates to the yearly level as well, by calculating a simple average of the monthly values.

3.4 Descriptives

Number of managers	
Could match employees at least one month	131 139
Only one illness year	9 896
Matched to healthy	7 844
Observed before & after, and in years -1, -2	2 245
Matched to healthy	2 236

Table 3.1: Sample Restriction

There are 131,139 individuals working at a manager position to whom we could match an employee pool at least in one month, based on their occupation code and firm identifier. Out of them, 9,896 experienced a single health shock episode, and out of these 9,896, only 2,245 satisfied the requirements regarding their presence at the firm both during the two years preceding their treatment, and at some point after treatment. When comparing the evolution of separation rates of treated and non-treated managers, we need the restricted sample, but during the analysis of the employment outcomes of treated managers, we need all 9,896 treated managers in order to get a comprehensive picture. Therefore, the matching procedure was performed for both groups, and was based on — apart from demographic characteristics — prior wages for the large sample used for analyzing individual outcomes, and on prior separation rates for the restricted sample used for analyzing future separation rates. The procedure resulted in successfully matching 7,844 out of the 9,986, and 2,236 out of the 2,245 treated managers in the entire and the restricted sample, respectively. Figure C.2 in the Appendix shows the fitted probabilities of a health shock prior to the event year separately for the always healthy and for the treated managers, for the restricted sample. For the estimated probability of the health shock the age, gender and prior separation rates are used in a logit specification. After the coarsened exact matching using manager characteristics and prior separation rates the fitted probabilities show similar distribution for treated and non-treated on the matched sample.

Figure C.3 in the Appendix illustrates the employment outcomes faced by the 9,896 managers experiencing a single health shock, following their illness episode. Table C.1 compares these outcomes to the matched control group of always healthy managers. Twelve months after the first month of the treatment year, that is, in the following January, 66.64% of treated managers are still working in the same position at the same firm as in the beginning of the treatment year. This ratio gradually declines to 30% during the following three years. 4.23% of managers stay at their firm, but work in a new managerial occupation by the 13^{th} month, and another 5.14% get a non managerial occupation at the same firm. These figures increase over time: to 6.34% and 9.87% by the 49^{th} month, respectively. Those managers, who leave their job to work elsewhere, either work as managers at a new company (7.52% at the beginning of the first year after treatment, and 19.27% at the beginning of the fourth), or get a non managerial job (4.55% and 16.26% at the two, above mentioned points in time, respectively). Only 1.73% receive unemployment benefit and 2.36% become pensioners, however, this second figure goes up to 7.53% by the 49^{th} month. Comparing the frequency of these outcome scenarios to the experiences of matched always healthy managers, two phenomena are visible. First, at the beginning of the first year after treatment, the biggest difference is that the likelihood of staying at the same firm, in the same job is 5.56 percentage points lower for the managers who have gone through an illness episode, compared to their matched, always healthy counterparts. Also, they have 0.11 percentage points less chance to get a new managerial position at the same firm, which may be a sign of foregone promotions for the treated managers. Only a fraction of this disadvantage is compensated by the fact that they have a 0.46 percentage points higher change to get a managerial position at a new firm. By the 49^{th} month, treated managers have a lower chance of working at the same firm, as control managers, either in their original position, or in other managerial or non managerial positions: the differences are 5.45, 0.35, and 1.01 percentage points, respectively. Correspondingly, they have a 0.57 and a 1.03 percentage points higher chance for working at different firms in managerial and non-managerial occupations, respectively. Second, summing up figures in the last column shows that even four years are not enough for managers to entirely leave the effects of their illness episode behind: while 18.23% of them has no job by this time, the corresponding ratio is only 13.02% for the control group, and this difference is almost entirely coming from the difference in their likelihood of having a manager position.

Not only the above mentioned employment outcomes, but also wages seem to be affected by temporary illness episodes. To measure this effect, we estimate Equations (3.1) and (3.2) with the outcome variable being the manager's logged nominal wage conditional on staying in his position. As Table C.2 in the Appendix shows, illness episodes have a strong negative effect on managers' wages, as it decreases it by 13.4% compared to the wage evolution of their matched healthy counterparts. The effect is the largest in the first year after illness (14.6%), and gradually declines, it is 9.4% in the third year.

The average number of employees belonging to a manager was 8.6 for the treated managers and 8 for the controls. Although a slight decrease is visible in both figures, the difference in the differences – as tested later in Section 3.5 – is not significant. Separation rates have a mean value of 0.019 both in the treated and the control groups before the treatment, respectively, meaning that the chance of any given employee working under the supervision of managers in our sample to leave his job from one month to the next, is around 2%. These values change to 0.024 and 0.023 after the treatment, respectively. These figures only include values corresponding to the same managerial position that the manager had at the beginning of the treatment year.

	Before	After
Treated	0.0188	0.0244
Control	0.0189	0.0232

Table 3.2: Average separation rates

	Before	After
Treated	8.62	8.37
Control	7.99	7.82

Table 3.3: Average number of employees

3.5 Results

3.5.1 Regression results

Table C.3 in the Appendix presents estimation results from Equations (3.1) and (3.2), using the matched sample. As presented in column (1), the separation rate of managers experiencing a health shock increased by 0.15 percentage points more that those, who have

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not experienced such a shock. This is an 8% effect compared to the mean value 0.0188, and is highly significant. When estimated in our less parsimonious model, presented in column (2), each of the coefficients corresponding to the individual years are statistically insignificant. This suggests, that the sample size is, unfortunately, insufficient to determine the exact timing of the estimated effect.

3.5.2 Department downsizing?

This increase, however, may not necessarily mean a restructuring in the manager's employee pool, it may also simply indicate a decrease in the size of the employee pool. If a manager cannot perform his job at the same quality as before his illness episode, it may trigger a decrease in the number of his employees. To check, whether this is the case, we estimate the previous models with the number of employees being the outcome variable instead of the separation rate. Table C.4 in the Appendix provides the estimation results. The estimated coefficients are not statistically different from zero, hence we conclude that department downsizing does not account for the increase in the separation rates.[5]

3.5.3 Dismissal or voluntary leave?

An alternative explanation of the above results may involve deteriorating working conditions arising from the temporary illness of the manager — such as longer working hours or more stress as a result of worsening leadership — and employees leaving the firm voluntarily instead of being dismissed by the manager. We are unable to identify in the data, whether it was the employer or the employee who has decided about the termination of a working relationship. Also, even if we observed the legal nature of this termination, this decision is usually based on mutual dissatisfaction, the origin of which is often impossible to detect. However, one exercise that may lead us closer to verifying that the increase in the separation rate is primarily due to an increase in dismissals as opposed to voluntary leaves is to limit our attention to employees who are more likely to have been dismissed against their own intentions. Therefore we investigate the work history of the employees following the separation from their original employer, and classify the end of the working

[5]Note, that we cannot separate the employees among managers working at the same time, at the same firm, with the same occupation code; in case there are more than one such managers, we basically observe their average department size, but not if employees are moved from one manager to another. But that does not change our conclusion here, because separation rates are also averages in this sense.

relationship as a voluntary leave if the leaving employee experiences a wage increase of at least 10% within three months after leaving the firm, and a dismissal otherwise. Then we calculate the ratio of voluntarily leaving employees among all employees in the employee pool, and the ratio of dismissed employees within the employee pool. (Note, that the sum of these two figures is, by definition, the separation rate.) Estimating Equations (3.1) and (3.2) using voluntary leave rates and dismissal rates as the outcome variables instead of the separation rates should give us a hint on whether it is the employees or the managers, who initiate the termination more often after the illness episode of the latter. For these estimations, the coarsened exact matching procedure is repeated: instead of matching for prior separation rates, we match for the previous two years' dismissal rate or voluntary leave rate.

Approximately 10% of separations got classified as voluntary leave, and the remaining 90% are assumed to be dismissals. The average value of voluntary separation rate is 0.0029 (0.0027 in the matched sample), while the average dismissal rate is 0.025 (0.02 in the matched sample). The coefficients corresponding to the ratio of voluntarily leaving employees — presented in Table C.5 in the Appendix — are also one order of magnitude smaller, and only statistically significant at the 10% level in the model with pooled post-treatment years, and not statistically different from zero in the model estimating the effects separately for each post-treatment year. On the other hand, the coefficients corresponding to dismissal rates — presented in Table C.6 in the Appendix — are significantly positive and quantitatively even larger than the estimated coefficients regarding the effect of the illness episode on separation rates. As presented in column (1), the ratio of dismissed employees increased 0.26 percentage points more after the treatment for the treated managers than for the control managers, a 13% effect compared to the mean value. The effect seems to be the highest in the second year after the treatment (0.29 percentage points). A plausible explanation of this timing is that layoff decisions take time to be made and implemented.[6] All in all, we conclude, that the increased turnover we find among the employees of managers experiencing an illness episode, are most likely primarily driven by the decisions of the managers — partly because those decisions are more affected by the illness episode, than employee's voluntary leaves, but mostly because dismissals constitute a much larger share in all separations than voluntary leaves.[7]

[6]The period of notice regulated in law was between one and twelve months at that time, depending on the length of the employment relationship and the agreement between the two parties.

[7]Obviously, these figures are dependent on the way we defined voluntary leaves, and experimenting

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3.6 Directions for future research

There are several other aspects of managers' employment decisions that may potentially be changed by a transforming illness experience. Analyzing changes in the demographic composition of managers' employee pool – that is, in the ratio of e.g. females, young or elderly people, healthy and less healthy individuals among the managers employees – may shed light on whether or not managers become more empathetic towards population groups with a more vulnerable labor market status, or with people going through similar health shock experiences as themselves. The effect on fellow managers' wage evolution is also a factor to be explored, as it may tell us more about what exactly is happening in a firm, when a manager becomes temporarily ill. While these questions are beyond the scope of the present study, they may be addressed in a follow-up paper.

Thinking beyond our current opportunities, databases containing information about specific diseases would enable the researcher to differentiate between life threatening and less serious diseases, and, more importantly, between expected and unexpected hospital stays. For example, an elective surgery based on a pre-existing condition is less likely to cause a prompt change in one's behavior, than a sudden accident.

with different classifications may be an interesting exercise for the future.

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Appendix A

Appendix for Chapter 1

Figure A.1: Changes in the benefit schedule

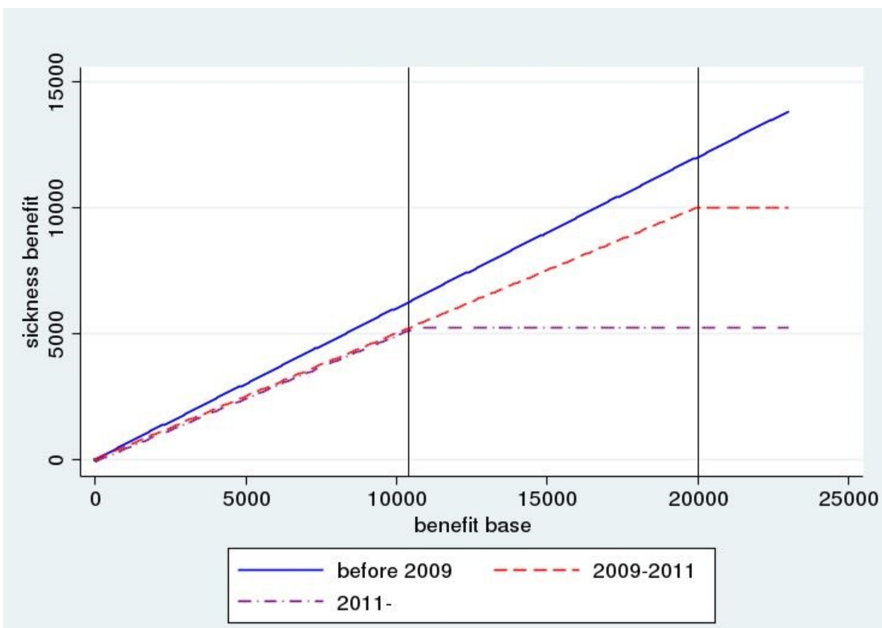


Figure A.2: Monthly average number of sick days among all ensurees

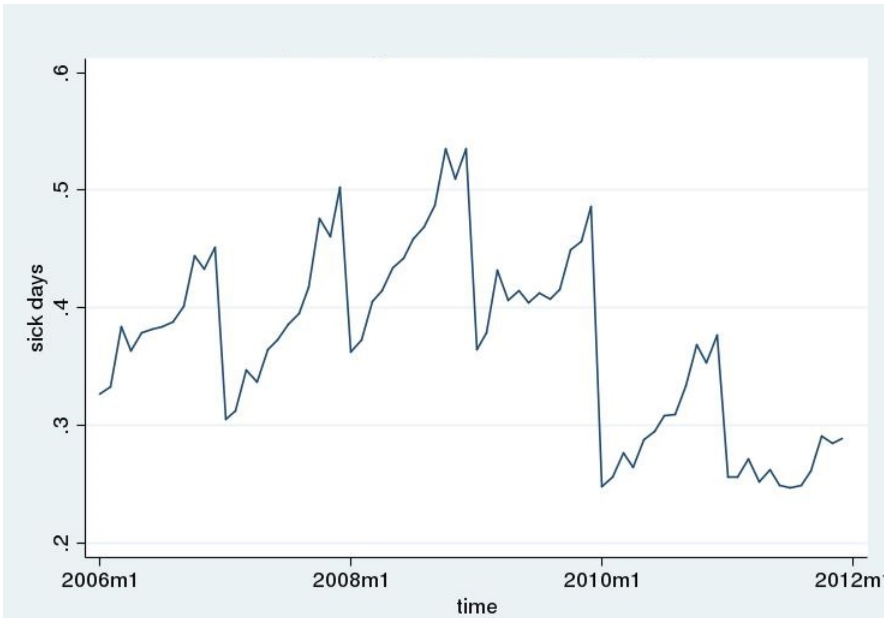
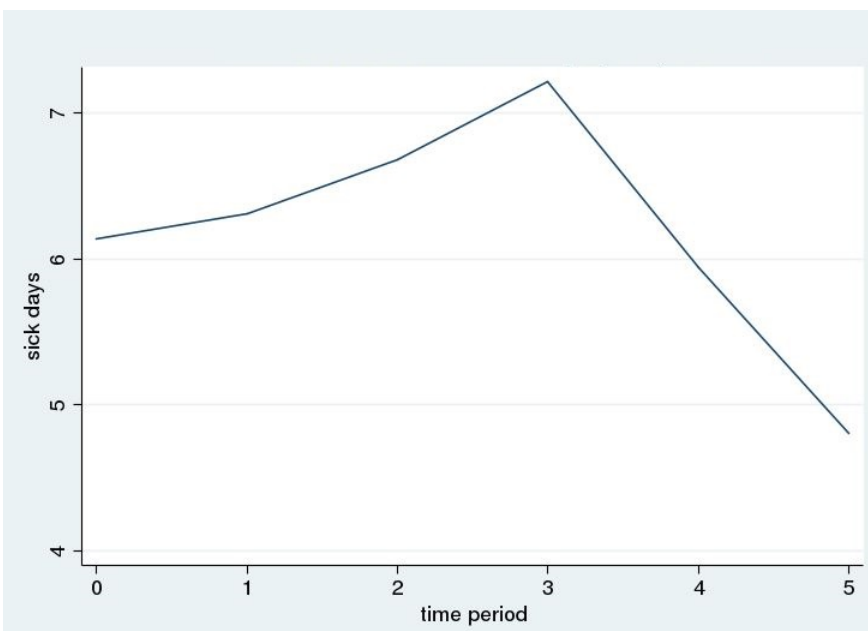


Figure A.3: Yearly average number of sick days in the estimation sample



Time periods on the horizontal axis are: 0 = August 2005 - July 2006, 1 = August 2006 - July 2007, 2 = August 2007 - July 2008, 3 = August 2008 - July 2009, 4 = August 2009 - July 2010, 5 = August 2010 - July 2011

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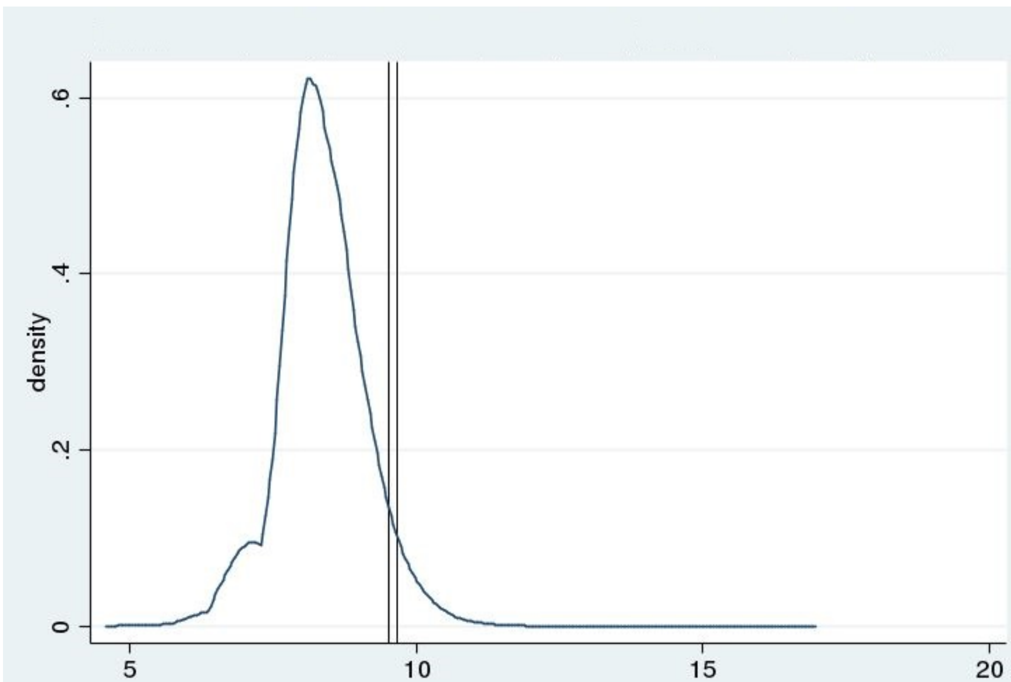
Figure A.4: Kernel density estimate of daily income



Based on 2009 log daily income. The two vertical lines correspond to the benefit cap introduced in 2009 August. The first one from the left represents the cap for individuals with 60% replacement rate, the second one corresponds to those having a replacement rate of 50%.

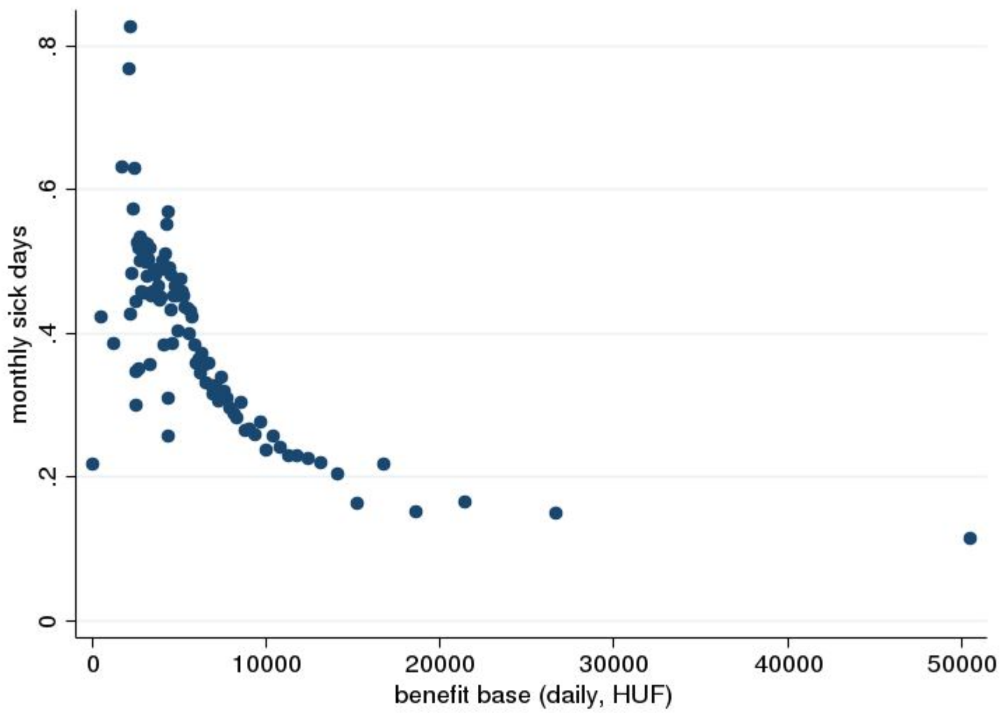


Figure A.5: Number of sick days and the benefit base, 2008

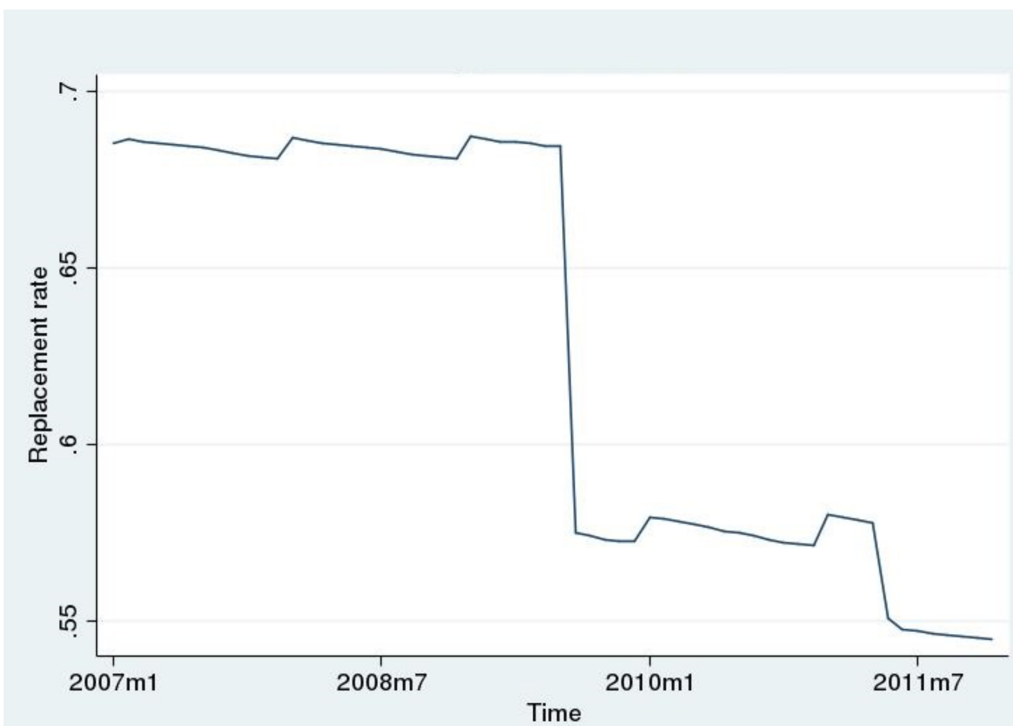


Figure A.6: Legislated and actual replacement rates

	All insurees	Estimation sample	Restricted sample (ceiling)	Restricted sample (ceiling, group)	Restricted sample, low r.r. group	Restricted sample, high r.r. group
Ratio of males	0.50 (0.500)	0.50 (0.500)	0.49 (0.500)	0.48 (0.499)	0.53 (0.499)	0.48 (0.499)
Age	40.52 (10.220)	40.63 (10.188)	40.43 (10.212)	42.09 (9.225)	35.71 (10.835)	41.45 (9.781)
Log income	11.51 (2.144)	11.62 (1.950)	11.43 (1.999)	11.73 (1.438)	11.10 (2.024)	11.50 (1.986)
Monthly sick leave days	0.49 (3.365)	0.48 (3.366)	0.53 (3.541)	0.31 (2.615)	0.41 (3.016)	0.56 (3.640)
Replacement rate	0.63 (0.080)	0.63 (0.080)	0.63 (0.074)	0.63 (0.080)	0.55 (0.050)	0.65 (0.065)
Number of observations	78,819,395	74,658,654	63,479,798	26,162,388	10,934,197	52,175,439
Number of individuals	2,036,365	2,011,320	1,796,302	744,589	1,013,609	1,387,030

Table A.1: Descriptive statistics of subsamples of the January 2007 - December 2011 period

10.14754/CEU.2019.01

	All insurees	Estimation sample	Restricted sample (ceiling)	Restricted sample (ceiling, group)	Restricted sample, low r.r. group	Restricted sample, high r.r. group
Ratio of males	0.50 (0.500)	0.50 (0.500)	0.50 (0.500)	0.49 (0.500)	0.54 (0.498)	0.49 (0.500)
Age	40.42 (10.214)	40.53 (10.186)	40.48 (10.183)	42.47 (9.350)	35.35 (10.827)	41.42 (9.776)
Log income	11.48 (2.180)	11.59 (1.995)	11.54 (1.989)	11.84 (1.471)	11.12 (2.027)	11.62 (1.972)
Monthly sick leave days	0.51 (3.438)	0.50 (3.441)	0.51 (3.474)	0.31 (2.603)	0.41 (3.053)	0.53 (3.542)
Replacement rate	0.64 (0.070)	0.64 (0.069)	0.64 (0.067)	0.64 (0.070)	0.56 (0.049)	0.66 (3.894)
Number of observations	68,505,082	64,926,108	62,675,379	29,735,890	9,407,700	52,895,522
Number of individuals	1,991,026	1,964,703	1,913,034	879,758	942,897	1,526,402

Table A.2: Descriptive statistics of subsamples of the January 2007 - April 2011 period

10.14754/CEU.2019.01

	All insurees	Estimation sample	Restricted sample (ceiling)	Restricted sample (ceiling, group)	Restricted sample, low r.r. group	Restricted sample, high r.r. group
Ratio of males	0.50	0.50	0.49	0.50	0.53	0.48
	(0.500)	(0.500)	(0.500)	(0.500)	(0.499)	(0.500)
Age	40.99	41.08	40.97	41.81	36.33	41.95
	(10.251)	(10.214)	(10.173)	(9.620)	(10.715)	(9.779)
Log income	11.52	11.66	11.60	11.83	11.14	11.70
	(2.272)	(1.908)	(1.901)	(1.446)	(2.134)	(1.832)
Monthly sick leave days	0.42	0.41	0.42	0.27	0.34	0.43
	(3.099)	(3.080)	(3.108)	(2.433)	(2.745)	(3.177)
Replacement rate	0.57	0.57	0.57	0.57	0.50	0.59
	(0.069)	(0.069)	(0.062)	(0.067)	(0.009)	(0.057)
Number of observations	37,479,651	35,405,403	33,821,063	22,216,437	5,787,197	27,917,314
Number of individuals	1,856,967	1,809,138	1,734,298	1,077,840	744,389	1,282,186

Table A.3: Descriptive statistics of subsamples of the August 2009 - December 2011 period

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	(1) Sick days	(2) Sick days	(3) Sick days	(4) Sick days	(5) Sick days	(6) Sick days
Replacement rate	1.132***	0.930***	0.933***	0.337***	0.220***	0.843***
	(0.0120)	(0.0103)	(0.0103)	(0.0133)	(0.0648)	(0.0145)
R^2	0.0003	0.115	0.115	0.0444	0.0269	0.174
Number of observations	71,201,113	71,201,113	71,201,113	25,332,237	9,953,641	49,886,842
Number of individuals	1,962,274	1,962,274	1,962,274	724,196	955,673	1,337,267
Controls	yes	yes	yes	yes	yes	yes
Fixed effects	yes	yes	yes	yes	yes	yes
Income & insurance time controls	no	yes	yes	yes	yes	yes
Sample restriction	no	no	no	no switch	low r.r. group	high r.r. group
Implied elasticity	1.473	1.211	1.214	0.680	0.298	0.980

Due to computational constraints, two digit NACE code dummies are replaced by 22 sector dummies created by grouping industrial sectors.
As tested on smaller samples, this has virtually no effect on the estimated coefficients. List of sectors are available upon request.
Standard errors in parentheses. Standard errors are clustered at the individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Estimation of elasticities for 2007-2011

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	(1) Sick days	(2) Sick days	(3) Sick days	(4) Sick days
Replacement rate	1.346***	0.603***	0.536***	1.227***
	(0.0135)	(0.0149)	(0.0657)	(0.0164)
R^2	0.120	0.0500	0.0171	0.169
Number of observations	61,807,788	28,813,862	8,558,497	50,702,512
Number of individuals	1,905,023	852,979	883,178	1,473,724
Controls	all	all	all	all
Fixed effects	yes	yes	yes	yes
Sample restriction	no	no switch	low r.r. group	high r.r. group
Implied elasticity	1.711	1.255	0.724	1.518

Standard errors in parentheses. Standard errors are clustered at the individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Estimation of elasticities for January 2007 - April 2011

	(1) Sick days	(2) Sick days	(3) Sick days	(4) Sick days
Replacement rate	0.418***	0.237***	1.036***	0.384***
	(0.0168)	(0.0217)	(0.1831)	(0.0212)
R^2	0.0934	0.0215	0.0098	0.151
Number of observations	33,792,718	21,537,349	5,252,581	26,891,885
Number of individuals	1,731,970	1,042,950	686,322	1,227,150
Controls	all	all	all	all
Fixed effects	yes	yes	yes	yes
Sample restriction	no	no switch	low r.r. group	high r.r. group
Implied elasticity	0.580	0.497	1.507	0.524

Standard errors in parentheses. Standard errors are clustered at the individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Estimation of elasticities for August 2009 - December 2011

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	(1)	(2)	(3)	(4)
	Δ Sick days	Δ Sick days	Δ Sick days	Δ Sick days
Change in replacement rate	0.0982***	0.0740***	0.137**	0.0810
	(0.0193)	(0.0203)	(0.0570)	(0.0674)
Adjusted R^2	0.006	0.007	0.002	0.003
F-test	6.26	5.44	3.90	4.57
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Number of individuals	865,028	823,587	178,394	166,203
Corresponding second stage	own health	own health	colleagues	colleagues
Sample restriction	no	yes	no	yes

Standard errors in parentheses. Standard errors are robust in column (1) and (2).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: First stage regressions

	(1) Inpatient	(2) Outpatient	(3) Medicine	(4) Inpatient	(5) Outpatient	(6) Medicine
Change in sick days	0.0742	1.118	2.822**	-0.0627	-0.243	2.398
	(0.906)	(1.010)	(1.096)	(1.289)	(1.415)	(1.489)
Number of individuals	865,028	865,028	865,028	823,587	823,587	823,587
Controls	yes	yes	yes	yes	yes	yes
Sample restriction	no	no	no	yes	yes	yes
IV	change in r.r.	change in r.r.	change in r.r.	change in r.r.	change in r.r.	change in r.r.

Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Effect on own health outcomes

	(1) Inpatient	(2) Outpatient	(3) Medicine	(4) Inpatient	(5) Outpatient	(6) Medicine
Change in sick days	20.43**	6.369**	8.877**	13.14	2.982	4.833
	(9.780)	(3.240)	(4.266)	(12.24)	(3.558)	(4.679)
Number of individuals	147,671	147,671	147,671	137,451	137,451	137,451
Controls	yes	yes	yes	yes	yes	yes
Sample restriction	no	no	no	yes	yes	yes
IV	change in r.r.	change in r.r.	change in r.r.	change in r.r.	change in r.r.	change in r.r.

Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Effect on colleagues' health outcomes

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	(1) Sick days	(2) Sick days	(3) Sick days	(4) Sick days
Replacement rate	0.903***	0.453***	0.230***	0.755***
	(0.0146)	(0.0165)	(0.2299)	(0.0174)
R^2	0.0873	0.0331	0.0107	0.144
Number of observations	31,305,831	14,053,296	4,720,973	25,039,313
Number of individuals	953,975	433,555	472,960	715,145
Controls	all	all	all	all
Fixed effects	yes	yes	yes	yes
Sample restriction	no	no switch	low r.r. group	high r.r. group
Implied elasticity	1.494	1.077	0.418	1.197

Standard errors in parentheses. Standard errors are clustered at the individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Estimation of elasticities for January 2007 - April 2011, males

	(1) Sick days	(2) Sick days	(3) Sick days	(4) Sick days
Replacement rate	0.319***	0.266***	0.640***	0.278***
	(0.0181)	(0.0229)	(0.1683)	(0.0224)
R^2	0.0706	0.0109	0.0011	0.142
Number of observations	16,993,109	10,748,003	2,856,029	13,178,665
Number of individuals	876,135	537,471	368,113	596,413
Controls	all	all	all	all
Fixed effects	yes	yes	yes	yes
Sample restriction	no	no switch	low r.r. group	high r.r. group
Implied elasticity	0.544	0.647	1.200	0.458

Standard errors in parentheses. Standard errors are clustered at the individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Estimation of elasticities for August 2009 - December 2011, males

	(1) Sick days	(2) Sick days	(3) Sick days	(4) Sick days
Replacement rate	1.924***	0.886***	0.946***	1.880***
	(0.0257)	(0.0286)	(0.1152)	(0.0317)
R^2	0.120	0.0500	0.0171	0.169
Number of observations	30,501,957	14,760,566	3,837,524	25,663,199
Number of individuals	951,048	419,424	410,218	758,579
Controls	all	all	all	all
Fixed effects	yes	yes	yes	yes
Sample restriction	no	no switch	low r.r. group	high r.r. group
Implied elasticity	1.992	1.645	0.981	1.924

Standard errors in parentheses. Standard errors are clustered at the individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Estimation of elasticities for January 2007 - April 2011, females

	(1) Sick days	(2) Sick days	(3) Sick days	(4) Sick days
Replacement rate	0.426***	0.089**	1.389***	0.385***
	(0.0320)	(0.0435)	(0.4005)	(0.0425)
R^2	0.0767	0.0317	0.0120	0.136
Number of observations	16,799,609	10,789,346	2,396,552	13,713,220
Number of individuals	855,835	505,479	318,209	630,737
Controls	all	all	all	all
Fixed effects	yes	yes	yes	yes
Sample restriction	no	no switch	low r.r. group	high r.r. group
Implied elasticity	0.504	0.165	1.621	0.453

Standard errors in parentheses. Standard errors are clustered at the individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Estimation of elasticities for August 2009 - December 2011, females

Appendix B

Appendix for Chapter 2

Figure B.1: Distribution of the aggregated number of sick days during pregnancy

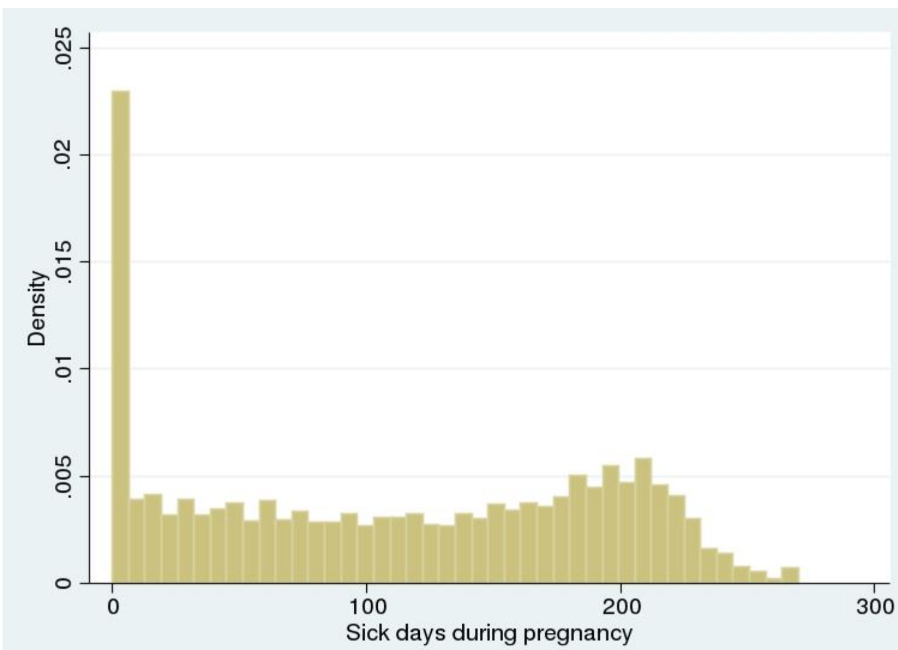

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Figure B.2: Ratio of pregnant women on sick leave 0-270 days before maternity leave

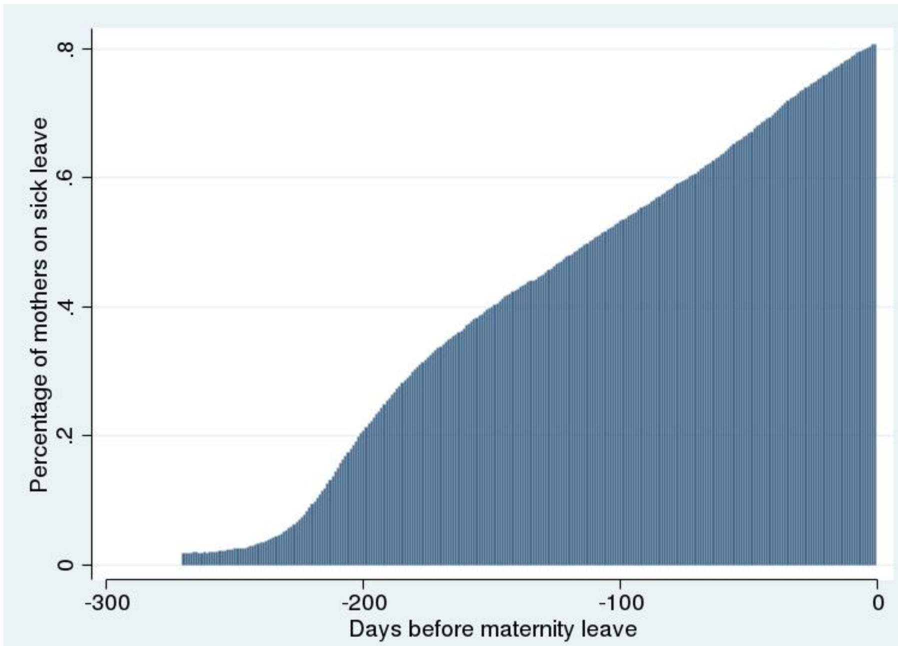

Figure B.3: Distribution of mothers' previous years' earnings (zero income levels are replaced with zeros in log)

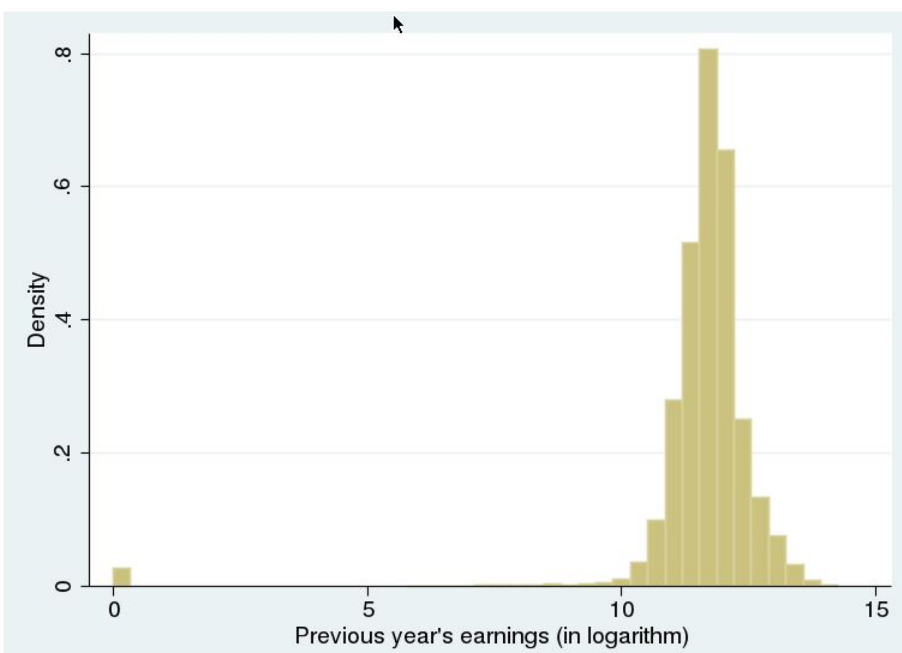

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Figure B.4: Distribution of firm size proxy

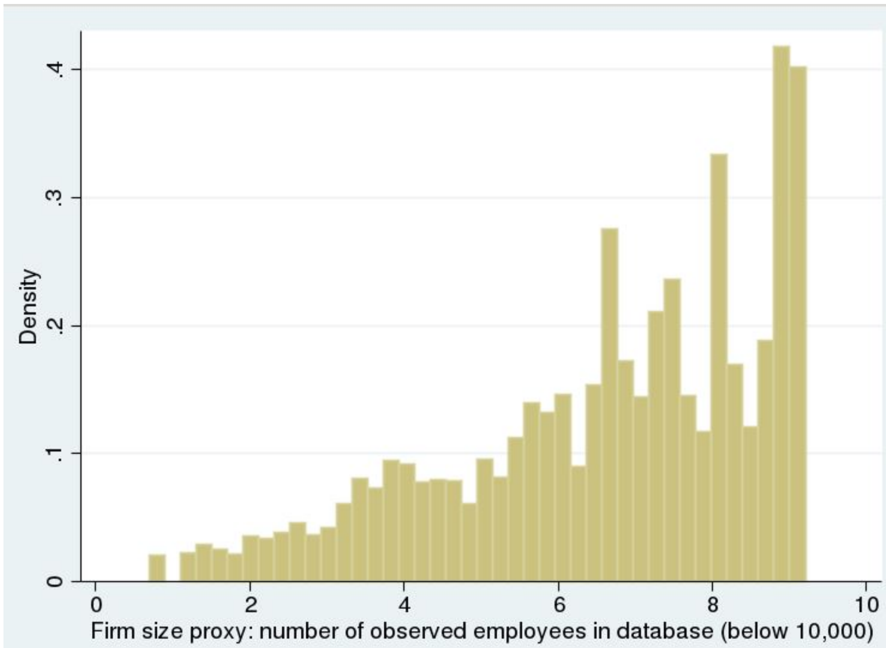

Figure B.5: Distribution of firm data coverage

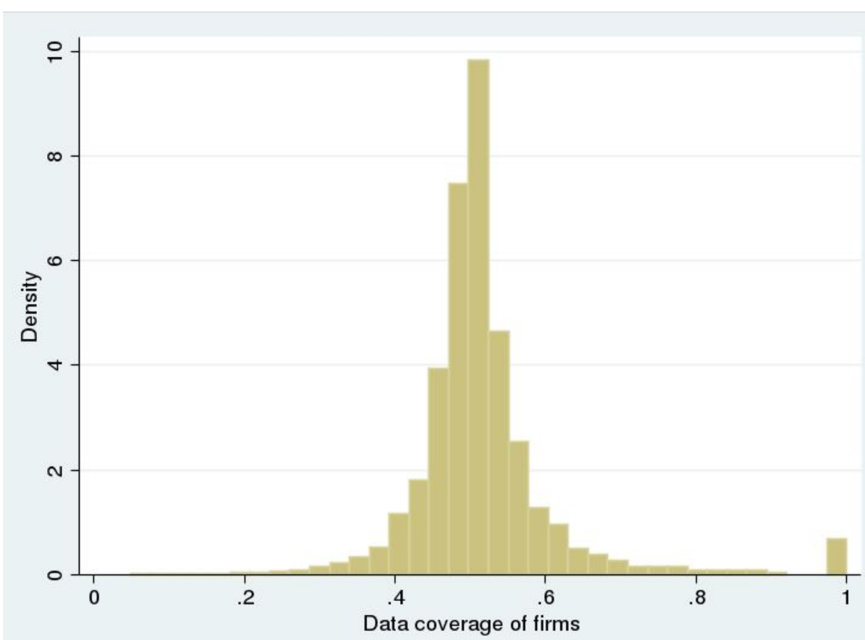

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Table B.1: Most frequent occupations

Occupation	No.	%
Shop assistants	1,848	10.7
Primary school teachers	1,529	8.8
General nurses	1,417	8.2
Specialized nurses	1,360	7.9
Secondary school teachers, instructors	800	4.6
Shop cashiers	675	3.9
Third-level education teaching professionals	598	3.5
(e.g. university or college professors, associate		
Specialized medical assistants	471	2.7
Finance clerks	452	2.6
Kindergarten teachers	424	2.4
Analytic bookkeeping clerks	369	2.1
Market researcher, advertising and PR occupations	351	2.0
General practitioners	329	1.9
Accounting clerks	323	1.9
Waiters, restaurant salespersons	308	1.8
Health and educational services workers	283	1.6
(e.g. assistant nurses, ambulance men, hospital orderlies, nannies)		
Professional nurses	280	1.6
General medical assistants	267	1.5
Trade clerks	257	1.5
All of the above combined	12,341	71.3
Total	17,322	100.0

Data in this table represent observations from 2003-2010, because of the change in the occupation classification system as of 2011. The list of most frequent occupations in the 2011 sample is available upon request.

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Table B.2: Regression results

	(1) Sick leave	(2) Sick leave	(3) Sick leave	(4) Sick leave
Leader is a new parent	-9.617**	-11.49***	-11.72***	-11.16**
	(4.371)	(4.236)	(4.233)	(4.427)
Leader's age				0.0903
				(0.0990)
Leader is male				-0.529
				(1.839)
Observations	18932	18220	18220	18220
Adjusted R^2	0.019	0.284	0.292	0.292
Number of explanatory variables	93	287	344	346
Individual controls	no	yes	yes	yes
Firm level controls	no	no	yes	yes
Time dummies	yes	yes	yes	yes
Previous health condition	no	yes	yes	yes
Leader pool controls	no	no	no	yes

Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table B.3: Regression results using various sample restrictions

	(1) Sick leave	(2) Sick leave	(3) Sick leave
Leader is a new parent	-12.52***	-7.302	-10.46**
	(4.388)	(5.119)	(5.237)
Leader's age	0.0852	0.0116	-0.238**
	(0.0961)	(0.108)	(0.110)
Leader is male	-1.025	0.841	-0.868
	(1.776)	(2.036)	(2.000)
Observations	27175	9655	9655
Adjusted R^2	0.267	0.302	0.171
Number of explanatory variables	352	333	167
Individual controls	yes	yes	yes
Firm level controls	yes	yes	yes
Time dummies	yes	yes	yes
Previous health condition	yes	yes	yes
Leader pool controls	yes	yes	yes
Large firms included	yes	no	no
No mixed leader groups	no	yes	yes
ISCO control	yes	yes	no

Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Regression results for worse and better covered firms

	(1) Sick leave	(2) Sick leave	(3) Sick leave	(4) Sick leave
Leader is a new parent	-13.63***	-7.668	6.445	-22.53**
	(4.587)	(7.998)	(11.46)	(10.69)
Observations	20831	3907	1817	2166
Adjusted R^2	0.098	0.155	0.147	0.165
Number of explanatory variables	33	33	33	353
Time dummies	year, month	year, month	year, month	year, month
Sectors covered	all	all	private	private
Data coverage	all	all	below 0.5	minimum 0.5

Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table B.5: Regression results using only female supervisors

	(1) Sick leave	(2) Sick leave	(3) Sick leave	(4) Sick leave
Leader is a new parent	-8.496**	-11.10***	-11.70***	-11.55***
	(3.670)	(3.502)	(3.530)	(3.699)
Leader's age				0.0143
				(0.110)
Observations	25406	24517	24517	24517
Adjusted R^2	0.022	0.262	0.266	0.266
Number of explanatory variables	92	284	339	340
Individual controls	no	yes	yes	yes
Firm level controls	no	no	yes	yes
Time dummies	yes	yes	yes	yes
Previous health condition	no	yes	yes	yes
Leader pool controls	no	no	no	yes

Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Regression results using various sample restrictions and only female supervisors

	(1) Sick leave	(2) Sick leave	(3) Sick leave
Leader is a new parent	-11.52***	-9.753**	-9.608**
	(3.699)	(4.356)	(4.425)
Leader's age	0.0150	-0.0863	-0.0978
	(0.110)	(0.126)	(0.127)
Observations	24518	7419	7419
Adjusted R^2	0.266	0.313	0.165
Number of explanatory variables	340	314	164
Individual controls	yes	yes	yes
Firm level controls	yes	yes	yes
Time dummies	yes	yes	yes
Previous health condition	yes	yes	yes
Leader pool controls	yes	yes	yes
Large firms included	yes	no	no
No mixed leader groups	no	yes	yes
ISCO control	yes	yes	no

Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: The effect of supervisors' family status on male employees' sick leave take-up

	(1) Sick leave	(2) Sick leave	(3) Sick leave	(4) Sick leave
Leader is a new parent	0.0636	0.00414	-0.00351	0.00887
	(0.0478)	(0.0554)	(0.0551)	(0.0549)
	0.183	*0.940*	*0.949*	*0.872*
Observations	614548	556613	556613	556613
Adjusted R^2	0.001	0.017	0.018	0.018
Number of explanatory variables	107	216	271	273
Time dummies	yes	yes	yes	yes
Individual controls	no	yes	yes	yes
Firm level controls	no	no	yes	yes
Previous health condition	no	yes	yes	yes
Leader pool controls	no	no	no	yes

Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.8: The effect of sick benefit during pregnancy, induced by supervisors' parental status, on medium term health outcomes

	(1) Inpatient	(2) Outpatient	(3) Medical
Sick leave	-0.00369	0.0103	-0.00765
	(0.0254)	(0.0199)	(0.0207)
	0.884	*0.607*	*0.711*
Observations	15434	15438	15437
Individual controls	yes	yes	yes
Firm level controls	yes	yes	yes
Time dummies	yes	yes	yes
Previous health condition	yes	yes	yes
Leader pool controls	yes	yes	yes
IV	parent leaders	parent leaders	parent leaders

Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table B.9: Predefined HSCO pairs: HSCO-93 for 2003-2010

HSCO	Supervisor	HSCO	Employee
1325	Department managers of restaurants and hotels	3643	Hotel porters, receptionists
1415	General managers of small undertakings in restaurants and hotels	4291	Client information clerks
		5122	Confectioners
		5123	Waiters, restaurant salespersons
		5124	Cooks
		5125	Chambermaids
		5129	Hotels and restaurants workers n.e.c.
		9114	Kitchen helpers
1333	Department managers in health care and welfare services	2211	General practitioners
		2212	Specialized medical doctors
		2213	Dentists
		2214	Specialized dentists
		2215	Pharmacists
		2216	Specialized pharmacists
		2222	Optometrists
		2223	Dieticians
		2224	Physiotherapist
		2225	Institution based nurses
		2226	Ambulance attendance
		2229	Human health related professionals n.e.c.
		2230	Professional nurses
		2432	Kindergarten teachers
		2441	Teachers for the handicapped
		2442	Teachers for the physically disabled
		2443	Health educators
		3211	General nurses
		3212	Specialized nurses
		3221	Personal care workers
		3222	Specialized personal care workers
		3231	General medical assistants
		3232	Specialized medical assistants
		3233	Dental assistants
		3234	Pharmaceutical assistants
		3235	Specialized medicine supply assistants
		3239	Medical assistants n.e.c.
		3242	Midwives
		3244	Dieticians
		3248	Dental mechanic
		3311	Welfare assistants
		3312	Mental hygiene assistants
		3313	Welfare care workers
		3319	Welfare associate professionals n.e.c.
		3412	Child- and youth-care associate professionals
		3414	Health education assistants
		3415	Assistant for the education of the challenged/handicapped
		5314	Masseurs
		5320	Health and educational services workers (e.g. assistant nurses, ambulance men, hospital
1334	Department managers in education	2410	Third-level education teaching professionals (e.g. university or college professors, associate professors, assistant professors)
		2421	Secondary school teachers, instructors
		2422	Secondary level vocational training instructors
		2429	Secondary education teaching professionals n.e.c.
		2431	Primary school teachers
		2432	Kindergarten teachers
		2432	Kindergarten teachers
		2439	Primary education teaching professionals n.e.c.
		2441	Teachers for the handicapped
		2442	Teachers for the physically disabled
		2443	Health educators
		2449	Special education teaching professionals n.e.c. (e.g. psycho-pedagogical teachers)
		2491	Education specialists, school inspectors
		2499	Teaching professionals n.e.c. (e.g. welfare instructors, vocational training instructors in a company, irrespective of the educational level)
		3411	Teachers without third-level qualification
		3412	Child- and youth-care associate professionals
		3413	Pedagogical assistants
		3414	Health education assistants
		3415	Assistant for the education of the challenged/handicapped
		3419	Teaching associate professionals n.e.c.
		5320	Health and educational services workers (e.g. assistant nurses, ambulance men, hospital

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HSCO	Supervisor	HSCO	Employee
1342	Accountancy and finance managers	2512	Tax advisors, consultants
		2513	Financial and credit organizers
		2514	Auditors
		2518	Auditors
		3604	Wage and social security accounting clerks
		3605	Finance clerks
		3606	Accounting clerks
		4111	Analytic bookkeeping clerks
		4119	Analytic accounting clerks n.e.c.
		4121	Stock clerks
		4122	Financial, personnel clerks
1343	Human resources (personnel) managers	2523	Personnel organizers
		3603	Personnel clerks
		4112	Payroll clerks
		4122	Financial, personnel clerks
1344	Advertising and other public relations managers	2521	Market researcher, advertising and PR occupations
		3622	Exhibition and advertising clerks
1347	Computing services managers	2131	Computer science professionals (e.g. systems-analysts, operations-research analysts)
		2132	Electronic data processing organizers
		2133	Software developers
		2139	Other computing professionals with third-level qualification, n.e.c.
		3131	Computer-network operators
		3132	Computer programmers
		3133	Database managers
		3139	Computer associate professionals n.e.c.
		3141	Schedule programmers
1335	Department managers in cultural services	1611	Librarians
		2612	Archivists
		2613	Curators (restorers, taxidermists)
		2614	Cultural organizers
		2615	Book and newspaper editors
		2616	Journalists
		2617	Broadcasting editors (radio & TV)
		2619	Cultural proffessionals n.e.c.
		2621	Writers (except journalists)
		2622	Literary translators
		2623	Sculptors, painters and related artists
		2624	Industrial designers
		2625	Composers
		2626	Film, stage and related directors
		2627	Cameramen, artistic photographers
		2629	Creative artists n.e.c.
		2631	Actors, stage performing artists, puppet artists
		2632	Musicians, singers
		2633	Choreographers, dancers
		2639	Performing artists n.e.c.
		3711	Library assistants
		3712	Archivist assistants
		3713	Cultural organizer assistants
		3714	Broadcasting (radio, TV) assistant editors
		3715	Book and newspaper assistant editors
		3717	Translators, interpreters
		3719	Cultural associate professionals n.e.c.
		3721	Supporting actors
		3722	Film, stage and related assistant directors
		3723	Folk musicians
		3724	Restaurant and night-club musicians
		3725	Circus artists
		3729	Artistic associate professionals n.e.c.
		5341	Photographers, photo and film developers
		5342	Light technicians and other motion picture workers
		5343	Scenery shifters
		5344	Cinema projectionists
		5349	Cultural, sports and entertainment services workers n.e.c.

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HSCO	Supervisor	HSCO	Employee
1354	Supervisors in wholesale and retail trade, restaurants and hotels	2517	Trade organizers
		3621	Trade clerks
		3643	Hotel porters, receptionists
		4212	Shop cashiers
		4291	Client information clerks
		5111	Shopkeepers
		5112	Shop assistants
		5119	Wholesale and retail trade workers n.e.c.
		5122	Confectioners
		5123	Waiters, restaurant salespersons
		5124	Cooks
		5125	Chambermaids
		5129	Hotels and restaurants workers n.e.c.
		9114	Kitchen helpers
		9131	Manual materials handlers, hand packers
1414	General managers of small undertakings in wholesale and retail trade	2517	Trade organizers
		3621	Trade clerks
		4212	Shop cashiers
		5111	Shopkeepers
		5112	Shop assistants
		5119	Wholesale and retail trade workers n.e.c.
		9131	Manual materials handlers, hand packers
5121	Restaurant managers, restaurateurs	5123	Waiters, restaurant salespersons
		5124	Cooks
		5129	Hotels and restaurants workers n.e.c.
		9114	Kitchen helpers
1425	General managers of small undertakings in cultural services	2611	Librarians
		2612	Archivists
		2613	Curators (restorers, taxidermists)
		2614	Cultural organizers
		2615	Book and newspaper editors
		2616	Journalists
		2617	Broadcasting editors (radio, TV)
		2619	Cultural proffessionals n.e.c.
		2621	Writers (except journalists)
		2622	Literary translators
		2623	Sculptors, painters and related artists
		2624	Industrial designers
		2625	Composers
		2626	Film, stage and related directors
		2627	Cameramen, artistic photographers
		2629	Creative artists n.e.c.
		2631	Actors, stage performing artists, puppet artists
		2632	Musicians, singers
		2633	Choreographers, dancers
		2639	Performing artists n.e.c.
		3711	Library assistants
		3712	Archivist assistants
		3713	Cultural organizer assistants
		3714	Broadcasting (radio, TV) assistant editors
		3715	Book and newspaper assistant editors
		3717	Translators, interpreters
		3719	Cultural associate professionals n.e.c.
		3721	Supporting actors
		3722	Film, stage and related assistant directors
		3723	Folk musicians
		3724	Restaurant and night-club musicians
		3725	Circus artists
		3729	Artistic associate professionals n.e.c.
		5341	Photographers, photo and film developers
		5342	Light technicians and other motion picture workers
		5343	Scenery shifters
		5344	Cinema projectionists
		5349	Cultural, sports and entertainment services workers n.e.c.

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Table B.10: Predefined HSCO pairs: HSCO-08 for 2011

HSCO	Supervisor	HSCO	Employee
1322	Information and communications technology service manager	2123	Telecommunications engineer
		2136	Graphic and multimedia designer
		2141	System analyst (information technology)
		2151	Database designer and operator
		2152	System administrator
		2153	Computer network analyst, operator
		2159	Other database and network analyst, operator
		3141	Information and communications technology operations technician
		3142	Information and communications technology user support technician
		3143	Computer network and systems technician
		3144	Web technician
		3145	Broadcasting and audio-visual technician
		3146	Telecommunications engineering technician
		7342	Information and communications technology installer and repairer
1323	Banking manager	3612	Banking administrator
		3613	Stock exchange and finance representative, broker
1324	Social welfare manager	3511	Social assistant
		3513	Social services assistant, special social assistant
		3515	Youth assistant
1325	Childcare service manager	2432	Early childhood educator
		2441	Special needs teacher
1326	Aged care service manager	5223	Home personal care worker
		5229	Other personal care worker
1327	Health service manager	2211	General practitioner
		2212	Specialized medical doctor
		2213	Dentist, specialized dentist
		2214	Pharmacist, specialized pharmacist
		2222	Optometrist
		2223	Dietician and nutrition adviser
		2224	Physiotherapist
		2225	District nurse
		2226	Ambulance officer
		2227	Audiologist and speech therapist
		2228	Complementary medicine professional
		2229	Other human health (related) professional
		2231	Nursing professional
		2232	Midwifery professional
		2410	University and higher education teacher
		2441	Special needs teacher
		2442	Conductor
		3311	Nursing associate professional
		3312	Midwifery associate professional
		3321	Medical assistant
		3322	Health care documentarist
		3323	Operator of medical imaging diagnostic and therapeutic equipment
		3324	Medical laboratory assistant
		3325	Dental assistant
		3326	Pharmacy and pharmaceutical supplies assistant
		3333	Dental technician
		3339	Other human health care related professional
		5222	Assistant nurse, dresser
		5223	Home personal care worker
		5229	Other personal care worker
1328	Educational manager	2421	Secondary education teacher
		2422	Vocational education teacher
		2431	Primary school teacher
		2432	Early childhood educator
		2441	Special needs teacher
		2442	Conductor
		2491	Education expert, school adviser
		2492	Language teacher (outside the educational system)
		2493	Music teacher (outside the educational system)
		2494	Teacher of other arts (outside the educational system)
		2495	Teacher of information technology (outside the educational system)
		2499	Other specialized teacher, educator
		3410	Educational assistant
		5221	Babysitter, nurse

HSCO	Supervisor	HSCO	Employee
1331	Hotel manager	4222	Receptionist
		4223	Hotel receptionist
		9112	Cleaner and helper in offices, hotels and other establishments
1332	Restaurant manager	3222	Head-cook, chef
5131	Restaurant keeper	5132	Waiter
		5133	Bartender
		5134	Cook
		5135	Confectioner
		9112	Cleaner and helper in offices, hotels and other establishments
		9235	Fast food restaurant assistant
		9236	Kitchen helper
1333	Sales and marketing manager	2534	Information and communications technology sales professional
		3622	Sales administrator
		5111	Shopkeeper
		5112	Shop supervisor
		5113	Shop salesperson
		5117	Shop cashier, ticket clerk
		5129	Other commercial occupation, not elsewhere classified
		9224	Counter and shelf filler
1335	Cultural centre manager	2136	Graphic and multimedia designer
		2627	Linguist, translator, interpreter
		2711	Librarian, information specialist librarian
		2712	Archivist
		2713	Museologist, museum collection curator
		2714	Cultural organizer
		2715	Editor of book and magazine publication
		2716	Journalist, editor of radio and television broadcast
		2717	Specialized coach, sports organizer, manager
		2721	Writer (except journalists)
		2722	Artist
		2723	Artist-craftsman, industrial designer, clothes-designer
		2724	Composer, musician, singer
		2725	Director, director of photography
		2726	Actor, puppet player
		2727	Dancer, choreographer
		2728	Artist in circus and in similar performing arts
		2729	Other creative and performing arts professional
		3514	Signing interpreter
		3711	Supernumerary, extra
		3712	Assistant director
		3713	Photographer
		3714	Scenery shifter, decorator
		3715	Complementary film producing and theatre occupation
		3717	Special technician in cultural institutions
		3719	Other arts and cultural professional
		8137	Photographs and films laboratory assistant
		8326	Cinema operator, projectionist
1336	Sports and recreational centre manager	2719	Other culture and sports professional
		3332	Physiotherapist assistant, masseur/masseuse
		3721	Athlete and sports player
		3722	Fitness and recreation instructors and programme leader
1411	Accounting and financial services branch manager	2511	Finance analyst and investment adviser
		2512	Tax adviser, tax consultant
		2513	Auditor, accountant
		2514	Controller
		3611	Finance administrator (except banking administrator)
		3614	Accounting administrator
		4121	Accountant (analytical)
		4123	Finance, statistics, insurance administrator
		4129	Other accounting worker
		4131	Stocks and materials clerk

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HSCO	Supervisor	HSCO	Employee
1412	HR manager	2523	Personnel and careers professional
		2524	Training and staff development professional
		4122	Payroll clerk
		4134	Human policy administrator
1414	Policy and planning manager	2521	Management and organization analyst, organizer
		2522	Business policy analyst, organizer
1415	Retail and wholesale trade manager	2531	Advertising and marketing professional
		2533	Sales professional
		2534	Information and communications technology sales professional
1416	Advertising and PR manager	2532	Public relations professional
		3632	Marketing and PR administrator

Appendix C

Appendix for Chapter 3

Figure C.1: Employee separation rates by length of manager spells

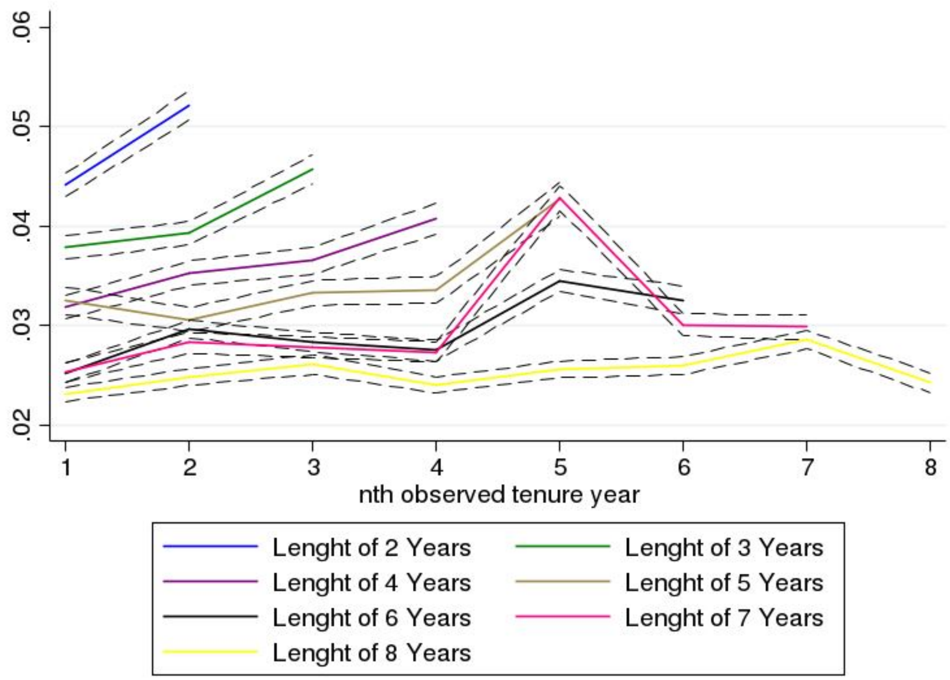

Figure C.2: Predicted probability of being treated

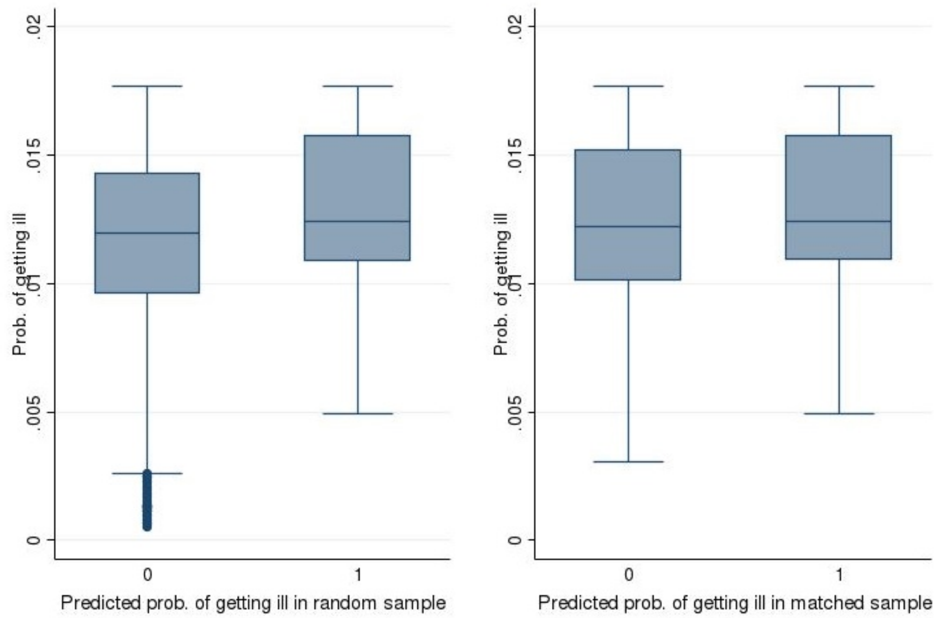

Figure C.3: Evolution of managers' labor market status following their illness episode

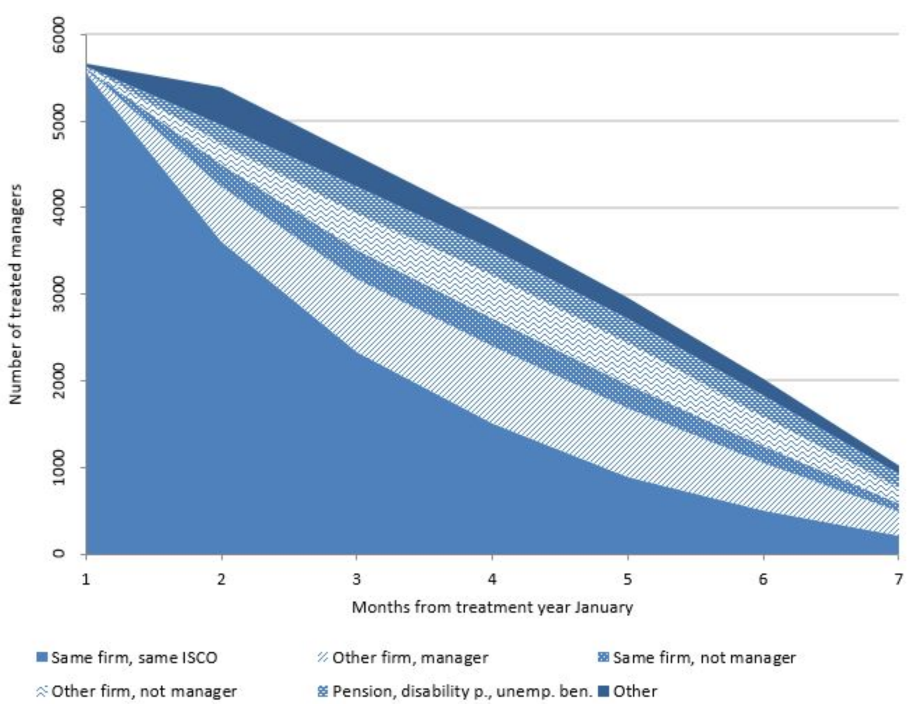

Table C.1: Managers' employment history after treatment

Employment outcome	12 months later			24 months later			48 months later		
	Treat	Control	Diff.	Treat	Control	Diff.	Treat	Control	Diff.
Same firm. same occupation	66.64 %	72.20 %	-5.56 pp.	50.69 %	56.05 %	-5.36 pp.	30.03 %	35.48 %	-5.45 pp.
Same firm. new manager occupation	4.23 %	4.34 %	-0.11 pp.	5.39 %	5.76 %	-0.37 pp.	6.34 %	6.69 %	-0.35 pp.
Same firm. not manager occupation	5.14 %	5.23 %	-0.09 pp.	7.59 %	8.00 %	-0.41 pp.	9.87 %	10.88 %	-1.01 pp.
New firm. manager	7.52 %	7.06 %	0.46 pp.	12.87 %	12.51 %	0.36 pp.	19.27 %	18.70 %	0.57 pp.
New firm. not manager	4.55 %	4.93 %	-0.38 pp.	9.78 %	8.96 %	0.82 pp.	16.26 %	15.23 %	1.03 pp.
Unemployment benefit	1.73 %	1.22 %	0.51 pp.	1.81 %	1.41 %	0.40 pp.	2.70 %	1.79 %	0.91 pp.
Pension. disability pension	2.36 %	1.24 %	1.12 pp.	4.84 %	2.32 %	2.52 pp.	7.53 %	4.13 %	3.40 pp.
Other	7.84 %	3.78 %	4.06 pp.	7.03 %	4.99 %	2.04 pp.	8.00 %	7.10 %	0.90 pp.

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Table C.2: Effect of health shock on managers' wage

	(1) Wage	(2) Wage
inter	-0.134***	
	(0.0227)	
Year 1		-0.146***
		(0.0237)
Year 2		-0.138***
		(0.0277)
Year 3		-0.0943***
		(0.0345)
Adjusted R^2	0.033	0.031
Number of observations	183237	182768
Controls	yes	yes
Individual FE	yes	yes
Sample restriction	Year -1 -2	Year -1 -2
Matched sample	yes	yes

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Table C.3: Effect of health shock on separation rates

	(1) Separation rate	(2) Separation rate
inter	0.00149**	
	(0.000679)	
Year 1		0.00125
		(0.000765)
Year 2		0.00148
		(0.00104)
Year 3		0.00129
		(0.00119)
Adjusted R^2	0.019	0.020
Number of observations	101123	101101
Controls	yes	yes
Individual FE	yes	yes
Sample restriction	Year -1 -2	Year -1 -2
Matched sample	yes	yes

Table C.4: Effect of health shock on the number of employees

	(1) Number of employees	(2) Number of employees
inter	-0.0630	
	(0.193)	
Year 1		0.249
		(0.244)
Year 2		-0.314
		(0.241)
Year 3		-0.180
		(0.248)
Adjusted R^2	0.000	0.000
Number of observations	104841	104788
Controls	yes	yes
Individual FE	yes	yes
Sample restriction	Year -1 -2	Year -1 -2
Matched sample	yes	yes

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Table C.5: Effect of health shock on voluntary separation rates

	(1) Separation rate vol	(2) Separation rate vol
inter	0.000331*	
	(0.000195)	
Year 1		0.000215
		(0.000238)
Year 2		0.000529
		(0.000330)
Year 3		0.0000715
		(0.000290)
Adjusted R^2	0.008	0.008
Number of observations	130105	130105
Controls	yes	yes
Individual FE	yes	yes
Sample restriction	Year -1 -2	Year -1 -2
Matched sample	yes	yes

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Table C.6: Effect of health shock on dismissal rates

	(1) Separation rate dm	(2) Separation rate dm
inter	0.00262***	
	(0.000610)	
Year 1		0.00238***
		(0.000662)
Year 2		0.00290***
		(0.000874)
Year 3		0.00237**
		(0.00105)
Adjusted R^2	0.017	0.017
Number of observations	122364	122364
Controls	yes	yes
Individual FE	yes	yes
Sample restriction	Year -1 -2	Year -1 -2
Matched sample	yes	yes

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Predefined ISCO pairs

Leader FEOR	Name	Employee FEOR	Name
1321	Department managers in agriculture and forestry	2125	Agricultural (horticultural) engineers
1351	Supervisors in agriculture and forestry	2126	Forestry and nature reserve engineers
1411	General managers of small undertakings in agriculture and forestry	3112	Surveying and GIS (Geographical Information System)
		3124	technicians
		3124	Agricultural (horticultural) technicians
		3125	Forestry and natural reserve technicians
		3126	Environmental protection technicians
		6111	Crop growers
		6112	Bio-gardeners
		6113	Vegetable growers
		6114	Fruit growers
		6115	Wine growers
		6116	Ornamental plant and flower gardeners
		6117	Tree nursery workers
		6118	Park and landscape workers
		6121	Medicinal herb growers
		6122	Reed- and poplar-farming workers
		6129	Crop growing and gardening workers n.e.c.
		6131	General livestock-farming workers
		6132	Cattle-farming workers
		6133	Pig-farming workers
		6134	Horse-farming workers
		6135	Sheep-farming workers
		6136	Poultry farming workers
		6137	Small animal breeders
		6139	Animal husbandry workers n.e.c.
		6211	Foresters, aid foresters
		6212	Forest tree nursery workers
		6213	Logging, lumbering (manual and machine operating) workers
		6219	Forestry workers and loggers n.e.c.
		6221	Hunters, game raisers
		6229	Game farming workers n.e.c.
		6311	Fishermen, fish farmers
		6319	Fishery workers n.e.c.
		6411	Plant protection, plant health protection workers
		6412	Soil conservation and amelioration workers
		8293	Agricultural machine operators, mechanics
		8311	Agricultural engine drivers and operators
		8312	Forestry plant operators
		8313	Plant protection machine operators
		8319	Agricultural and forestry mobile-plant drivers, operators n.e.c.
		8321	Earth moving equipment operators
		9210	Agricultural labourers (e.g. day-labourers, rangers)
		9220	Forestry, hunting, fishery labourers (e.g. fishing wardens)
1322	Department managers in manufacturing	2111	Mining engineers
1352	Supervisors in manufacturing	2113	Food and beverage industry engineers
1412	General managers of small undertakings in manufacturing	2114	Wood and light industry engineers
		2115	Chemical engineers
		2116	Metallurgical engineers
		2117	Mechanical engineers
		2118	Electrical engineers
		2121	Light-current (electronic) and telecommunications engineers
		2122	Transport engineers
		3113	Food and beverage industry technicians
		3114	Wood and light industry technicians
		3115	Chemical engineering technicians
		3117	Mechanical engineering technicians
		3118	Power-current (electrical) engineering technicians
		3121	Light-current (electronics) engineering technicians

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		3122	Transportation technicians
		5234	Telecommunications workers
		5316	Dry cleaners, dyers
		5341	Photographers, photo and film developers
		5344	Cinema projectionists
		5371	Drainage and sanitation workers
		5372	Public bath attendants
		5373	Water supply workers
		7111	Deep-drilling workers
		7112	Blasters
		7113	Miners, aid miners
		7114	Trammers
		7115	Quarry workers, stone cutters
1322	Department managers in manufacturing	2111	Mining engineers
1352	Supervisors in manufacturing	2113	Food and beverage industry engineers
1412	General managers of small undertakings in manufacturing	2114	Wood and light industry engineers
		7119	Solid minerals extraction workers n.e.c.
		7121	Crude oil extraction workers
		7129	Crude oil and natural gas extraction workers n.e.c., research drilling workers
		7211	Meat, fish and poultry processing workers
		7212	Food preservers, fruit and vegetable processing workers
		7213	Vegetable oil manufacturers
		7214	Milk processing workers
		7215	Milling industry workers
		7216	Bakers, pastry industry workers
		7217	Sugar industry workers
		7218	Sweets industry products manufacturers
		7219	Food processing workers n.e.c.
		7221	Alcohol, alcoholic drinks manufacturers
		7222	Wine and champagne producers
		7223	Brewery workers
		7224	Soft drinks, mineral and soda water manufacturers
		7230	Tobacco preparers and tobacco products manufacturers
		7311	Fibre preparers
		7312	Spinners
		7313	Weavers
		7314	Knitters
		7315	Dyers, textile printing, finishing workers
		7319	Textile industry workers n.e.c.
		7321	Tailors, dressmakers, needlewomen, model makers
		7322	Tailor's cutters (in manufacture of garments)
		7323	Hatters, milliners, cap makers (except knitters)
		7324	Pelt dressers, fur dyers
		7325	Furriers
		7329	Dress making and fur processing workers n.e.c.
		7331	Tanners, leather dressers
		7332	Saddlers, leather belt makers
		7333	Fancy leather goods and luggage makers
		7334	Leather glove makers
		7335	Shoemakers and repairers
		7336	Leather dressmakers and repairers
		7339	Leather industry workers n.e.c.
		7341	Cabinet-makers
		7343	Upholsterers
		7344	Wood pattern makers
		7345	Coopers, wheelwrights
		7346	Wood turners
		7349	Wood industry workers n.e.c.
		7351	Typesetters, typographical editors
		7352	Printers
		7353	Relief printing, photogravure, planography preparatory workers
		7354	Bookbinders
		7355	Recorded media (audio, video, computer data carrier) reproduction workers

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		7359	Printing workers n.e.c.
		7411	Metallurgical raw material preparers
		7412	Iron, steel and non-ferrous metal smelters
		7414	Sheet metal workers
		7415	Metal casting workers
		7419	Metallurgy workers n.e.c.
		7421	Locksmiths
		7422	Tool and die makers
		7423	Forging-press workers
		7424	Industrial precious metal workers
		7425	Welders, flame cutters
		7426	Blacksmiths
1322	Department managers in manufacturing	2111	Mining engineers
1352	Supervisors in manufacturing	2113	Food and beverage industry engineers
1412	General managers of small undertakings in manufacturing	2114	Wood and light industry engineers
		7429	Metal processing, shaping and forming and surface treatment workers n.e.c.
		7431	Motor-vehicle mechanics, engine repairmen
		7432	Aircraft mechanics, repairmen
		7433	Agricultural equipment mechanics, repairmen
		7439	Mechanics, repairmen n.e.c.
		7441	Mechanical instrument mechanics
		7442	Precision instrument mechanics
		7443	Electronic instrument mechanics
		7444	Radio-, TV- and computer-mechanics
		7445	Electrical equipment mechanics
		7449	Electrical equipment and precision instrument mechanics n.e.c.
		7490	Steel and metal trades workers n.e.c.
		7511	Animal hair and feather processing workers
		7512	Reed and poplar processing workers
		7513	Broom and brush makers
		7514	Toy, fancy-goods, and sporting-goods makers and repairers
		7515	Embroiders, lace-makers
		7519	Handicraft industry workers n.e.c.
		7521	Sign painters
		7522	Jewellers, engravers, precious stone grinders
		7523	Potters, ceramists
		7524	Glass-makers
		7525	Tire and rubber products repairers
		7526	Musical instrument makers, repairers
		7527	Concrete building block makers
		7529	Miscellaneous industry workers n.e.c.
		7612	Carpenters, scaffolders
		7636	Building stone cutters, stonemasons, artificial stone setters
		7637	Stove makers
		7638	Glaziers
		7641	Road construction and paving workers, road maintenance workers
		7642	Railroad construction and maintenance workers
		7644	Pipeline setters
		7645	Underwater construction workers
		7649	Civil engineering workers n.e.c
		8111	Food products machine operators
		8112	Beverage products machine operators
		8113	Tobacco products machine operators
		8121	Textile industry machine operators and production-line workers
		8122	Dressmaking machine operators and production-line workers
		8123	Leather tanning and processing machine operators and production-line workers
		8124	Shoemaking machine operators and production-line workers
		8125	Wood processing machine operators and production-line workers
		8126	Paper and pulp industry machine operators
		8127	Printing machine operators
		8129	Light industry machine operators and production-line workers n.e.c.
		8131	Petroleum refinery and processing machine operators
		8132	Gas-making and processing machine operators
		8133	Basic chemicals and chemical products machine operators
		8134	Pharmaceutical products machine operators
		8135	Fertilizer and plant-protection products machine operators
		8136	Plastic processing machine operators
		8137	Rubber goods manufacturers, vulcanizers
		8141	Ceramic products machine operators
		8142	Fine ceramics products machine operators
		8143	Glass and glass-products machine operators
		8144	Concrete building block machine operators
		8145	Lime and cement products machine operators

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		8149	Building materials industry machine operators n.e.c.
		8191	Metallurgical machine operators
		8192	Metal working machine operators
		8193	Production-line assemblers
		8199	Processing machine operators, production-line workers n.e.c.
		8211	Solid minerals extraction machine operators
		8219	Mining-plant operators n.e.c.
		8221	Power-production and transformation plant mechanics and operators
		8222	Coal- or oil-fired power-generating plant operators
		8223	Nuclear-fuelled power-generating plant operators
		8224	Hydroelectric power-generating station mechanics and machine operators
		8229	Power production and related plant operators n.e.c.
		8231	Water works machine operators
		8232	Sewage plant operators
		8233	Water pump operators
		8239	Water treatment plant operators n.e.c.
		8240	Packaging-machine operators
		8291	Boiler operators (licensed boilermen)
		8292	Decontaminating machine and equipment operators
		8299	Other non-manufacturing machine operators n.e.c.
		8341	Crane operators
		8342	Elevator and conveying machine operators
		8343	Lift-trolley operators
		8344	Loading/unloading machine operators
		8349	Material conveying machine operators n.e.c.
1323	Department managers in construction	2112	Surveyors and GIS (Geographical Information System) engineer
1353	Supervisors in construction	2123	Architects
1413	General managers of small undertakings in construction	2124	Civil engineers
		3112	Surveying and GIS (Geographical Information System) technicians
		3123	Construction technicians
		3194	Draughtspersons
		7112	Blasters
		7342	Building joiners
		7611	Bricklayers, masons
		7612	Carpenters, scaffolders
		7613	Reinforced concrete frame assemblers
		7614	Building block assemblers
		7619	Building frame workers n.e.c.
		7621	Plumbers and pipe-network fitters
		7622	Air-conditioning and ventilation mechanics
		7623	Elevator repairers
		7624	Building electricians
		7629	Building-assembling workers n.e.c.
		7631	Insulation workers
		7632	Roofers
		7633	Building tinsmiths
		7634	Tilers, coverers
		7635	Painters
		7636	Building stone cutters, stonemasons, artificial stone setters
		7637	Stove makers
		7638	Glaziers
		7639	Building finishers and related trades workers n.e.c.
		7641	Road construction and paving workers, road maintenance workers
		7642	Railroad construction and maintenance workers
		7643	Bridge structure construction workers
		7644	Pipeline setters
		7645	Underwater construction workers
		7649	Civil engineering workers n.e.c
		8341	Crane operators
		8342	Elevator and conveying machine operators
		8343	Lift-trolley operators
		8344	Loading/unloading machine operators
		8349	Material conveying machine operators n.e.c.
		9190	Labourers and helpers n.e.c. (e.g. odd-job persons)

1326	Department managers in transport, forwarding and storage	3161	Captains, shipmasters (sea and river)
		3162	Ships' deck officers, steersmen
		3163	Pilots, flight engineers
		8356	Heavy-truck and lorry drivers
1345	Supply and distribution managers	3161	Captains, shipmasters (sea and river)
		3162	Ships' deck officers, steersmen
		3163	Pilots, flight engineers
		8356	Heavy-truck and lorry drivers
1355	Supervisors in transport, forwarding and storage	3161	Captains, shipmasters (sea and river)
		3162	Ships' deck officers, steersmen
		3163	Pilots, flight engineers
		8356	Heavy-truck and lorry drivers
1416	General managers of small undertakings in transport, forwarding and storage	3161	Captains, shipmasters (sea and river)
		3162	Ships' deck officers, steersmen
		3163	Pilots, flight engineers
		8356	Heavy-truck and lorry drivers
1331	Department managers in business services	2131	Computer science professionals (e.g. systems-analysts, operations-research analysts)
1421	General managers of small undertakings in business services	2132	Electronic data processing organizers
1331	Department managers in business services	2139	Other computing professionals with third-level qualification, n.e.c.
		2331	Employment counsellors
		2332	Career counsellors
		2512	Tax advisors, consultants
		2513	Financial and credit organizers
		2514	Auditors
		2515	Plant economists, management organizers
		2517	Trade organizers
		2518	Auditors
		2521	Market researcher, advertising and PR occupations
		2522	Commercial sales representatives
		2523	Personnel organizers
		2529	Business professionals n.e.c.
		2624	Industrial designers
		3121	Light-current (electronics) engineering technicians
		3129	Technicians n.e.c.
		3131	Computer-network operators
		3133	Database managers
		3139	Computer associate professionals n.e.c.
		3603	Personnel clerks
		3606	Accounting clerks
		3608	Planning clerks
		3622	Exhibition and advertising clerks
		3624	Stock and management clerks
		3631	Bank financing clerks
		3632	Payment and deposit clerks
		3633	Cash and securities clerks
		3635	Brokers and dealers
		3639	Financial intermediation clerks n.e.c.
		4111	Analytic bookkeeping clerks
		4112	Payroll clerks
		4119	Analytic accounting clerks n.e.c.
		4121	Stock clerks
		4122	Financial, personnel clerks
		4219	Tellers, cashiers n.e.c.
		5234	Telecommunications workers
1347	Computing services managers	2121	Light-current (electronic) and telecommunications engineers
		2131	Computer science professionals (e.g. systems-analysts, operations-research analysts)
		2132	Electronic data processing organizers
		2139	Other computing professionals with third-level qualification, n.e.c.
		2624	Industrial designers
		3121	Light-current (electronics) engineering technicians
		3129	Technicians n.e.c.

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		3131	Computer-network operators
		3133	Database managers
		3139	Computer associate professionals n.e.c.
		5234	Telecommunications workers
		7443	Electronic instrument mechanics
		7444	Radio-, TV- and computer-mechanics
1333	Department managers in health care and welfare services	2211	General practitioners
1423	General managers of small undertakings in health care and welfare services	2212	Specialized medical doctors
		2213	Dentists
		2214	Specialized dentists
		2215	Pharmacists
		2216	Specialized pharmacists
		2221	Public hygiene supervisors
		2222	Optometrists
		2223	Dieticians
		2224	Physiotherapist
		2225	Institution based nurses
		2226	Ambulance attendance
		2229	Human health related professionals n.e.c.
		2230	Professional nurses
		2432	Kindergarten teachers
		2441	Teachers for the handicapped
		2442	Teachers for the physically disabled
		3211	General nurses
		3212	Specialized nurses
		3221	Personal care workers
		3222	Specialized personal care workers
		3231	General medical assistants
		3232	Specialized medical assistants
		3233	Dental assistants
		3239	Medical assistants n.e.c.
		3242	Midwives
		3244	Dietitians
		3311	Welfare assistants
		3312	Mental hygiene assistants
		3313	Welfare care workers
		3319	Welfare associate professionals n.e.c.
		3412	Child- and youth-care associate professionals
		3415	Assistant for the education of the challenged/handicapped
		5320	Health and educational services workers (e.g. assistant nurses, ambulance men, hospital
		5330	Welfare services workers (e.g. communal or home based personal care workers)
1334	Department managers in education	2410	Third-level education teaching professionals (e.g. university or college professors, associate professors, assistant professors)
		2421	Secondary school teachers, instructors
		2422	Secondary level vocational training instructors
		2429	Secondary education teaching professionals n.e.c.
		2431	Primary school teachers
		2432	Kindergarten teachers
		2439	Primary education teaching professionals n.e.c.
		2441	Teachers for the handicapped
		2442	Teachers for the physically disabled
		2443	Health educators
		2449	Special education teaching professionals n.e.c. (e.g. psycho-pedagogical teachers)
		2491	Education specialists, school inspectors
		2499	Teaching professionals n.e.c. (e.g. welfare instructors, vocational training instructors in a company, irrespective of the educational level)
		3411	Teachers without third-level qualification
		3412	Child- and youth-care associate professionals
		3413	Pedagogical assistants
		3414	Health education assistants
		3415	Assistant for the education of the challenged/handicapped
		3419	Teaching associate professionals n.e.c.
		5320	Health and educational services workers (e.g. assistant nurses, ambulance men, hospital orderlies, nannies)

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1424	General managers of small undertakings in educational services	2410	Third-level education teaching professionals (e.g. university or college professors, associate professors, assistant professors)
		2421	Secondary school teachers, instructors
		2422	Secondary level vocational training instructors
		2429	Secondary education teaching professionals n.e.c.
		2431	Primary school teachers
		2432	Kindergarten teachers
		2439	Primary education teaching professionals n.e.c.
		2441	Teachers for the handicapped
		2442	Teachers for the physically disabled
		2443	Health educators
		2449	Special education teaching professionals n.e.c. (e.g. psycho-pedagogical teachers)
		2491	Education specialists, school inspectors
		2499	Teaching professionals n.e.c. (e.g. welfare instructors, vocational training instructors in a company, irrespective of the educational level)
		3411	Teachers without third-level qualification
		3412	Child- and youth-care associate professionals
		3413	Pedagogical assistants
		3414	Health education assistants
		3415	Assistant for the education of the challanged/handicapped
		3419	Teaching associate professionals n.e.c.
		5320	Health and educational services workers (e.g. assistant nurses, ambulance men, hospital orderlies, nannies)
1324	Department managers in wholesale and retail trade	2517	Trade organizers
		2522	Commercial sales representatives
		3621	Trade clerks
		5111	Shopkeepers
		5112	Shop assistants
		5114	Occupations in making up consignment of goods
		5119	Wholesale and retail trade workers n.e.c.
		9131	Manual materials handlers, hand packers
1325	Department managers of restaurants and hotels	4291	Client information clerks
		3643	Hotel porters, receptionists
		5121	Restaurant managers, restaurateurs
		5122	Confectioners
		5123	Waiters, restaurant salespersons
		5124	Cooks
		5129	Hotels and restaurants workers n.e.c.
		9114	Kitchen helpers
1354	Supervisors in wholesale and retail trade, restaurants and hotels	2517	Trade organizers
		2522	Commercial sales representatives
		3621	Trade clerks
		5111	Shopkeepers
		5112	Shop assistants
		5114	Occupations in making up consignment of goods
		5119	Wholesale and retail trade workers n.e.c.
		9131	Manual materials handlers, hand packers
		4291	Client information clerks
		3643	Hotel porters, receptionists
		5121	Restaurant managers, restaurateurs
		5122	Confectioners
		5123	Waiters, restaurant salespersons
		5124	Cooks
		5129	Hotels and restaurants workers n.e.c.
		9114	Kitchen helpers
1414	General managers of small undertakings in wholesale and retail trade	2517	Trade organizers
		2522	Commercial sales representatives
		3621	Trade clerks
		5111	Shopkeepers
		5112	Shop assistants
		5114	Occupations in making up consignment of goods
		5119	Wholesale and retail trade workers n.e.c.
		9131	Manual materials handlers, hand packers
1415	General managers of small undertakings in restaurants and hotels	4291	Client information clerks
		3643	Hotel porters, receptionists
		5121	Restaurant managers, restaurateurs
		5122	Confectioners
		5123	Waiters, restaurant salespersons
		5124	Cooks
		5129	Hotels and restaurants workers n.e.c.
		9114	Kitchen helpers

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1335	Department managers in cultural services	2611	Librarians
1425	General managers of small undertakings in cultural services	2612	Archivists
		2613	Curators (restorers, taxidermists)
		2614	Cultural organizers
		2621	Writers (except journalists)
		2623	Sculptors, painters and related artists
		2624	Industrial designers
		2625	Composers
		2626	Film, stage and related directors
		2627	Cameramen, artistic photographers
		2629	Creative artists n.e.c.
		2631	Actors, stage performing artists, puppet artists
		2632	Musicians, singers
		2633	Choreographers, dancers
		2639	Performing artists n.e.c.
		3711	Library assistants
		3712	Archivist assistants
		3713	Cultural organizer assistants
		3719	Cultural associate professionals n.e.c.
		3721	Supporting actors
		3722	Film, stage and related assistant directors
		3723	Folk musicians
		3724	Restaurant and night-club musicians
		3725	Circus artists
		3729	Artistic associate professionals n.e.c.
		5342	Light technicians and other motion picture workers
		5343	Scenery shifters
		5344	Cinema projectionists
1339	Department managers in production and services n.e.c.	2618	Qualified coaches, sport organizers/coordinators
1429	General managers of small undertakings n.e.c.	2619	Cultural proffessionals n.e.c.
		3232	Specialized medical assistants
		5314	Masseurs
		5349	Cultural, sports and entertainment services workers n.e.c.
		5361	Policemen
		5362	Fire-fighters
		5363	Penal enforcement warden
		5366	Security guards
		5365	Bodyguards
		5364	Natural reserve wardens
		5355	Public place inspectors
		5369	Protective services workers n.e.c.
1342	Accountancy and finance managers	2512	Tax advisors, consultants
		2513	Financial and credit organizers
		2514	Auditors
		2518	Auditors
		3606	Accounting clerks
		3624	Stock and management clerks
		4111	Analytic bookkeeping clerks
		4119	Analytic accounting clerks n.e.c.
		4121	Stock clerks
		4122	Financial, personnel clerks
		4219	Tellers, cashiers n.e.c.
1343	Human resources (personnel) managers	2331	Employment counsellors
		2332	Career counsellors
		2523	Personnel organizers
		3603	Personnel clerks
		4112	Payroll clerks
		4122	Financial, personnel clerks

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1349	Functional unit managers n.e.c.	2514	Auditors
		2515	Plant economists, management organizers
		2518	Auditors
		2529	Business professionals n.e.c.
		3608	Planning clerks
		2513	Financial and credit organizers
		2512	Tax advisors, consultants
1341	Marketing managers	2517	Trade organizers
		2521	Market researcher, advertising and PR occupations
		2522	Commercial sales representatives
1344	Advertising and other public relations managers	2521	Market researcher, advertising and PR occupations
		3622	Exhibition and advertising clerks

In our dataset, occupations are coded using the Hungarian Standard Classification of Occupations, which is almost identical to the International Standard Classification of Occupations. [a]

[a]For a full comparison of the two classification schemes, see https://www.ksh.hu/docs/osztalyozasok/feor/fordkulcs_feor_isco_hu.pdf