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Working Paper 2017:13

Department of Economics  
School of Economics and Management

# The Effect of Paid Vacation on Health: Evidence from Sweden

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# The Effect of Paid Vacation on Health: Evidence from Sweden

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## Abstract

This study estimates the causal effect of paid vacation on health. Using register data on the universe of central government employees in Sweden, I exploit an age-based rule stipulated in the collective agreement covering these employees. I achieve identification by combining a regression discontinuity with a difference-in-differences design to control for time-invariant differences between consecutive birth cohorts and isolate the true effect at two separate discontinuities at ages 30 and 40. The main results indicate that an increase of three paid vacation days at age 30 and four days at age 40 do not cause significant changes in health, as proxied by visits to specialized outpatient care, inpatient admissions, and long-term sick leaves. These findings challenge the anecdotal view of additional paid vacation days as an adequate means to improve workers' health.

**JEL codes:** I18, J22, J81, M52

**Keywords:** health, vacation, holiday, annual leave, working time

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# 1 Introduction

Paid vacation, denoting the vacation leave to which a worker is entitled in a year, and paid annual leave<sup>1</sup> have become the target of policy makers during the Great Recession. Indeed, several countries have made cuts in a bid to stimulate the economy and increase its competitiveness.<sup>2</sup> Workers' health represented at best a secondary concern in all of these decisions. This is surprising from a historical viewpoint, as it contradicts the main argument based on which paid vacation was introduced and extended around the world. The earliest labor market regulations mandating paid vacation date back to the 1930s and were mainly intended to maintain and protect workers' health. In Sweden, the country considered in this study, the first statutory regulation on vacation entitlement was implemented in 1931 as part of the Occupational Safety Act, and in 1938 turned into a fully-fledged law that emphasized the importance of vacation for workers' physical and mental health (SOU, 2001). Over time, mandated vacation entitlements have become increasingly more generous.<sup>3</sup> In 1993, the Council of the European Union adopted the so-called Working Time Directive (93/104/EC) which required all member states to introduce, inter alia, at least four weeks of paid vacation per year. The purpose of this directive was to protect workers' health and safety. Despite this pervasive emphasis on workers' health, credible empirical evidence of the impact of paid vacation on health is virtually non-existent.

In this study, I examine the effects of paid vacation on health in the context of Sweden. In general, the relationship between health and vacation entitlement is difficult to disentangle. Workers can self-select into sectors or jobs that grant them different numbers of predefined (or individually-negotiated) vacation days. The problem is that the self-selection of workers might be related to their health. Even in instances where rich information on workers' health and vacation entitlement is available, there might be unobserved factors that confound the relationship. This study overcomes these difficulties by using an age-based rule for receiving additional paid vacation days that applies to employees in the central government sector in Sweden. This rule creates a quasi-experimental setting that can be exploited to estimate the causal effect on health.

More specifically, the age-based rule permanently increases the annual vacation entitlement from 28 to 31 days in the calendar year an employee turns 30 and from 31 to 35 days

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<sup>1</sup>Paid annual leave is the sum of paid vacation days and paid public holidays. Whereas paid public holidays (e.g., May Day) typically take place on the same date every year, a worker can more or less freely distribute paid vacation days across the year. Whether workers stay at home or travel while on vacation is not considered in this study.

<sup>2</sup>To give a few examples, the Finnish government intended in 2015 to cut vacation days for civil servants from 38 to 30 days but instead reduced their vacation pay by 30 percent for the years 2017 to 2019. In 2012, the Irish government capped paid annual leave for civil servants at 32 days; previously it could amount to 40 days in some cases. In Saudi Arabia, vacation was capped at 30 days for civil servants in 2016. In Switzerland, a proposal for an extension of the minimum vacation entitlement from four to six weeks was voted down in a referendum in 2012, due to concerns about negative consequences for the economy's competitiveness.

<sup>3</sup>The content of the health-related argument for paid vacation has changed over time from the so-called protection motive to the recreation motive in Sweden. The foremost reason for the introduction in the 1930s was to protect workers from "wear and tear" and work-related accidents. Nowadays, vacation should give workers time for rest, recuperation, and relaxation to maintain health as well as an opportunity to pursue own interests during leisure time (Arbetsgivarverket, 2009; Ericson and Eriksson, 2015).

in the calendar year an employee turns 40. The whole vacation entitlement is received in the beginning of each calendar year. This creates two discontinuities at ages 30 and 40 around which one group of employees for the first time receives additional vacation days (“treatment group”) and another group has to wait for a whole year to also receive those days (“control group”). The outcome of interest is the effect on health during this year of differential vacation entitlements. To overcome problems arising from the comparison of employees in the older treatment group and the younger control group who come from two consecutive birth cohorts, I combine a regression discontinuity (RD) design with a difference-in-differences (DID) approach. The idea is to estimate the difference in the outcome of interest at a counterfactual threshold at age 28 (38) and then to subtract this difference from the one estimated at the actual threshold at age 30 (40). This cancels out health-related factors that are time-invariant between two consecutive birth cohorts and yields the true causal effect.

A theoretical framework to help think about the mechanism behind changes in the number of paid vacation days and its effect on health is Grossman’s (1972) concept of the health production function. In Sweden, workers’ earnings remain unchanged as the number of *paid* vacation days increases, as there is only a negligible extra vacation pay per vacation day on top of the ordinary salary. In the absence of additional income to be spent on health-enhancing products and services, no health gain should be expected from this income channel. Additional vacation days endow workers with more leisure time though, by reducing the stipulated annual working hours. Leisure time is usually assumed to be health enhancing. There is also some causal empirical evidence that increases in annual working hours impair health; see, e.g., Hummels et al. (2016).<sup>4</sup> Yet it is not a priori clear if the same should be assumed for vacation days. The direction of the effect on health depends arguably on the activities pursued during vacation. These activities might be a function of workers’ age, parenthood, marital status, or the match between spouses’ vacation entitlements. It is thus an empirical question to determine the direction and size of the effect of additional paid vacation days on workers’ health.

For the empirical analysis, I use register data on the universe of central government employees in Sweden and link all employment records for the period 1997 to 2011 to health registers. I construct three measures of health; indicators for visits to specialized outpatient care, admissions to inpatient care, and long-term sick leaves (lasting longer than 14 days). As no information on actual use of vacation days is available, I perform an intention-to-treat (ITT) analysis. From a policy perspective, ITT effects are the relevant measures to assess the effectiveness of changes in paid vacation entitlement in practice.

The results indicate that the effects of receiving three (four) additional vacation days at

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<sup>4</sup>A small related literature focuses on the causal effects of changes in the regular weekly working hours on workers’ health while holding earnings constant. Berniell and Bietenbeck (2017) find that longer working hours have negative effects on health, and Lepinteur (2019) finds positive effects of shorter working hours on job and leisure satisfaction. However, reductions in the regular weekly working hours provide workers with additional leisure time every week of the year, whereas additional vacation days provide additional leisure time during certain days of the year. The way workers are “treated” with additional leisure time is thus different.

age 30 (40) on all measures of employees' health are small and statistically indistinguishable from zero. I also consider a single indicator for experiencing any of the three health events, a summary index of health, and stacking the data at ages 30 and 40 in order to increase statistical power. This allows me to rule out effect sizes larger than 0.5% of a standard deviation for health deteriorations and larger than 5% of a standard deviation for health improvements.<sup>5</sup> I also show that income remains unchanged at both thresholds. Thus, the effect on health should solely be based on changes in leisure time.

I investigate two dimensions of heterogeneity in the effects. There are no significant effects in the subsamples of men, women, (un)married employees, (lone) parents, childless employees, employees with or without a university degree, and employees with more recent health problems. There is neither any evidence of significant effects by broad group of diagnoses. A set of robustness tests supports this finding. The results call the health-related argument that surrounded the introduction and the extension of paid vacation into question when put to the test in a modern context. It also means that recent policy changes that cut vacation entitlements might have entailed limited consequences for younger workers' health, if the effects are assumed to be symmetric.

Previous empirical research on the relationship between vacation and health can be classified into two different branches. The first branch are studies in psychology and occupational medicine conducted mostly in Western European countries and Israel. The main results from this literature are as follows (see de Bloom et al. (2009) for a meta-analysis, and, e.g., Kühnel and Sonnentag (2011); de Bloom et al. (2012, 2013) for subsequent studies). First, being on a vacation (typically lasting from a few days to three weeks) is associated with positive but small gains in health and well-being (measured mostly in terms of subjective life satisfaction, mood, exhaustion, and health complaints). Second, these positive effects fade out quickly after returning to work; typically after one to four weeks.<sup>6</sup> Note that there is no evidence in this literature for the commonly held belief that for a vacation to be "healthy" it needs to last for at least two to three uninterrupted weeks, and that the health benefit of such a vacation is greater than that of a shorter vacation. The usual study setting is a small and unrepresentative sample of 30 to 500 participants, who answer questionnaires about health and well-being shortly before a vacation, during the vacation, and right after the vacation. A simple before-after analysis is used to measure effects. It is therefore difficult to attribute these results a causal interpretation.

The second branch consists of a handful of studies in the medical literature (Eaker et al., 1992; Gump and Matthews, 2000; Chikani et al., 2005; Strandberg et al., 2017; Kim, 2019). The four US-based studies focus on the extensive margin of whether taking any vacation in a certain year is related to health, whereas a Finnish study compares individuals who took more than 21 days to those who took less. These studies are longitudinal cohort studies that relate vacation use in a certain year to health outcomes measured after several years.

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<sup>5</sup>By way of comparison, Lepinteur (2019) finds statistically positive effects ranging from 7% to 15% of a standard deviation of shorter weekly working hours on job and leisure satisfaction.

<sup>6</sup>This pattern has been dubbed "the vacation cycle", which consists of a vacation effect (i.e., the boost in well-being and health during a vacation) and a fade-out effect (i.e., the positive outcomes vanish swiftly as vacationers return to their routine environment) (Kirillova and Lehto, 2015).

They find positive associations between taking any/more vacation and health (measured in terms of mortality, myocardial infarction, and mental health). However, these studies cannot determine cause and effect.

In economics, to the best of my knowledge, there is only one study by Schnitzlein (2012) that touches upon the topic of paid vacation and health. The author shows an association between not claiming the full vacation entitlement in the previous year and lower subjective health satisfaction as well as more days of sick leave in the following year among a sample of German full-time workers. The contribution of my study is to provide the very first *causal* estimate of the effect of an increase in the number of paid vacation days on health. Notably, the analysis is based on high-quality register data on *all* central government employees in Sweden and uses *objective* measures of health. The estimated effect is a local average treatment effect (LATE), but it is identified separately at two different ages, which contributes to the generalizability of the results. I corroborate the external validity of the results by showing that the study population appears to be similar in terms of health to private sector employees. The studied increases in vacation days are also of a magnitude that is commonly brought up in policy discussions.

## 2 Institutional context: Vacation entitlement in Sweden

In Sweden, the statutory standard weekly working time has been 40 hours since 1973. The Annual Leave Act (*semesterlagen*) has mandated five weeks of paid vacation per year (which translate into 25 workdays) since 1978. Despite these national laws regulating working time, collective agreements that cover almost the entire workforce govern the Swedish labor market. These agreements cannot undercut the statutory regulations, but only provide more favorable conditions (Arbetsgivarverket, 2009). For instance, the majority of Swedish workers receives more than the 25 days of paid vacation mandated by the Annual Leave Act (SOU, 2001). Notably, the vacation entitlement is more generous in the public sector.

The focus in this study is on the central government sector, which is the highest tier in the Swedish public sector and is comprised of all government agencies. The by far largest collective agreement covering almost all central government employees is the General Agreement on Salaries and Benefits (ALFA; *allmänt löne- och förmånsavtal*), which came into force on January 1, 1997.<sup>7</sup> Regarding vacation entitlement, the ALFA stipulates the following age-based rule.<sup>8</sup> Until the year an employee turns 29, she receives

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<sup>7</sup>The original agreement was called ALFA Cirkulär 1997:A 4. The subsequent renewals were ALFA Cirkulär 1998:A 8, ALFA Cirkulär 2001:A 8, ALFA Cirkulär 2002:A 5, ALFA Centrala avtal 2005:4, ALFA Centrala avtal 2008:1, and ALFA ALFA-T Centrala avtal 2011:4. The rules on vacation entitlement have not been altered in any of the renewals.

<sup>8</sup>Since October 1, 2007, about 40% of all employees covered by the ALFA and represented by the trade union Saco-S have the possibility to negotiate different terms on the number of paid vacation days with their employer on an individual basis. An analysis by Saco-S has shown that only a tiny fraction of employees (0.1% of all central government employees in 2010 and 0.3% of all in 2011) actually made use of this possibility (Saco-S and Arbetsgivarverket, 2014). As a robustness check, I run the analysis only including the years before 2007.

28 days of paid vacation per year. The year she turns 30, she receives a permanent increase to 31 paid vacation days. The year she turns 40, she receives another permanent increase to 35 paid vacation days that lasts until retirement.<sup>9</sup> It is crucial to note that the full vacation entitlement for the entire calendar year is received in the beginning of the calendar year, without any requirement on having worked in the previous calendar year. Therefore, this rule creates two discontinuities; one at age 30 and one at age 40. For instance, at the first discontinuity, people born on December 31 in calendar year  $t-30$  receive three additional vacation days in year  $t$ , whereas people born on January 1 in calendar year  $t-29$  just miss out on those days in year  $t$ . In addition, the standard weekly working time stipulated by the ALFA has been 39 hours and 45 minutes since 1997. Thus, the only changes to employees' regular working time in the period after 1997 is induced by the age-based changes in the vacation entitlement.

Several additional provisions in the ALFA and the Annual Leave Act are noteworthy. First, an employee must be employed year-round to receive and make use of the full vacation entitlement.<sup>10</sup> Second, vacation days cannot be paid out in money.<sup>11</sup> Third, the employer is obliged to ensure that vacation days are spent as leave from work, and that at least 20 vacation days are taken every year. Fourth, vacation days (in excess of the 20 days that have to be taken every year) can be saved. There is an upper limit of 40 (35 since 2011) saved vacation days (exceeding ones are lost and not paid out in money), but no time limit on how many years those can be saved. Fifth, sick days during a vacation leave do not constitute vacation days. Sixth, part-time employment entitles to the same number of vacation days.<sup>12</sup> Lastly, each vacation day comes along with a small supplement of 0.44% of the monthly salary in addition to the ordinary salary, irrespective of having worked in the previous calendar year.<sup>13</sup>

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<sup>9</sup>Note that vacation days correspond to workdays in the standard five-day workweek stipulated by the ALFA. A 40-year-old employee thus receives exactly seven weeks of paid vacation per year.

<sup>10</sup>If an employee uses all vacation days for a year but quits her employment before the end of the year, she has to pay back vacation days in proportion to the length of employment.

<sup>11</sup>There are three exceptions. Vacation days can be paid out in money if an employment lasts for fewer than three months. They must be paid out in money if an employee quits her employment and still has vacation days left. If an employee due to sickness or other reasons could not take all vacation days during a year, then these days are saved for later, but if the total number of saved days exceeds 40 days (35 days since 2011), the exceeding days are paid out in money.

<sup>12</sup>For instance, a 45-year-old part-time central government employee who works four hours Monday to Friday (i.e., 50% part-time) gets 35 vacation days per year. If the same employee would work eight hours on Mondays and Tuesdays, four hours on Wednesdays, and not work on Thursdays and Fridays, then the vacation entitlement is 17.5 days (= 35 days \* 50% employment) per year instead.

<sup>13</sup>For instance, with a gross monthly salary of SEK 25,000 (USD 2650), the vacation pay is comprised of the ordinary salary per workday of about SEK 1136 (USD 120) (assuming 22 workdays per month) and the supplement of SEK 110 (USD 12) (= 0.44% \* SEK 25,000). Three (four) additional vacation days at age 30 (40) would thus increase annual earnings by a mere 0.11% (0.15%). By contrast, annual working time decreases by 1.35% (1.83%) at age 30 (40), assuming 250 workdays per year and that the full vacation entitlement is used.



## 3 Data and descriptive statistics

### 3.1 Register data

The data used in this study come from the Swedish Interdisciplinary Panel (SIP), administered by the Centre for Economic Demography, Lund University, Sweden. The SIP database comprises the entire Swedish population born between 1932 and 1980 as well as information on their children. It encompasses several national registers that cover mostly the period from 1968 until 2011, with richer data availability towards the end of the period. The registers are linked through personal identifiers. As the ALFA came into force on January 1, 1997 and the available data stretch until 2011, I base the analysis on pooled data from 1997 to 2011.

Central government employees<sup>14</sup> covered by the ALFA are identified via the Register-based Labor Market Statistics.<sup>15</sup> Swedish register data do not contain information on how workers make use of their vacation entitlement. It is therefore impossible to know when and how many vacation days a worker actually takes and saves, and in which way they are taken (full weeks or more spread out). To ensure that the rule on vacation entitlement stipulated by the ALFA applies fully, I impose the following sample restrictions. First, an employee must have been employed year-round in the same government agency to be sure that she received and was able to use the full vacation entitlement.<sup>16</sup> This requirement drops employees who (i) started or ended an employment during the calendar year, and who (ii) switched within the central government sector from one agency to another, as it was not always possible to transfer saved vacation days. Second, an employee can only have had one year-round employment to be sure that she was not subject to rules on vacation entitlement from another collective agreement. This requirement drops employees who (i) had additional year-round employments in other tiers of the public sector or the private sector, and who (ii) had several year-round employments in the central government sector. As a result, the final sample contains employees who were employed year-round solely in the central government sector and subject to the ALFA. However, these employees might still have had one or more temporary secondary employments, which could be used for working during vacation. In a robustness check, I exclude such employees.

I obtain information on employees' month and year of birth from the Population Register together with relevant background information on sex, civil status, children, and immigrant status. Data on the highest educational degree attained come from the Education Register, on income from the Income and Taxation register, and on employees'

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<sup>14</sup>Approximately 230,000-250,000 people or 5-6 percent of the Swedish working population were employed in the central government sector in the period 1997-2011 (Statskontoret, 2015). Note that employees in (wholly or partly) state-owned enterprises and foundations are not central government employees.

<sup>15</sup>It is possible to distinguish them from a small group of employees who are working in special government agencies called *statliga affärsverk*. They are covered by another collective agreement that mandates similar though not identical rules on vacation entitlement.

<sup>16</sup>This requirement does not exclude employees with poor health, as employees cannot be dismissed due to, e.g., long and repeated sick leaves. Staff turnover has been fairly stable at least since 2006. About 10-13% of those employed in a certain year quit their job in the following year (including due to retirement) (Statskontoret, 2015).

occupation from the Occupation Register.

### 3.2 Health measures

To measure employees' health, I draw on two different registers. The first one is the National Patient Register, maintained by the National Board of Health and Welfare, which includes data on admissions to inpatient care since 1964 (complete coverage since 1987), and on visits to specialized outpatient care since 2001 (with increasing completeness in more recent years). An admission to inpatient care (i.e., a hospitalization) entails usually one or more overnight stays at a hospital. Yet it is also possible that a patient is admitted and discharged during the same day (e.g., due to acute abdominal pain that turns out to be innocuous).<sup>17</sup> Visits to specialized outpatient care encompass all visits to ambulatory care at hospitals and specialized clinics. Visits to a general practitioner at a local health care center are not covered. For the period 1997 to 2011, the register contains the underlying main diagnosis (coded according to the tenth edition of the International Statistical Classification of Diseases and Related Health Problems (ICD-10)<sup>18</sup>) for each admission and visit.

The second register is the MiDAS (Micro Data for Analyses of the Social Insurance) database, maintained by the Social Insurance Agency, which records all sick leaves that are paid for by the Social Insurance Agency since 1994. There are two types of sick leaves; due to sickness and due to disability. The focus in this study is solely on the former. Since 1992, sickness benefits are only paid out by the Social Insurance Agency starting after a sickness period of 14 calendar days<sup>19</sup> and hence recorded in the database.<sup>20</sup> Thus, only long-term sick leaves due to sickness can be considered. For the period 1997 to 2011, the MiDAS database contains the underlying diagnosis (coded according to ICD-10) for a sick leave due to sickness for 44.4% of all cases; in 1997 only 0.1% have a valid diagnosis whereas 92% have one in 2011.

I construct three basic measures of health from the registers. The first one is a dummy variable indicating whether an employee had made at least one visit to specialized outpatient care during a calendar year. The second measure is a dummy variable indicating whether an employee had been admitted to inpatient care at least once during a calendar year. The third measure is a dummy variable indicating whether an employee had been on long-term sick leave due to sickness lasting longer than 14 days at least once during a calendar year and hence received sickness benefits from the Social Insurance Agency. Figure

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<sup>17</sup>It is not possible to distinguish between acute and planned admissions in the data.

<sup>18</sup>12.3% of all cases in the register in 1997 are classified based on ICD-9, which I recode accordingly. In all subsequent years, more than 99.9% of all cases have a valid ICD-10 code.

<sup>19</sup>Between January 1, 1997 and March 31, 1998 the Social Insurance Agency paid out sickness benefits after a sickness period of 28 calendar days, and between July 1, 2003 and December 31, 2004 after 21 calendar days.

<sup>20</sup>The first day of a sickness period is not remunerated. From day 2 to 14, the employer is obliged to pay sick pay equal to 80% of the ordinary salary. The sickness benefits paid out by the Social Insurance Agency as of day 15 amount to slightly less than 80% of the ordinary salary, but there is a cap on the maximum benefits per day. In 2008, sickness benefits paid out by the Social Insurance Agency became time-limited to one year in normal cases and a maximum of 914 days in exceptional cases. I address the potential impact of this policy change in a robustness check.

A1 illustrates the age-specific means of the three measures for the considered sample of central government employees. Based on the main diagnosis of the health care contact or the long-term sick leave, I also divide the three health dummies into twenty broad groups of diagnoses, according to all “chapters” of the ICD-10 classification.<sup>21</sup>

The use of objective measures of health that have direct implications for policy making constitutes a strength of this study. These measures are not plagued by self-report bias or measurement error. The drawback is that there is only information on health for individuals who have been in contact with certain parts of the health care system or who have been on long-term sick leave.<sup>22</sup> This means that the analysis does not fully extend to less severe conditions. However, in the Swedish health care system the regional county councils must provide all of their residents with equal access to health care at very low out-of-pocket patient fees.<sup>23</sup> The financial disincentives for selection into health care are thus small. Furthermore, the use of (long-term) sick leave as a measure for health might not seem ideal, as it can be influenced by factors such as social norms; see, e.g., Hesselius et al. (2009). However, a worker in Sweden needs to obtain a medical certificate by the seventh day of the sickness period at the latest to keep receiving sick pay from her employer. There is thus an economic incentive to get in touch with the health care system and obtain the certificate, which makes it less likely that a worker neglects doing so in case of illness.

### 3.3 Descriptive statistics

Table 1 describes the samples used around the discontinuities at ages 30 and 40. Since the analysis is based on pooled data from 1997 to 2011, people born between January 1967 and December 1984 are included in the four-year age interval considered at age 30, and those born between January 1957 and December 1974 at age 40. There are slightly more men than women employed at both ages. Almost 10 percent are born outside Sweden, but only 5 percent do not possess Swedish citizenship, which is a requirement for employment in certain sections of the central government, such as the police, the armed forces, and the judicial system.<sup>24</sup> Central government employees are highly educated, and the higher share of university graduates around age 30 than 40 is a product of a considerable number

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<sup>21</sup>The most common diagnoses (based on two-digit ICD-10 codes) at ages 30 and 40 for outpatient care are follow-up examination after surgery (Z09), abdominal and pelvic pain (R10), supervision of normal pregnancy (Z34), and special examination of persons without complaint or reported diagnosis (e.g., blood pressure, allergy tests, pap test, mammography) (Z01). For inpatient care, they are childbirth (O80+O81+O82), abdominal and pelvic pain (R10), gallstones (K80), and acute appendicitis (K35). For sick leave, they are disorders related to severe stress (F43), depression (F32), back pain (M54), and maternal care for other conditions predominantly related to pregnancy (O26).

<sup>22</sup>As opposed to inpatient admissions, some outpatient visits might be used for preventive purposes and thus reflect more of an input to health. Excluding outpatient visits which might be partly preventive (diagnosis group with ICD-10 code Z00-Z99) does not affect the results (available upon request).

<sup>23</sup>Every outpatient visit and every day spent at a hospital is subject to a patient fee (about SEK 200 (USD 21) and SEK 100 (USD 11), respectively), but a ceiling limits the total amount a patient has to pay during a 12-month period.

<sup>24</sup>Table A1 provides an overview of the ten most common occupational groups in the central government sector for the period 2001-2011. College, university, and higher education teaching professionals (which encompass academic staff and PhD students) are the most common group as most major universities and university colleges are run as government agencies.

of PhD students and postdoctoral researchers being employed at that age. A larger share of people are married, have children, and are lone parents around age 40 than 30.

Important for the external validity of the results, central government employees are similar to private sector employees and a bit more dissimilar to employees in other parts of the public sector in terms of the three health measures considered in this study; see Figure A2. However, central government employees are higher educated than other employees and the share of female employees is lower/higher than in the remaining public sector/private sector.

## 4 Empirical strategy

The fundamental challenge with estimating the causal effect of paid vacation on health is the non-random assignment of different vacation entitlements to workers. In a “naive” regression of health on the number of vacation days, the estimated health effect would be biased downwards, if workers with poor health select into jobs with more vacation days. Conversely, if workers with better health select into jobs with more vacation days, the estimated health effect would be biased upwards.

In this study, I overcome this empirical challenge by taking advantage of provisions pertaining to central government employees in Sweden, which generate discontinuities in the number of paid vacation days at ages 30 and 40. A natural approach is to exploit these quasi-experimental settings in a regression discontinuity (RD) design. The idea would be to use the health outcome of employees who turn 29 (39) early in a calendar year (“control group”) as counterfactual for that of slightly older employees who turn 30 (40) late in the same calendar year (“treatment group”) and therefore are treated with additional vacation days during that year.

### 4.1 Invalidity of an RD design

The Swedish context poses certain threats to the two key identifying assumptions in a standard RD design. According to the first assumption, all predetermined and observable covariates that influence the outcome (health in this study) should be continuous across the threshold (Lee and Lemieux, 2010). This is unlikely to hold, since the control group and the treatment group constitute two consecutive birth cohorts. In Sweden, people born in a certain calendar year start school at the same time, and it is the norm that students who reach school leaving age finish the grade they began with. This means that people in the treatment group should have on average one more year of work experience and a correspondingly higher income than those in the control group in any given year. Furthermore, people in the treatment group born in December in a certain year and people in the control group born in January in the following year differ only by a single month in terms of age but might not be perfectly comparable. The calendar year-based design of the Swedish school system might have imprinted lasting differences on them due to relative

age effects (i.e., maturity differences) that affect students who are in the same class.<sup>25</sup> I formally check the continuity assumption for covariates in Table 2 by (i) comparing mean values in a window of 12 months on either side of the threshold in column 1 and (ii) running an RD regression in column 2. The results point indeed to significant discontinuities in certain covariates linked to health, such as sex, education, and income.

According to the second assumption, individuals should not be able to manipulate the running variable (age in this study) and precisely sort around the threshold (McCrary, 2008). Date of birth (and hence age) is not manipulable by individuals themselves. Their parents might however have timed the birth.<sup>26</sup> Figure A3 shows that the distribution of the number of central government employees around the threshold is not smooth. Panels (a) and (b) reveal a persistent seasonal pattern, in which most employees are born in March to May<sup>27</sup> and fewer are born in December than in January in the four-year age interval shown around ages 30 and 40.<sup>28</sup> The McCrary (2008) density test in columns 1 and 2 of Table A3 picks up this seasonal pattern and indicates a significant jump at the thresholds.<sup>29</sup> It should be noted though that this seasonal birth pattern is not specific to central government employees. According to Statistics Sweden, it can also be observed in every single Swedish cohort born between 1957 and 1984.

## 4.2 Setup

To address the challenges posed by the comparison of individuals from two consecutive birth cohorts, I combine the RD design with a difference-in-differences (DID) strategy to identify the causal effect.<sup>30</sup> The idea is to estimate the difference in health at a counterfactual threshold at age 28 (38) and then to subtract this difference from the one estimated at the actual threshold at age 30 (40). This cancels out time-invariant factors between two consecutive birth cohorts (such as all issues related to school starting age and the seasonal birth pattern discussed above). The identifying assumption that ensures that the effect of vacation on health can be interpreted as causal is then that any covariate that

<sup>25</sup>For instance, Plug (2001) shows that maturity differences within the class room influence school performance and earnings in the Netherlands, a country where people of the same birth cohort attend the same class, just as in Sweden.

<sup>26</sup>Fredriksson and Öckert (2014) show that parental education is continuous across the December-January threshold for the entire Swedish native population born 1935-1955. This indicates that Swedish parents did not time the birth of their children.

<sup>27</sup>Note that the peak in March to May is about nine months after the period when Swedes take most of their vacation days; see Panel (a) in Figure A5.

<sup>28</sup>Panel (a) in Figure A3 also shows that the number of observations is almost linearly increasing from age 27 to 30. This is related to two issues. First, many university graduates, who constitute a substantial share of central government employees, enter into employment at that age. Second, there are fewer observations aged 31 and increasingly fewer for every additional year below that age, as the register data are less complete for cohorts born after 1980. I address the latter issue in a robustness check.

<sup>29</sup>A related concern is that foreign-born central government employees lack reliable information on date of birth; see, e.g., Torun and Tumen (2017). These employees are excluded in Panels (c) and (d) in Figure A3, yet the patterns are very similar to Panels (a) and (b), indicating no concerns. The results from the density test differ neither from the full sample; see Panels A2 and B2 of Table A3. In a robustness check, I nevertheless exclude foreign-born employees.

<sup>30</sup>This kind of estimation strategy, which is sometimes also referred to as difference-in-regression discontinuity design, has been previously used by, e.g., Lalive (2008), Leonardi and Pica (2013), and Grembi et al. (2016).

affects health is either continuous across the threshold (as in the standard RD design) or its discontinuity is constant between consecutive birth cohorts (the parallel trend assumption in the standard DID design). As a result, the only reason that average health differs between those that turn 30 (40) and those that turn 29 (39) in a certain year is because the former group received an increase in its vacation entitlement. Figure 1 visualizes the empirical strategy.

I estimate the causal effect of paid vacation on health separately at ages 30 and 40 using the following linear regression model:

$$\begin{aligned} Health_{ijt} = & \alpha_0 + \alpha_1 age_{ijt}^* + T_{ijt}(\beta_0 + \beta_1 age_{ijt}^*) + \\ & C_{ijt}[\gamma_0 + \gamma_1 age_{ijt}^* + T_{ijt}(\delta_0 + \delta_1 age_{ijt}^*)] + \zeta_j + \eta_t + \epsilon_{ijt} \end{aligned} \quad (1)$$

in a pooled sample of individual  $i$  employed in government agency  $j$  in calendar year  $t$ . Health is measured in calendar year  $t$ , i.e., the same year as individuals are “treated” with additional vacation days. The normalized running variable, denoted  $age_{ijt}^* = age_{ijt} - age_c$ , is measured as age in months<sup>31</sup>, where  $age_c = 348.5$  (468.5) at age 30 (40) in the beginning of calendar year  $t$ . Note that the running variable is also normalized at the counterfactual threshold, which is set to two years before the actual threshold. Given a maximum bandwidth of 12 months on either side of each threshold,  $age_{ijt}^* = [-11.5, \dots, +11.5]$ , where individuals born in December in year  $t-30$  and  $t-28$  have a value of  $+0.5$  and those born in January in year  $t-29$  and  $t-27$  a value of  $-0.5$ .  $T_{ijt}$  is a dummy variable<sup>32</sup> indicating whether treatment with additional vacation days occurred in year  $t$ .  $C_{ijt}$  is another dummy variable<sup>33</sup> indicating whether an individual is located around the actual or the counterfactual threshold. The RD-DID estimator is  $\delta_0$ , the interaction of both dummies. I also include fixed effects for the government agency ( $\zeta_j$ ) and the calendar year ( $\eta_t$ ).  $\epsilon_{ijt}$  is an error term.

As a default setup, I use a local linear regression allowing for different trends on each side of the two thresholds and use the maximum bandwidth of 12 months on either side of each threshold.<sup>34</sup> I implement a more flexible functional specification allowing for

<sup>31</sup>This is the finest granulation of age available in the data.

<sup>32</sup>The treatment indicator is defined as

$$T_{ijt} = \begin{cases} 1 & \text{if } (age_c - 24) < age_{ijt} < (age_c - 12) \text{ or } age_{ijt} > age_c \\ 0 & \text{if } age_{ijt} < (age_c - 24) \text{ or } (age_c - 12) < age_{ijt} < age_c. \end{cases}$$

<sup>33</sup>The counterfactual indicator is defined as

$$C_{ijt} = \begin{cases} 1 & \text{if } age_{ijt} > (age_c - 12) \\ 0 & \text{otherwise.} \end{cases}$$

<sup>34</sup>An alternative approach would be to use a local linear regression with a triangular kernel (instead of a rectangular kernel as in my case) to put more weight on observations closer to the thresholds. I follow however the suggestion by Lee and Lemieux (2010) and vary the size of the bandwidth instead.

quadratic trends as a robustness check.<sup>35</sup> I also consider smaller bandwidths of 6 months and 1 month<sup>36</sup> on either side of each threshold. As the running variable is discrete, specification error is introduced into regression model (1). This can be addressed by clustering standard errors on the discrete values of the running variable (Lee and Card, 2008). Similar to Fredriksson and Öckert (2014), I cluster standard errors on the interaction of the discrete values of the running variable with each calendar year, yielding 360 (= 24 months \* 15 years) clusters in the case of a 12-month bandwidth, in order to reach a sufficient number of clusters in the estimations.

### 4.3 Validity of the RD-DID design

I formally check the usefulness of the RD-DID design to remedy problems with the discontinuity of certain baseline covariates that might have a direct impact on health as well as the seasonal birth pattern in Table 2. Column 3 shows that virtually all covariates are now continuous across the thresholds. Gross earnings and earned income (all measured in the year prior to treatment) are not continuous at conventional significance levels at age 30<sup>37</sup> but disposable income, which is relevant for investing into one’s health, is continuous. The implicit parallel trend assumption for baseline covariates can also be assessed visually at the thresholds preceding the actual threshold that separate different birth cohorts (those turning 28 from those turning 29, those turning 27 from those turning 28, etc.). Figure 2 shows that the size of the discontinuities is rather similar for sex, education, and disposable income at ages 30 and 40, even though the pattern is more easily discernible at age 40 than at age 30. Figure A4 shows this information for additional covariates and an extended age range. Furthermore, the modified McCrary (2008) density test no longer detects a significant jump in the number of observations at the actual thresholds; see columns 3 and 4 of Table A3. As a result, the main assumptions for an RD-DID design to be valid seem to be fulfilled.

As mentioned above, Swedish register data<sup>38</sup> do not contain information about when, to what extent, and in which way employees make use of their vacation entitlement. As a result, regression model (1) provides intention-to-treat (ITT) estimates of the effect of receiving additional paid vacation days on health. The interpretation of the size of the ITT estimates depends on whether additional vacation days induce a jump in the days taken, i.e., whether a first stage exists. The fact that vacation days cannot be paid out in money means that it is likely that they are all taken, provided that additional leisure time

<sup>35</sup>The estimated regression model with quadratic trends (d=2) on each side of the thresholds is

$$Health_{ijt} = \sum_{b=0}^d (\alpha_b age_{ijt}^{*b}) + T_{ijt} \sum_{b=0}^d (\beta_b age_{ijt}^{*b}) + C_{ijt} [\sum_{b=0}^d (\gamma_b age_{ijt}^{*b}) + T_{ijt} \sum_{b=0}^d (\delta_b age_{ijt}^{*b})] + \zeta_j + \eta_t + \epsilon_{ijt}. \quad (2)$$

I do not consider higher dimensional trends, as suggested by Gelman and Imbens (2019).

<sup>36</sup>The minimum 1-month bandwidth corresponds to a comparison of means between employees born in December in a certain year and employees born in January in the following year.

<sup>37</sup>This could stem from the fact that the earnings development is more erratic at younger ages.

<sup>38</sup>There are also no representative Swedish surveys or European surveys that include Sweden that contain suitable vacation-related information.

(plus the small extra vacation pay) is valued higher than additional working time. The legal restrictions, explained in section 2, on the minimum number of days to be taken every year and the maximum number of days that can be saved, should also induce workers to take all days. Empirically, there is only some suggestive evidence available that indicates that Swedes take all vacation days that they are entitled to (Expedia, 2014). Hence, the first stage should be strong.

Despite the legal restrictions, there might be some groups of employees that comply imperfectly with the “treatment”. In Sweden, all public universities and university colleges are run as government agencies and their employees therefore belong to the central government sector. Even though academic staff and PhD students might take all their vacation days every year on paper, it does not mean that they are actually used and that an increase in the vacation entitlement leads to fewer days worked. The same might be true for high-level staff in other government agencies.<sup>39</sup> It is also possible that employees engage in a temporary secondary employment during vacation. I address all of these cases of potential imperfect compliance in robustness checks.

RD designs based on age-activated treatments estimate the full effect of what happens at the age threshold (Lee and Lemieux, 2010). In Sweden only very little changes at the actual thresholds at ages 30 and 40 and nothing at the counterfactual thresholds at ages 28 and 38 that could potentially influence health and hence be picked up by the RD-DID estimator.<sup>40</sup> Furthermore, at ages 30 and 40 there could be a round-number birthday effect, if people persistently feel differently or change their health-related behavior right before and/or after turning 30 and 40. Evidence on the existence of a round-number birthday effect is lacking for Sweden, but results from, e.g., Germany indicate no such effect (Kühne et al., 2015).

## 5 Results

### 5.1 Main outcomes

In this section, I present the main estimates of the effects of receiving additional paid vacation days on employees’ health. The estimates are effectively based on a linear probability model, as the three health measures are dummy variables and ordinary least squares

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<sup>39</sup>Goerke et al. (2015) find an association between trade union membership and days of paid vacation taken in Germany. Union membership is not a concern among central government employees in Sweden, as everyone is covered by the ALFA collective agreement irrespective of actual membership.

<sup>40</sup>On July 1, 2008, a small earmarked lump sum that can be used to pay for dental care was introduced. The year one turns 30 the annual lump sum is halved from SEK 300 (USD 32) to SEK 150 (USD 16). It is doubtful that this reduction leads to swift changes in dental health that in turn affect the health measures used in this study. In a robustness check, I exclude the years affected by this policy. At age 40, women in Sweden receive their first invitation letter for breast cancer screening (which used to come along with a small patient fee of SEK 150 (USD 16)). This routine started in a small part of Sweden in 1986 and had been gradually extended, but in 2005 the whole country was still not covered (Hellquist et al., 2011). This might have an impact on the health measures in this study, as positive test results are subject to further analysis and might lead to treatment at a hospital. As a robustness check, I carry out the analysis separately for women and men as well as diagnoses related to breast cancer screening and breast cancer treatment.



estimation is used. The estimated coefficients of the treatment effect can therefore be interpreted as probabilities of at least once visiting specialized outpatient care, being admitted to inpatient care, and being on long-term sick leave, respectively. Table 3 presents the numerical results and Figure 3 depicts the results graphically.

The main results in columns 1 to 3 in Panel A and B of Table 3 indicate no statistically significant effects at ages 30 and 40 for any health measure. The point estimate for outpatient visits at age 30 suggests a 0.55 percentage points (pp) lower probability (a 2% decrease relative to the pre-extension mean probability of 26.7%) of making a visit at the threshold, comparing employees who turn 30 and receive three additional vacation days to employees turning 29 while at the same time deducting the difference in the outcome at the counterfactual threshold comparing employees turning 28 to those turning 27. The point estimate at age 40 suggests a 0.96 pp lower probability of making a visit when receiving four additional vacation days (a 4% decrease relative to the pre-extension mean probability of 26.9%). The point estimates for inpatient admissions correspond to a 1% increase at age 30 and an 8% decrease at age 40 in the probability of being admitted relative to the pre-extension mean probabilities. The point estimates for long-term sick leaves correspond to a relative decrease by 1% at age 30 and no change (0%) at age 40.

The standard errors of the main estimates in Table 3 are rather large, which means that I cannot rule out economically large effects.<sup>41</sup> To increase the variation in the outcome measure, I consider a single indicator for experiencing any of the three types of health events in column 4 of Table 3.<sup>42</sup> This increases the precision of the estimates, and the 95% confidence interval (CI) excludes relative improvements in health larger than 9% at age 30 and 7% at age 40 as well as a relative deterioration in health larger than 4% at both age 30 and 40. To further increase precision, I consider a summary index of the three health measures in column 5 of Table 3.<sup>43</sup> The treatment effect at age 30 implies an improvement in health by 2.3% of a standard deviation (see Panel A) and by 2.1% of a standard deviation at age 40 (see Panel B), but these results are still statistically insignificant. In a final attempt to increase precision, I stack the data at ages 30 and 40 in order to increase the sample size in Panel C of Table 3. This reduces the standard

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<sup>41</sup>When standard errors are clustered at the 24 distinct values of the running variable, they decrease markedly and the point estimate for outpatient visits at age 40 is rendered significant at the 10% level. As pointed out by Kolesár and Rothe (2018), clustering standard errors by the running variable leads to biased confidence intervals. They recommend conventional heteroscedasticity-robust standard errors if the bandwidth is small. The use of heteroscedasticity-robust standard errors or unadjusted standard errors yields virtually identical confidence intervals as the clustered standard errors in Table 3 (results available upon request). It should also be noted that the standard errors for outpatient visits in Table 3 are smaller than of the two other health measures, as this measure has more variation.

<sup>42</sup>Here, I am forced to rely on a sample including the years 2001 to 2011, as data on specialized outpatient care are not available prior to 2001.

<sup>43</sup>As discussed by, e.g., Kling et al. (2007) and Hoynes et al. (2016), aggregating multiple outcome measures can improve statistical power. The summary index is the arithmetic average of the z-scores of the three health measures. The z-score is calculated by subtracting the mean of a health measure from the observed outcome and dividing the difference by the standard deviation of the health measure. I use the mean and the standard deviation of the control group (aged 27 or 29) in calculating the z-score separately around the actual and the counterfactual threshold. Thus, all three components of the index have mean 0 and standard deviation 1 for the control group. I re-normalize the summary index to have standard deviation 1.

errors of all estimates but leaves them insignificant. For instance, the point estimate in column 5 in Panel C for the health index suggests an improvement in health by 2.2% of a standard deviation. It narrowly misses to reach statistical significance (p-value: 0.115), but improvements in health larger than 5% of a standard deviation and a deterioration in health larger than 0.5% of a standard deviation can be ruled out based on the 95% CI. Overall, the estimated effect sizes are small and statistically indistinguishable from zero and I can rule out economically large effects.<sup>44</sup> There is thus no evidence that changes at the intensive margin of paid vacation entitlement affect health among younger employees.

## 5.2 Robustness

I perform a battery of tests to check the robustness of the main outcomes; see Tables A4 and A5. To start with, I investigate the model specification in Panel B. I add controls for sex, highest completed level of education, country of birth, being a parent with children aged 0-17, being married, and disposable income (measured in year  $t-1$ ) to model (1). The point estimates and standard errors barely change in magnitude. This lends credibility to the RD-DID design as all (observed and unobserved) covariates influencing health should be continuous across the threshold. I also implement a quadratic specification according to regression model (2), since there is not necessarily a linear trend in the outcomes. The results remain statistically insignificant. Local linear estimates relying on a smaller bandwidth of 6 months on either side of the thresholds indicate no significant effects. The same is true for the minimum bandwidth of 1 month.<sup>45</sup> The exclusion of the two fixed effects for government agency and calendar year also leaves the estimates insignificant.

In Panel C of Tables A4 and A5, I consider whether potential non-compliers with the treatment drive the main results. One group potentially containing non-compliers are foreign-born employees, due to inaccurate birth dates. Another group are employees with temporary secondary employments, who might use these employments to work during vacation from their main job. The occupational groups of college, university, and higher education teaching professionals as well as legislators, senior officials, and managers might not comply with the provisions on vacation entitlement in general and hence might not be affected by changes in the entitlement. The exclusion of all of these groups does not yield significant estimates. I also restrict the sample to the years 1997 to 2006, due to issues related to individually-negotiated vacation entitlements since 2007 (see section 2), a change in the maximum length of paid sick leaves in 2008 (see section 3), incomplete coverage of cohorts born after 1980 in the SIP database (see section 4.1), and the introduction of an

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<sup>44</sup>Another conceivable approach to increase statistical power is to use employees in non-government sectors as controls. However, it is impossible to identify workers that are covered by a certain collective agreement outside the central government sector unambiguously in the SIP database. In addition, other collective agreements often contain an increase in the vacation entitlement at age 40 but give employees the choice to decline it and receive a salary increase instead.

<sup>45</sup>An alternative approach would be to choose a small bandwidth (to reduce bias) and at the same time increase the size of the control group (to increase precision), as done by, e.g., Carneiro et al. (2015). Using a bandwidth of 6 months and defining observations around the two thresholds at age 27-28 and age 28-29 or around the three thresholds at ages 26-27, 27-28, and 28-29 (and at analogous thresholds at age 40) as the control group, I also obtain insignificant results (results available upon request).

earmarked lump sum for dental care services in 2008 (see section 4.3). The results indicate no significant effects for any health measure, in line with the results for the full sample.

In the analysis above, I always measured health outcomes during the entire year in which the treatment with additional vacation days occurs. Panel (a) in Figure A5 shows that most vacation days are taken during June to August among the whole Swedish working population.<sup>46</sup> Provided that central government employees behave similarly and use the additional vacation days to prolong a vacation during the summer, effects on health might be more concentrated during these months or the months thereafter. Panel (b) in Figure A5 shows indeed a dip in the number of health care contacts and long-term sick leaves in June to August among the Swedish working population. However, the correlation between the timing of vacation and health events could be spurious, as (i) health care workers are also on vacation and hence fewer non-acute medical services are scheduled, and (ii) many workers are vacationing abroad and do not use the Swedish health care system. Estimation results in which the health outcomes are only measured during the months of January to May, June to August, and September to December in year  $t$  are shown in Panel D of Tables A4 and A5. The estimates are insignificant, except for a 0.63 pp lower probability (a 22% decrease relative to the pre-extension mean probability, significant at the 5% level) of being admitted to inpatient care at age 40 in January to May, which is counterbalanced by a 0.40 pp higher admission probability (a 19% increase, significant at the 10% level) in September to December.

As a final robustness check, I run a set of placebo regressions in which I move the actual and the counterfactual thresholds one year backwards. Thus, instead of comparing employees turning 29 (39) to those turning 30 (40), those turning 28 (38) are now compared to those turning 29 (39). Neither the ones that turn 28 (38) nor those that turn 29 (39) receive additional vacation days, and there are no other age-activated treatments that are triggered at any thresholds. Therefore, there is no obvious reason for employees' health to change at these placebo thresholds. Table A6 shows the results of the placebo regressions conducted for the main outcomes reported in Table 3. There are no significant effects for any of the three measures of employees' health. Placebo regressions for all diagnosis groups and subgroups considered below yield also overwhelmingly insignificant results (results available upon request). In sum, the insignificant results from the placebo regressions lend strong support to the validity of the main results.

### 5.3 Results by diagnosis group

Table 4 reports estimates of regression model (1) for the ten most common groups of diagnoses instead of looking at all cases together.<sup>47</sup> For outpatient visits, there are no significant results for any diagnosis group at ages 30 and 40, except for one estimate at age 40 that indicates a 0.87 pp lower probability (a 12% decrease relative to the pre-

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<sup>46</sup>The concentration of vacation taken during the summer is due to a provision in the Annual Leave Act, which entitles every worker to at least four weeks of uninterrupted vacation leave during June to August.

<sup>47</sup>The results for the ten least common groups (which include circulatory diseases, ICD-10 code I00-I99) are overwhelmingly insignificant (results available upon request).

extension mean probability, significant at the 10% level) of a visit due to factors influencing health status and contact with health services (ICD-10 code Z00-Z99). For inpatient admissions, there are neither any significant results. For long-term sick leaves, there are only two significant estimates (both at the 10% level) at age 30 indicating a lower probability of a sick leave due to neoplasms (C00-D48) and a higher probability of a sick leave due to ill-defined symptoms and conditions (R00-R99). The diagnosis-specific results for sick leave should be interpreted with caution though, as information on the underlying diagnosis is missing for more than half of all cases in the SIP database. Notably, there are no significant results for mental and behavioral disorders (F00-F99), which includes depression and anxiety.<sup>48</sup>

#### 5.4 Results by subgroup

Table 5 presents estimates of regression model (1) for different subgroups. Even though the three health measures differ considerably in magnitude between female and male employees (see Figure A1), the sex-specific results indicate no significant effects of receiving additional vacation days. I also consider married and unmarried employees, employees with and without any children aged 0-17, as well as lone parents and the full sample excluding lone parents. The results indicate no significant effects, except for a 3.14 pp lower probability (an 11% decrease relative to the pre-extension mean probability, significant at the 10% level) of making an outpatient visit at age 40 in the sample of employees without children. In the samples of employees with and without a university degree as highest completed level of education, there are neither any significant effects. Likewise, there are no significant effects in the sample of employees with potentially worse underlying health status, defined as having experienced any of the three types of health events in the three years prior to the year of treatment.

#### 5.5 Non-health outcomes

There is a range of non-health outcomes that could be affected by the increases in the vacation entitlement. Gross earnings is a measure of productivity that is partly determined by employees' health. In Sweden, earnings are especially susceptible to employees' health, as the first day of a sickness period is not remunerated and subsequent days at less than 100% of the ordinary salary; see section 3. Table A7 shows that there are no significant differences in annual gross earnings at the discontinuities at ages 30 and 40. The same is true for earned income, which includes social security benefits on top of gross earnings,

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<sup>48</sup>One threat to the RD-DID design is the start of the breast cancer screening program for women at age 40. However, for outpatient visits at age 40, there are no significant estimates (neither in the female subsample nor in the full sample) for the diagnosis group Z01, which includes routine mammography, the diagnosis group Z12, which includes examination for breast cancer, the more general diagnosis group Z00-Z13, which comprises persons encountering health services for examination and investigation, and the diagnosis group C50, which encompasses breast cancer treatment. For inpatient admissions, there are virtually no observations with diagnosis codes Z01 and Z12 and only about 100 female observations with diagnosis codes Z00-Z13 and C50 in the four-year age interval around age 40, but the estimates for the latter two diagnosis groups are insignificant as well.

as well as for disposable income. These results might indicate that short-term sick leaves (lasting at most 14 days) are not affected by increases in vacation days. Table A7 also shows that neither the probability of having at least one temporary secondary employment nor the probabilities of being married or being a (lone) parent change significantly as employees receive more vacation days.

## 6 Discussion

The study provides ITT effects, as it is not possible to observe how many vacation days an employee uses every year. Employees who receive an increase in vacation days might react in different ways; they might use those days, save them for next year, not use them at all, formally use them but then still work from home, or work more in a temporary secondary employment. From a policy perspective, ITT effects are the relevant measures, as they reflect the effectiveness of the age-based rules in practice. They only correspond to the true magnitudes of the causal effects of additional vacation days if there is perfect compliance. Below, I discuss possible explanations for the small and statistically insignificant ITT effects.

In terms of mechanisms relating to the production of individual health in this study, any effect on health may only run through the time endowment channel, as additional paid vacation days leave annual earnings unchanged (the latter is supported by the results in Table A7). Whether additional vacation days actually reduce annual working hours and hence increase leisure time cannot be observed in the data, but as argued in section 4.3, the first stage should be strong. However, even if all additional vacation days are spent as leave from work, annual working hours might still not decrease if the annual workload stays constant. The workload would then have to be worked off through overtime or higher work pace. Yet at least for Canada and the US, empirical evidence does not support the existence of such a workload effect, as annual working hours decrease almost proportional to every week of vacation actually used (Altonji and Usui, 2007; Fasih, 2014).

I identify the health effects at the intensive margin of vacation entitlement and the relationship between health and vacation days might be concave. Even if leisure time increases proportional to the number of vacation days, the size of the change in the number of vacation days in relation to the baseline entitlement is arguably important. Adding three days on top of 28 days or four days on top of 31 days constitute comparatively modest changes. Provided that there are positive yet marginally decreasing health returns to vacation days, these changes would bring about absolute increases in health that might be too small to detect even in a large sample.

The context in which the results are obtained might be important, which is also relevant for the external validity of the results. Swedish workers have, according to the OECD<sup>49</sup>, a very good work-life balance, even in comparison to other wealthy countries. Apart from generous vacation entitlements, it is possible and fairly common to work anything

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<sup>49</sup><http://www.oecdbetterlifeindex.org/topics/work-life-balance/> (accessed November 12, 2018)

between 20% to 90% of full-time in Sweden.<sup>50</sup> It is also possible for workers to take unpaid vacation if the employer agrees. It is therefore comparatively easy to optimize the choice between working time and leisure time according to one’s preferences. The work hours constraint, which has been empirically linked to health (see, e.g., Bell et al. (2012)), might thus be barely affected by the receipt of additional vacation days. While these labor market features apply to the entire workforce, central government employees enjoy the most generous vacation entitlements among all workers in Sweden. They might derive well-being and consequently good health from this favorable context in itself, and not from the actual act of receiving additional vacation days. However, central government employees appear to be similar in terms of health to employees in the private sector, who represent the vast majority of the Swedish workforce. This corroborates the external validity of the results.

Lee and Lemieux (2010) also point to a context-dependent interpretation of results obtained from RD designs with “discontinuities in age with inevitable treatment”, as the notion of randomness of treatment is different. As every employee is eventually treated with additional vacation days, there is no ex ante uncertainty about the receipt of treatment. Employees may fully anticipate the treatment and change their behavior prior to its receipt, which might either accentuate or mute any observed effect. In the context of this study, a threat to the empirical design could be that employees work harder during the year(s) prior to the increase in the vacation entitlement, if they think that they can use the additional vacation days in the following year(s) to recover. It is difficult to address this issue practically, but the fact that the vacation entitlement is age-based and not performance-based means that there is no obvious reason for increased work effort or motivational effects.

Previous literature offers some support for the findings in this study. Small and short-lived effects of a vacation (irrespective of its length) on health and well-being are an established result in the empirical literature in psychology and occupational medicine. If the effects are indeed only transitory and disappear soon after returning to work, adding three or four days to the total vacation entitlement (which could be used to extend a certain vacation or for an own short vacation) might not have a profound impact on health.

Owing to the age-activated treatment in this study, there is a one-year time window to detect any health effects, as employees in the control group switch after one year to the treatment group and those who have been previously in the treatment group are treated for a second time. Effects that take longer than one year to materialize cannot be detected by the RD-DID estimator. In the light of the short-lived effects found in the aforementioned literature, it is not obvious to presume the existence of longer-run health effects. The approach in this study to estimate the health effects in the same year as the increase in the vacation entitlement occurs thus seems reasonable.<sup>51</sup>

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<sup>50</sup>Part-time workers cannot be identified in the SIP database.

<sup>51</sup>Studies that estimate the causal health effects of changes in annual or weekly working hours also measure workers’ health in the same year as the change in working time occurs; see Hummels et al. (2016); Berniell and Bietenbeck (2017); Lepinteur (2019).

The way in which I measure health is arguably important. The available data only permit the construction of objective yet rather “hard” measures of health. Other objective measures such as visits to primary health care or short-term sick leaves might be more susceptible to changes in the number of vacation days. Effects on short-term sick leaves fail however to find an expression in the effects on earnings in Table A7. Nevertheless, the three measures considered in this study are relevant for economic evaluations of changes in the number of paid vacation days.

## 7 Conclusion

The introduction and extension of paid vacation has historically been motivated by an effort to protect and maintain workers’ health by giving workers time to rest and recover. In this study, I provide causal evidence on the effects of increases at the intensive margin of paid vacation entitlement on workers’ health. I obtain small and statistically insignificant effects of receiving a permanent increase by three (four) additional paid vacation days at age 30 (40) on different measures of health in a sample of all central government employees in Sweden. These findings challenge the validity of the health-related argument for more paid vacation days (at least among younger workers) when put to the test in a modern context.

The results have also policy implications, noting though that the results are based on ITT effects. Firstly, granting workers additional paid vacation days, if they already have relatively generous vacation entitlements, might be a poor policy measure to improve their health. To achieve improvements in health, it might be more expedient to focus on factors that influence their day-to-day work, such as daily working hours, working overtime, or workplace well-being. Secondly, raising the vacation entitlement is a means of working time reduction. Such a measure would have a negative direct effect on total annual production per worker due to fewer days worked. The extent of an offsetting positive indirect effect on total annual production per worker due to healthier and more productive workers might be limited.

## Compliance with Ethical Standards

Conflict of Interest: The author declares no conflict of interest.

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## Figures and Tables

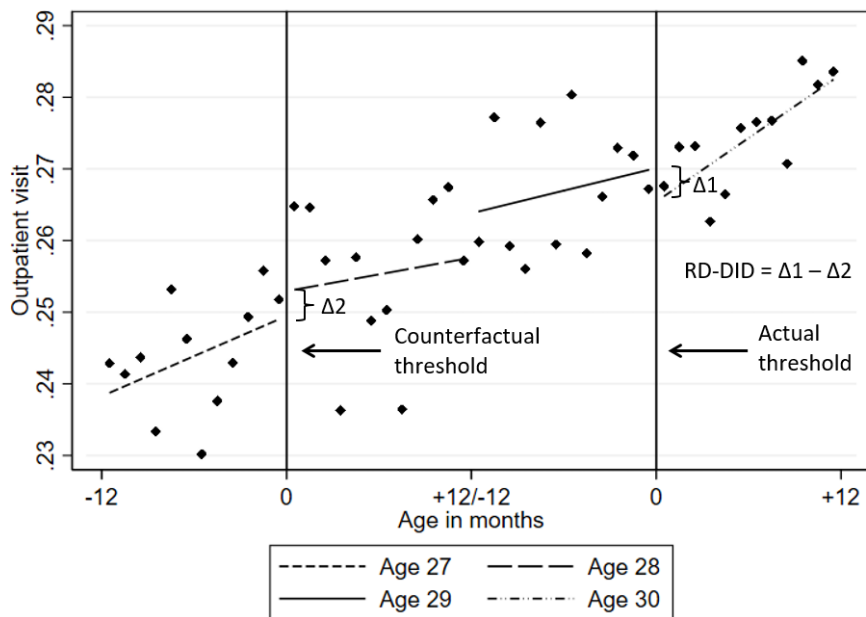
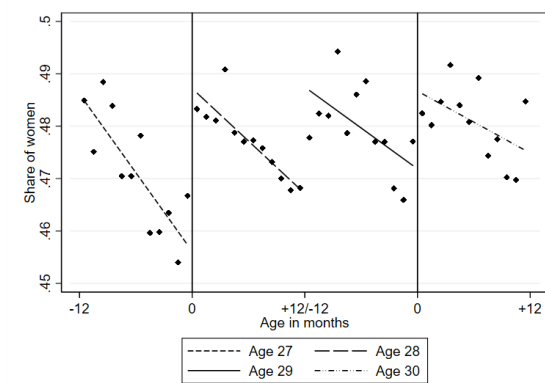
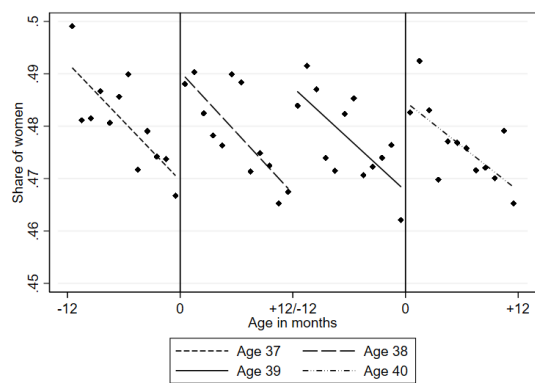


Figure 1: Visualization of the RD-DID design at age 30

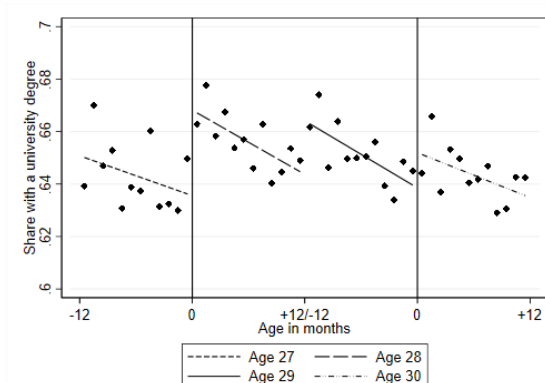
Notes: The RD-DID estimator takes the vertical distance between the fitted lines at the counterfactual (left) threshold and subtracts it from the vertical distance between the fitted lines at the actual (right) threshold. The resulting difference is the RD-DID estimator,  $\delta_0$ , in regression model (1). The bin size is one month.



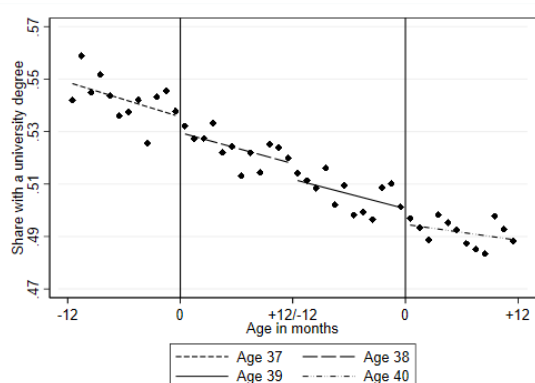
(a) Share of women (age 30)



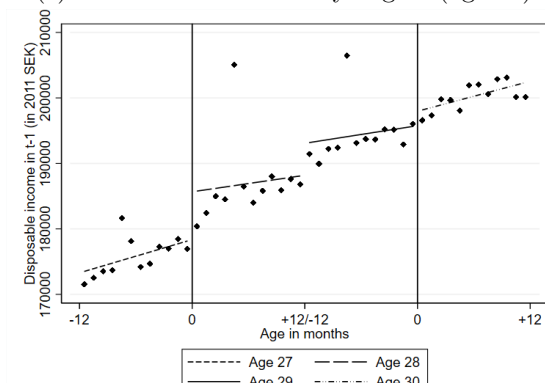
(b) Share of women (age 40)



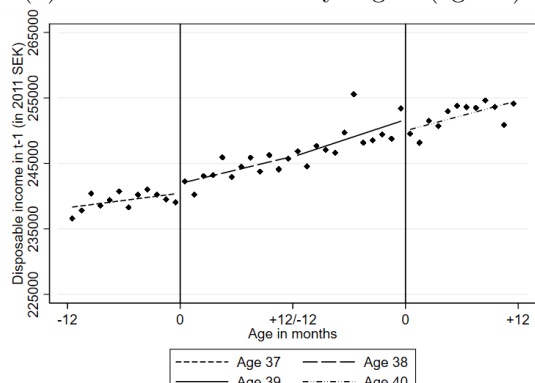
(c) Share with a university degree (age 30)



(d) Share with a university degree (age 40)



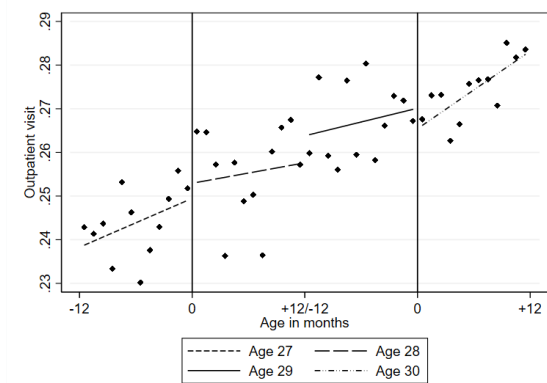
(e) Disposable income (age 30)



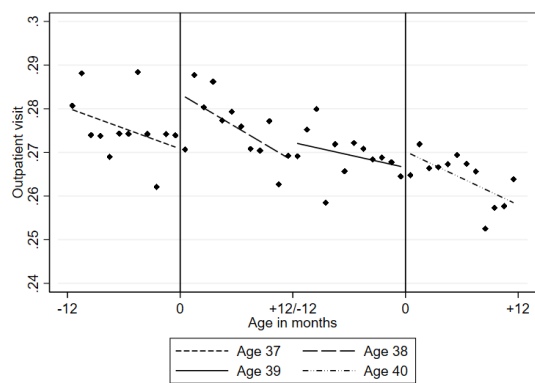
(f) Disposable income (age 40)

Figure 2: RD-DID plots for selected baseline covariates

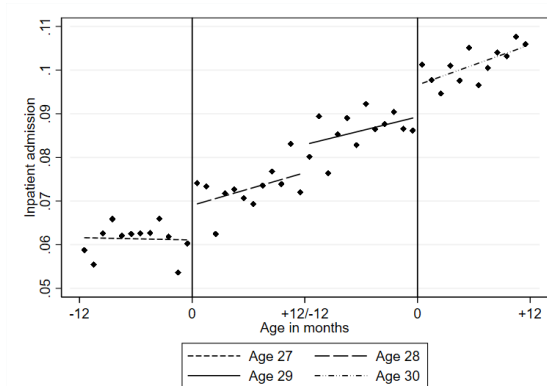
Notes: The bin size is one month. The zeros on the x-axis mark the counterfactual (left) threshold and the actual (right) threshold. The whole four-year age interval used in the empirical analysis is shown.



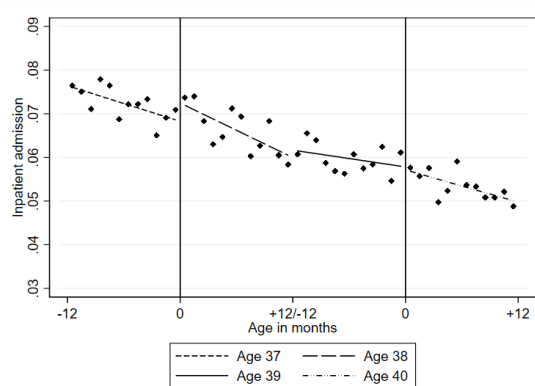
(a) Outpatient visit (age 30)



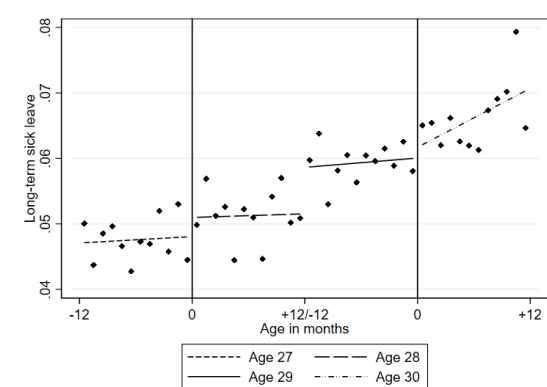
(b) Outpatient visit (age 40)



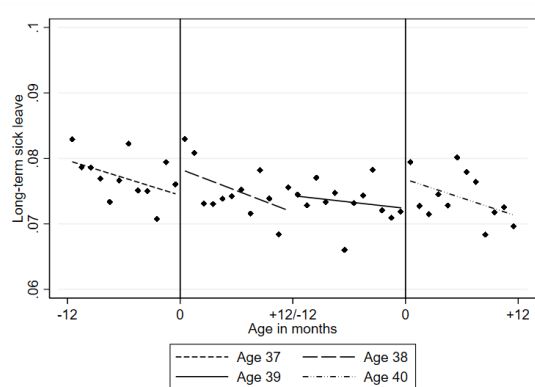
(c) Inpatient admission (age 30)



(d) Inpatient admission (age 40)



(e) Long-term sick leave (age 30)



(f) Long-term sick leave (age 40)

Figure 3: RD-DID plots for main outcomes

Notes: The bin size is one month. The zeros on the x-axis mark the counterfactual (left) threshold and the actual (right) threshold. The whole four-year age interval used in the empirical analysis is shown.

Table 1: Descriptive statistics

	Mean [Standard deviation]	
	Age 30	Age 40
<i>Health measures</i>		
Any outpatient visit†	.262 [.440]	.271 [.444]
Any inpatient admission	.083 [.275]	.063 [.243]
Any long-term sick leave	.057 [.232]	.075 [.263]
Any health event†	.310 [.462]	.314 [.464]
Health index†	.024 [1.022]	-.008 [.992]
<i>Socio-economic characteristics</i>		
Year of birth	1975.4 [4.4]	1966.0 [4.4]
Female	.477 [.499]	.478 [.500]
Born abroad	.090 [.286]	.091 [.287]
Non-Swedish citizen	.051 [.221]	.041 [.199]
No high school education	.011 [.104]	.022 [.146]
High school education	.141 [.348]	.233 [.423]
Some tertiary education	.200 [.400]	.229 [.420]
University graduate	.648 [.478]	.516 [.500]
Married	.206 [.404]	.518 [.500]
Any children, aged 0-17	.258 [.438]	.773 [.419]
Lone parent	.014 [.118]	.073 [.259]
Any temporary secondary employment	.234 [.424]	.237 [.425]
Gross earnings (in 2011 SEK)	261,638 [99,286]	321,027 [126,339]
Earned income (in 2011 SEK)	276,259 [81,175]	338,541 [113,868]
Disposable income (in 2011 SEK)	209,369 [512,431]	258,129 [107,561]
Any health event, previous year†	.211 [.408]	.269 [.443]
Any health event, two previous years†	.295 [.456]	.397 [.489]
Any health event, three previous years†	.340 [.474]	.481 [.500]
Observations	182,326	245,965

Notes: The data are based on the pooled sample for 1997-2011 and cover the four-year age interval used at each age in the empirical analysis. Observations fulfilling the inclusion criteria defined in section 3.1 but with missing information on either sex, education, country of birth, civil status, or income in year  $t$  or  $t-1$  (0.54% at age 30 and 0.52% at age 40 in the four-year age interval) are excluded. † There are fewer observations (135,744 and 190,498, respectively) for these variables, as no data for outpatient visits from before 2001 are available. A health event is defined as having had at least one outpatient visit, one inpatient admission, or one long-term sick leave. The lagged versions of 'any health event' lack data for outpatient visits prior to 2001. The use of only inpatient admissions and long-term sick leaves to define a health event yields qualitatively similar results. The health index is standardized to have a mean of 0 and a standard deviation of 1 in the control group (aged 29).

Table 2: Test of the continuity of baseline covariates

	(1)	(2)	(3)
	p-values based on ...		
	Means	RD	RD-DID
<i>Panel A: Age 30</i>			
Female	.686	.013	.393
Born abroad	.064	.000	.113
Non-Swedish citizen*	.009	.000	.406
No high school education	.617	.315	.840
High school education	.006	.000	.229
Some tertiary education	.435	.003	.589
University graduate	.011	.875	.210
Married*	.000	.000	.677
Any children, aged 0-17*	.000	.000	.595
Gross earnings (in 2011 SEK)*	.000	.002	.001
Earned income (in 2011 SEK)*	.000	.000	.000
Disposable income (in 2011 SEK)*	.000	.002	.141
Any health event, previous year†	.000	.001	.502
Any health event, two previous years†	.000	.000	.504
Any health event, three previous years†	.000	.000	.526
Observations	102,348	102,348	182,326
<i>Panel B: Age 40</i>			
Female	.644	.003	.681
Born abroad	.561	.017	.489
Non-Swedish citizen*	.066	.000	.797
No high school education	.007	.997	.999
High school education	.000	.023	.895
Some tertiary education	.214	.486	.563
University graduate	.000	.125	.685
Married*	.000	.306	.206
Any children, aged 0-17*	.000	.065	.542
Gross earnings (in 2011 SEK)*	.000	.249	.677
Earned income (in 2011 SEK)*	.000	.978	.367
Disposable income (in 2011 SEK)*	.000	.447	.160
Any health event, previous year†	.049	.531	.171
Any health event, two previous years†	.423	.220	.028
Any health event, three previous years†	.220	.184	.015
Observations	123,568	123,568	245,965

Notes: The table shows p-values, while the estimated coefficients can be found in Table A2. The p-values in column 1 refer to a t-test of the difference in means in the 12-month window on either side of the actual threshold. The p-values in column 2 (column 3) refer to the coefficient of the treatment indicator in a local linear RD (RD-DID) regression with separate trends using a 12-month bandwidth on either side of the actual (each) threshold. The regressions include fixed effects for calendar year and government agency and standard errors are clustered at the discrete values of the running variable multiplied by the calendar year. \* These time-varying covariates are measured in year  $t-1$ , i.e., the year prior to treatment. The data are based on the pooled sample for 1997-2011. † There are fewer observations for these variables, as no data for outpatient visits from before 2001 are available.

Table 3: Main results

	(1)	(2)	(3)	(4)	(5)
	Outpatient care	Inpatient care	Sick leave	Any health event	Health index
<i>Panel A: Age 30</i>					
Treatment effect	-0.0055 (0.0097)	0.0008 (0.0052)	-0.0007 (0.0046)	-0.0085 (0.0105)	-0.0226 (0.0221)
Observations	135,744	182,326	182,326	135,744	135,744
R-squared	0.022	0.015	0.014	0.023	0.021
Pre-extension mean	0.2672	0.0863	0.0594	0.3178	0.0000
<i>Panel B: Age 40</i>					
Treatment effect	-0.0096 (0.0084)	-0.0047 (0.0038)	0.0000 (0.0044)	-0.0055 (0.0085)	-0.0211 (0.0183)
Observations	190,498	245,965	245,965	190,498	190,498
R-squared	0.014	0.006	0.011	0.012	0.010
Pre-extension mean	0.2693	0.0597	0.0733	0.3111	0.0000
<i>Panel C: Age 30+40 (stacked)</i>					
Treatment effect	-0.0086 (0.0063)	-0.0026 (0.0031)	-0.0002 (0.0032)	-0.0071 (0.0065)	-0.0219 (0.0138)
Observations	326,242	428,291	428,291	326,242	326,242
R-squared	0.015	0.006	0.011	0.014	0.012
Pre-extension mean	0.2684	0.0715	0.0671	0.3140	0.0000

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each coefficient represents a separate regression. Standard errors in parentheses are clustered at the discrete values of the running variable multiplied by the calendar year (i.e., 360 clusters in columns 2 and 3, and 264 clusters in columns 1, 4, and 5, as data on outpatient care are only available since 2001). Estimations are based on regression model (1), a local linear RD-DID design allowing for different trends on each side of the thresholds using a 12-month bandwidth on either side of each threshold and including fixed effects for calendar year and government agency but no controls. The pre-extension mean refers to the 12-month period before the actual threshold.



Table 4: Results by diagnosis group

	Outpatient care	Inpatient care	Sick leave
<i>Panel A: Age 30</i>			
- Neoplasms (C00-D48)	-0.0002 (0.0027)	-0.0002 (0.0006)	-0.0007* (0.0004)
- Mental & behavioral disorders (F00-F99)	0.0021 (0.0021)	-0.0004 (0.0008)	-0.0012 (0.0019)
- Diseases of the respiratory system (J00-J99)	-0.0014 (0.0027)	-0.0002 (0.0008)	-0.0004 (0.0007)
- Diseases of the digestive system (K00-K93)	-0.0007 (0.0025)	-0.0005 (0.0012)	0.0005 (0.0007)
- Musculoskeletal diseases (M00-M99)	-0.0017 (0.0032)	0.0008 (0.0009)	0.0015 (0.0013)
- Diseases of the genitourinary system (N00-N99)	-0.0046 (0.0039)	-0.0003 (0.0008)	0.0004 (0.0003)
- Pregnancy & childbirth (O00-O99)†	-0.0083 (0.0075)	-0.0033 (0.0085)	0.0009 (0.0034)
- Symptoms not elsewhere classified (R00-R99)	-0.0013 (0.0037)	0.0015 (0.0013)	0.0012* (0.0007)
- Injuries & poisoning (S00-T98)	-0.0036 (0.0040)	-0.0006 (0.0014)	-0.0010 (0.0009)
- Other contacts with health services (Z00-Z99)	-0.0034 (0.0059)	0.0010 (0.0008)	-0.0005 (0.0004)
Observations	135,744	182,326	182,326
<i>Panel B: Age 40</i>			
- Neoplasms (C00-D48)	0.0033 (0.0029)	-0.0009 (0.0008)	-0.0003 (0.0006)
- Mental & behavioral disorders (F00-F99)	0.0014 (0.0018)	-0.0003 (0.0007)	0.0008 (0.0020)
- Diseases of the respiratory system (J00-J99)	-0.0013 (0.0023)	0.0002 (0.0007)	0.0003 (0.0010)
- Diseases of the digestive system (K00-K93)	-0.0004 (0.0024)	-0.0015 (0.0011)	0.0000 (0.0006)
- Musculoskeletal diseases (M00-M99)	-0.0011 (0.0033)	-0.0003 (0.0010)	-0.0013 (0.0015)
- Diseases of the genitourinary system (N00-N99)	-0.0004 (0.0039)	-0.0004 (0.0009)	0.0000 (0.0005)
- Pregnancy & childbirth (O00-O99)†	-0.0033 (0.0052)	-0.0061 (0.0061)	-0.0013 (0.0027)
- Symptoms not elsewhere classified (R00-R99)	-0.0009 (0.0034)	0.0014 (0.0011)	-0.0000 (0.0007)
- Injuries & poisoning (S00-T98)	-0.0005 (0.0037)	-0.0005 (0.0011)	0.0001 (0.0009)
- Other contacts with health services (Z00-Z99)	-0.0087* (0.0050)	0.0012 (0.0007)	-0.0002 (0.0004)
Observations	190,498	245,965	245,965

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each coefficient represents the treatment effect from a separate regression. Standard errors in parentheses are clustered at the discrete values of the running variable multiplied by the calendar year. Estimations are based on regression model (1) using a 12-month bandwidth. The diagnosis group-specific outcomes are based on the main diagnosis underlying the health event. † This outcome is based on the subsample of women (67,213 / 87,000 observations at age 30 and 94,123 / 117,568 observations at age 40).

Table 5: Results by subgroup

	Outpatient care	Inpatient care	Sick leave	Observations‡
<i>Panel A: Age 30</i>				
- Full sample	-0.0055 (0.0097)	0.0008 (0.0052)	-0.0007 (0.0046)	135,744 / 182,326
- Men	-0.0061 (0.0131)	-0.0015 (0.0043)	-0.0004 (0.0046)	68,531 / 95,326
- Women	-0.0067 (0.0144)	0.0010 (0.0094)	-0.0013 (0.0080)	67,213 / 87,000
- Married	-0.0014 (0.0239)	0.0087 (0.0157)	0.0036 (0.0134)	26,820 / 37,552
- Unmarried	-0.0065 (0.0106)	-0.0036 (0.0055)	-0.0013 (0.0047)	108,924 / 144,774
- Excluding parents†	-0.0168 (0.0106)	-0.0027 (0.0040)	0.0014 (0.0040)	101,875 / 135,234
- Parents†	0.0245 (0.0250)	0.0193 (0.0178)	-0.0013 (0.0149)	33,869 / 47,092
- Excluding lone parents†	-0.0042 (0.0099)	0.0001 (0.0052)	-0.0005 (0.0045)	133,963 / 179,766
- Lone parents†	-0.0477 (0.0999)	0.0551 (0.0660)	0.0065 (0.0681)	1,781 / 2,560
- With university degree	0.0006 (0.0112)	0.0022 (0.0064)	0.0003 (0.0049)	91,280 / 118,136
- Without university degree	-0.0221 (0.0189)	-0.0047 (0.0093)	-0.0042 (0.0088)	44,464 / 64,190
- Previous health events (last three years)	0.0218 (0.0199)	0.0099 (0.0129)	-0.0014 (0.0118)	46,175
<i>Panel B: Age 40</i>				
- Full sample	-0.0096 (0.0084)	-0.0047 (0.0038)	0.0000 (0.0044)	190,498 / 245,965
- Men	-0.0092 (0.0105)	-0.0006 (0.0039)	0.0060 (0.0049)	96,375 / 128,397
- Women	-0.0070 (0.0118)	-0.0071 (0.0072)	-0.0056 (0.0073)	94,123 / 117,568
- Married	0.0004 (0.0112)	-0.0033 (0.0059)	0.0009 (0.0061)	97,610 / 127,385
- Unmarried	-0.0192 (0.0133)	-0.0058 (0.0052)	-0.0012 (0.0062)	92,888 / 118,580
- Excluding parents†	-0.0314* (0.0169)	-0.0028 (0.0066)	-0.0137 (0.0084)	43,284 / 55,933
- Parents†	-0.0032 (0.0087)	-0.0048 (0.0049)	0.0042 (0.0051)	147,214 / 190,032
- Excluding lone parents†	-0.0113 (0.0085)	-0.0052 (0.0040)	-0.0006 (0.0046)	176,854 / 228,109
- Lone parents†	0.0138 (0.0374)	0.0008 (0.0166)	0.0039 (0.0180)	13,644 / 17,856
- With university degree	-0.0190 (0.0121)	-0.0059 (0.0054)	-0.0017 (0.0061)	102,183 / 126,839
- Without university degree	0.0016 (0.0125)	-0.0041 (0.0054)	0.0018 (0.0070)	88,315 / 119,126
- Previous health events (last three years)	-0.0213 (0.0134)	-0.0064 (0.0080)	-0.0027 (0.0092)	91,618

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each coefficient represents the treatment effect from a separate regression. Standard errors in parentheses are clustered at the discrete values of the running variable multiplied by the calendar year. Estimations are based on regression model (1) using a 12-month bandwidth. † Parents with children aged 0-17 years. ‡ The first figure refers to the number of observations for outpatient care, the second one to inpatient care and sick leave. Previous health events only have data as of 2001.

## Appendix

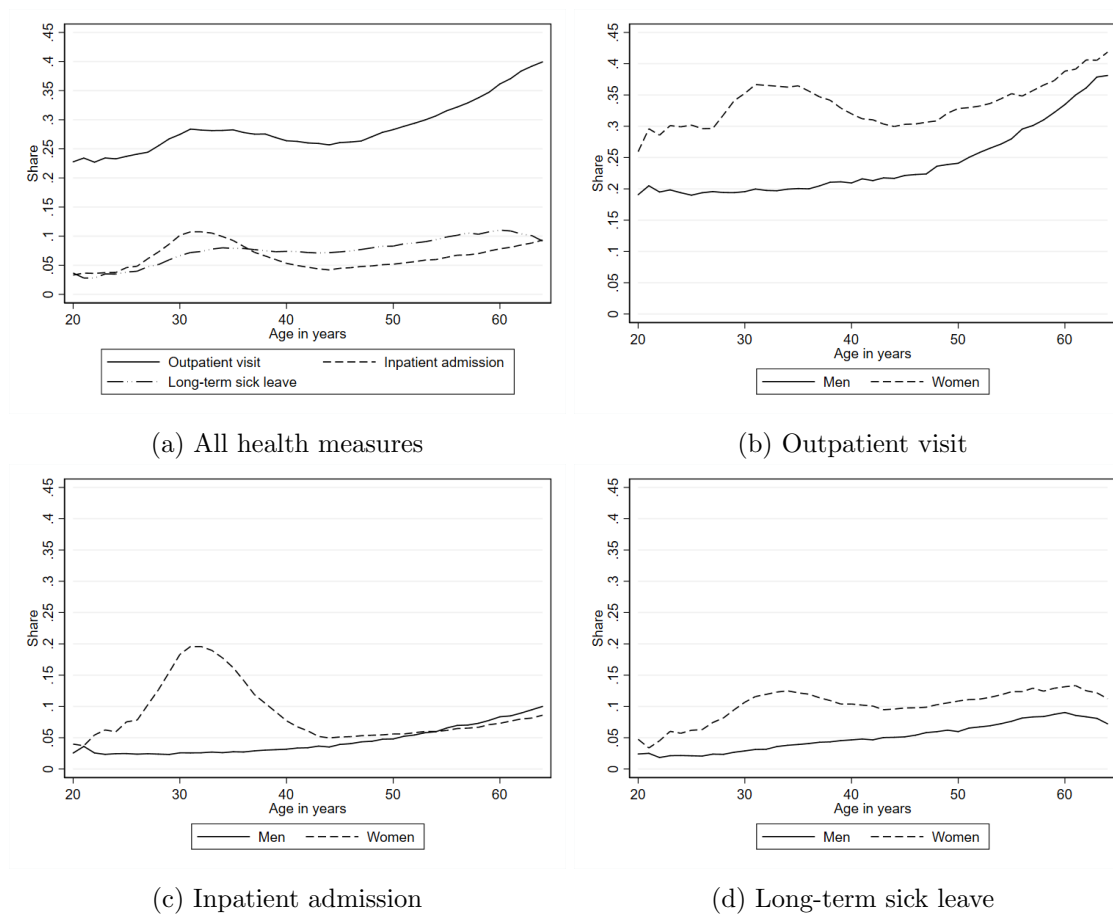
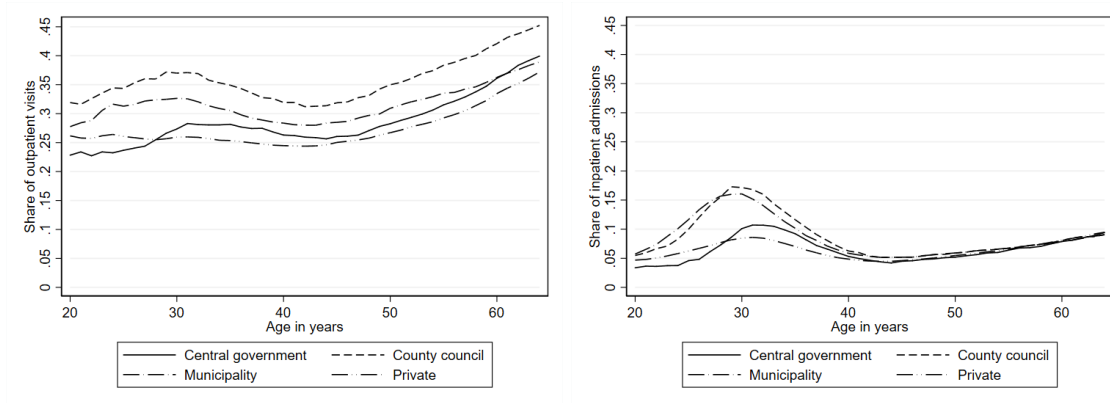


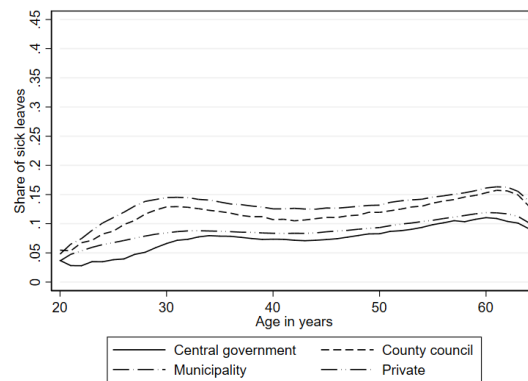
Figure A1: Age-specific share of central government employees with at least one outpatient visit, inpatient admission, or long-term sick leave for ages 20-64 years

Notes: The data are based on the pooled sample for 1997-2011 (2001-2011 for outpatient visits). All central government employees fulfilling the inclusion criteria defined in section 3.1 and without missing values on sex, education, country of birth, civil status, and income are included. The bulge at age 25-45 is due to conditions related to pregnancy and childbirth.



(a) Outpatient visit

(b) Inpatient admission



(c) Long-term sick leave

Figure A2: Age-specific share of employees with at least one outpatient visit, inpatient admission, or long-term sick leave for ages 20-64 years by sector of employment

Notes: 'Central government', 'county council', and 'municipality' are the three tiers of the public sector and 'private' refers to the private sector. The data are based on the pooled sample for 1997-2011 (2001-2011 for outpatient visits). All employees fulfilling the inclusion criteria defined in section 3.1 and without missing values on sex, education, country of birth, civil status, and income are included. The bulge at age 25-45 is due to conditions related to pregnancy and childbirth.

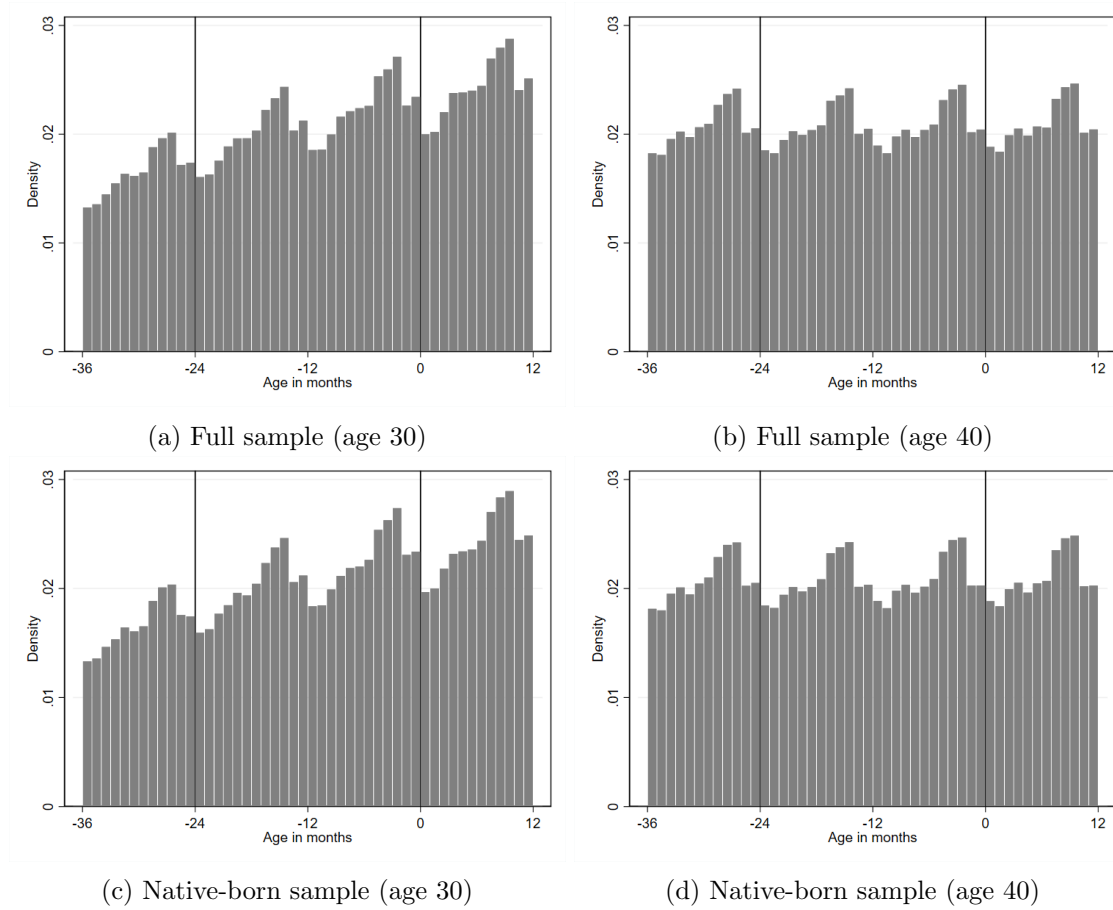


Figure A3: Density of the number of central government employees by age in months  
Notes: The bin size is one month. The zero on the x-axis marks the actual threshold. To the immediate right of it are people born in December in year  $t-30$  or  $t-40$ , and to the immediate left of it people born in January in year  $t-29$  or  $t-39$ . The whole four-year age interval used in the empirical analysis is shown. At age 30 people born between January 1967 and December 1984 are included in the four-year age interval, and at age 40 those born between January 1957 and December 1974.

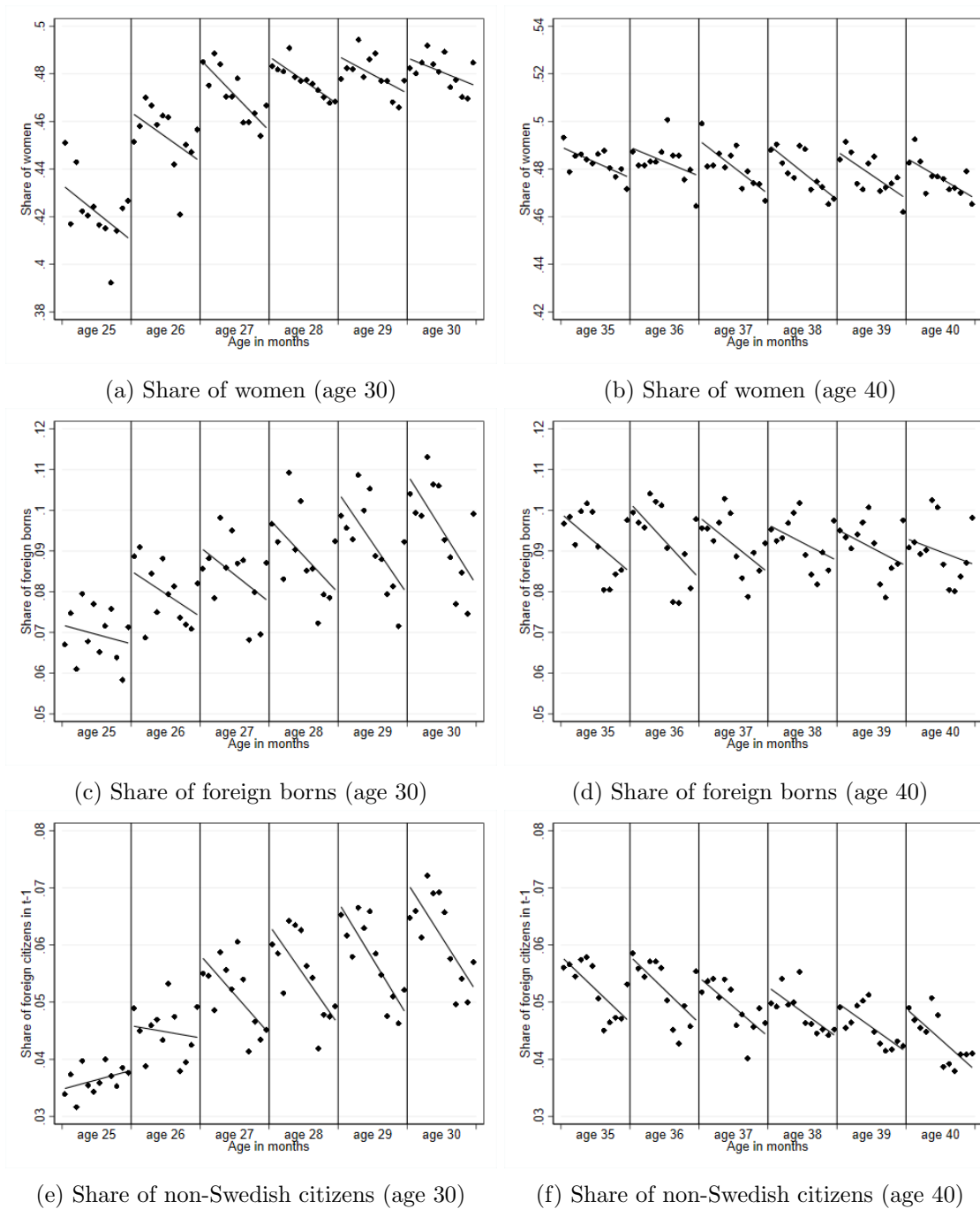
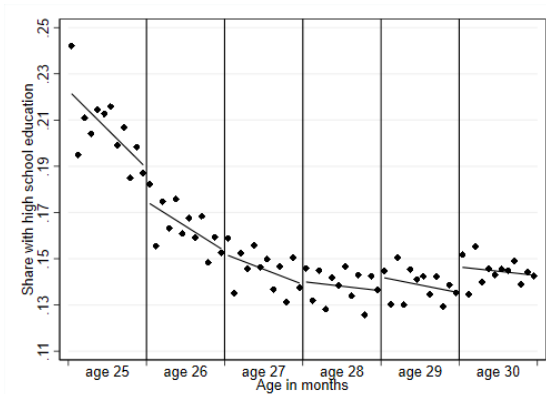
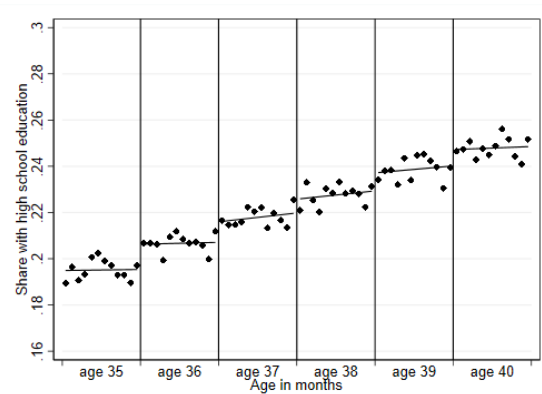


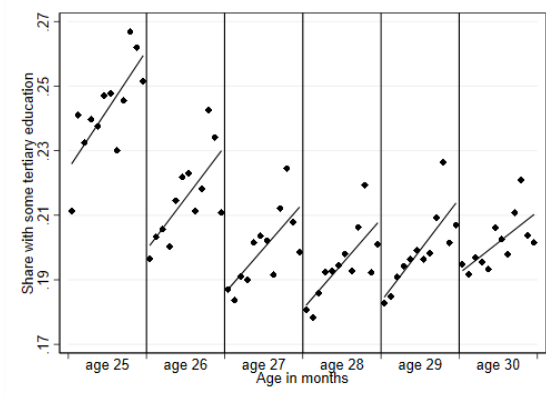
Figure A4: Extended RD-DID plots for baseline covariates (continued on next page)  
 Notes: The bin size is one month. The whole four-year age interval used in the empirical analysis and the two preceding age intervals are shown.



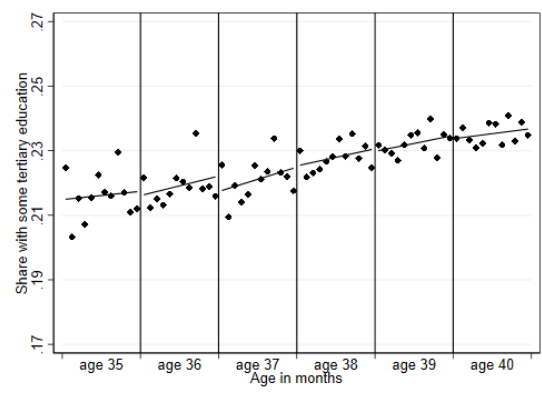
(g) Share with high school education (age 30)



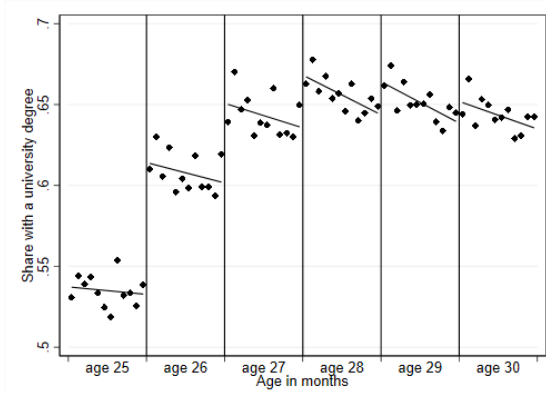
(h) Share with high school education (age 40)



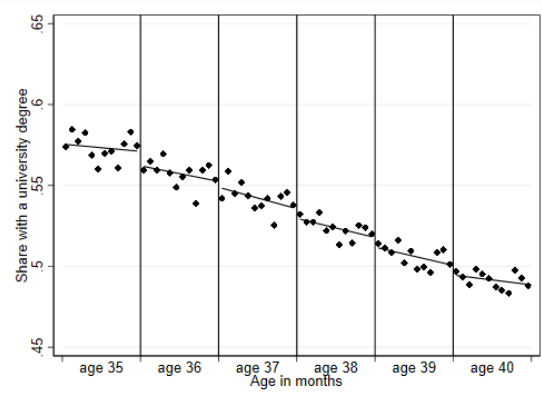
(i) Share with some tertiary education (age 30)



(j) Share with some tertiary education (age 40)



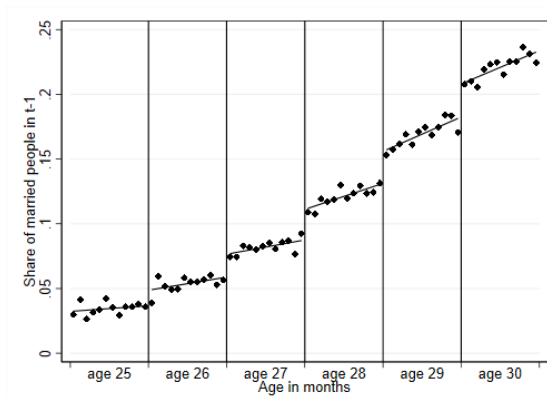
(k) Share with a university degree (age 30)



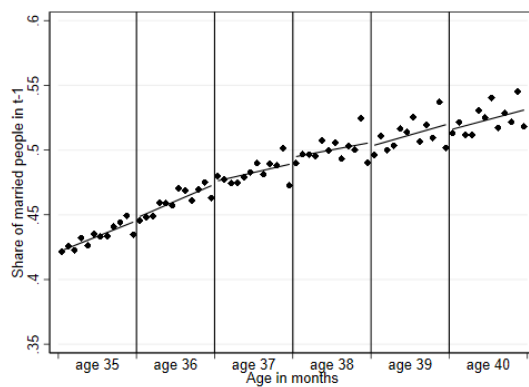
(l) Share with a university degree (age 40)

Figure A4: Extended RD-DID plots for baseline covariates (cont.)

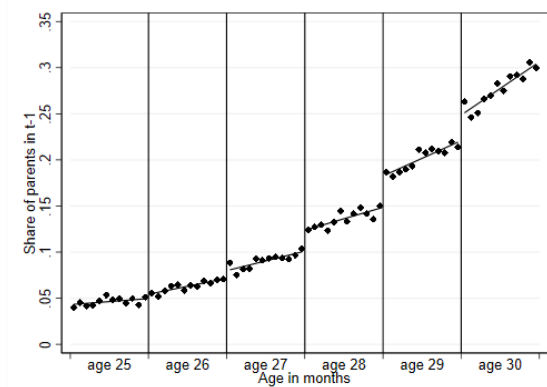
Notes: The bin size is one month. The whole four-year age interval used in the empirical analysis and the two preceding age intervals are shown.



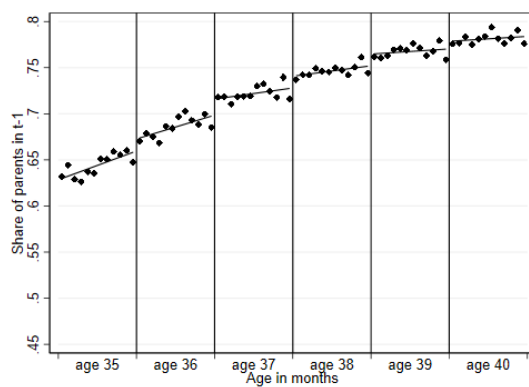
(m) Share of married people (age 30)



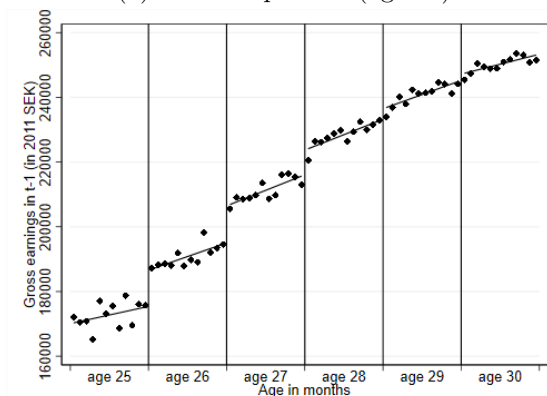
(n) Share of married people (age 40)



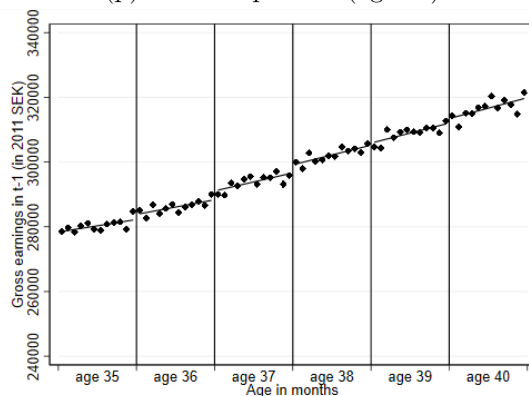
(o) Share of parents (age 30)



(p) Share of parents (age 40)



(q) Gross earnings (age 30)

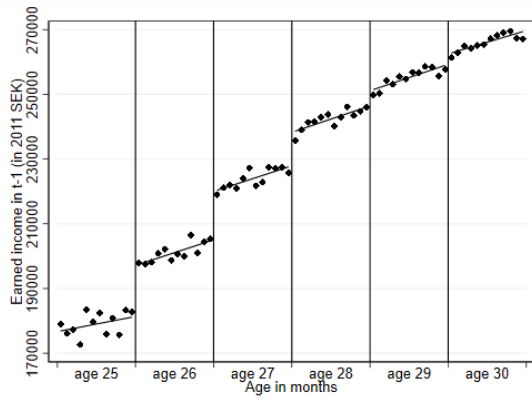


(r) Gross earnings (age 40)

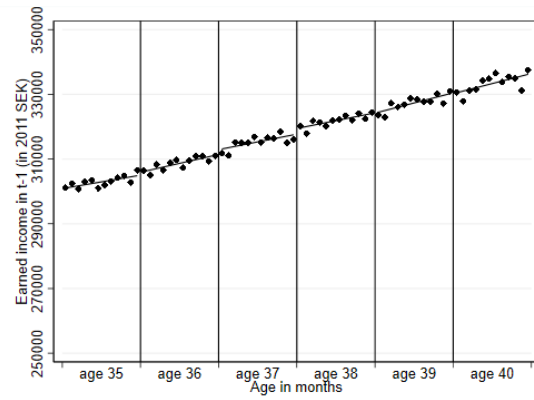
Figure A4: Extended RD-DID plots for baseline covariates (cont.)

Notes: The bin size is one month. The whole four-year age interval used in the empirical analysis and the two preceding age intervals are shown.

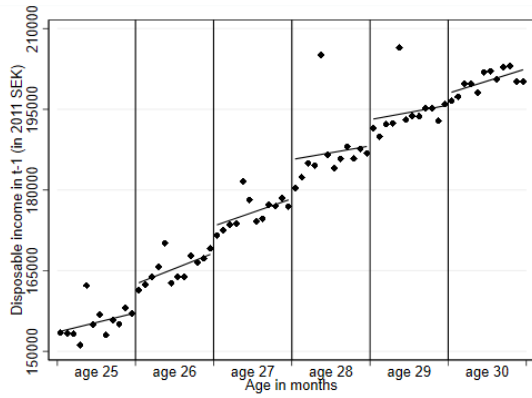




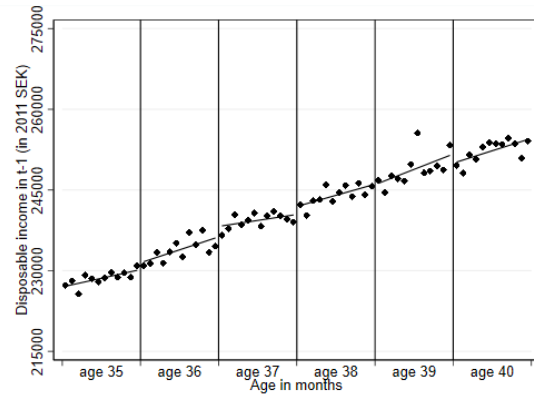
(s) Earned income people (age 30)



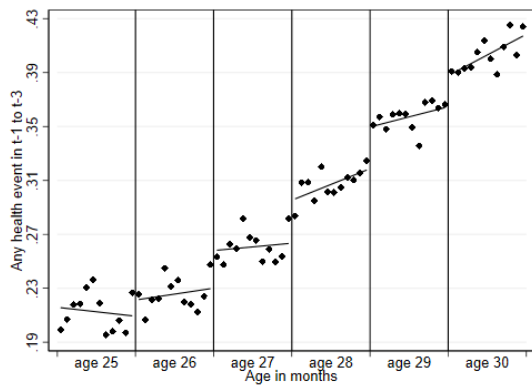
(t) Earned income people (age 40)



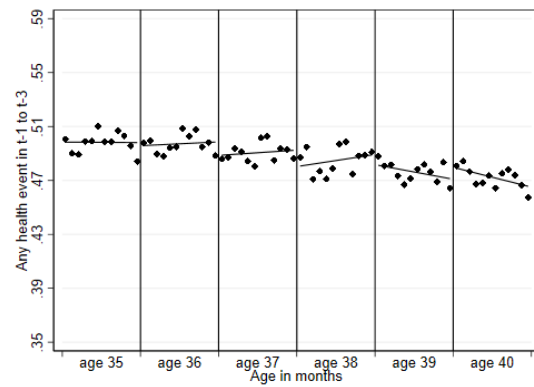
(u) Disposable income (age 30)



(v) Disposable income (age 40)



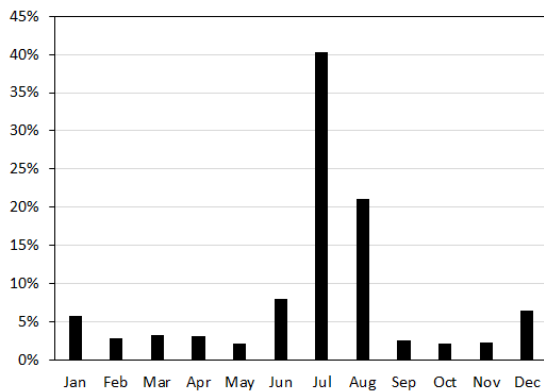
(w) Any health event in t-1 to t-3 (age 30)



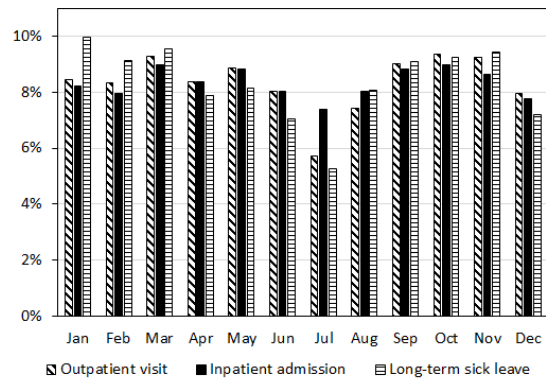
(x) Any health event in t-1 to t-3 (age 40)

Figure A4: Extended RD-DID plots for baseline covariates (cont.)

Notes: The bin size is one month. The whole four-year age interval used in the empirical analysis and the two preceding age intervals are shown.



(a) Vacation



(b) Health

Figure A5: Monthly shares of (a) total annual hours absent from work due to vacation and of (b) health events

Notes: The underlying population are all people in employment aged 25-54 in Sweden. For vacation, the monthly shares are mean shares for the years 2006-2011, based on data from Statistics Sweden's Labor Force Surveys. The exact numbers need to be treated with caution, as the survey respondents are asked about their circumstances in a certain reference week in every month (rather than on the whole month). For health, the monthly shares are mean shares of all outpatient visits, inpatient admissions, and long-term sick leaves, respectively, for the years 2006-2011, based on register data from the SIP database.

Table A1: Ten most common occupational groups, 2001-2011

Rank	Share	Occupational group
<i>Panel A: Age 30</i>		
1	24.28%	College, university, and higher education teaching professionals
2	8.77%	Police officers, inspectors, and detectives
3	7.70%	Armed forces
4	7.32%	Public service administrative professionals
5	5.78%	Customs, tax, and related government associate professionals
6	5.13%	Legal professionals
7	4.93%	Protective services workers
8	4.21%	Administrative associate professionals
9	2.49%	Computing professionals
10	2.28%	Other office clerks
-	1.77%	Missing information
-	25.34%	All other 98 groups
Total	100.00%	n = 135,744

*Panel B: Age 40*

1	11.52%	College, university, and higher education teaching professionals
2	10.82%	Public service administrative professionals
3	9.14%	Police officers, inspectors, and detectives
4	6.48%	Armed forces
5	5.17%	Customs, tax, and related government associate professionals
6	5.02%	Administrative associate professionals
7	4.38%	Computing professionals
8	3.67%	Legal professionals
9	3.47%	Business professionals
10	3.33%	Protective services workers
-	1.27%	Missing information
-	35.73%	All other 99 groups
Total	100.00%	n = 190,498

Notes: The data are based on the pooled sample for 2001-2011, as no information on occupation is available prior to 2001. All observations in the four-year age interval used in the empirical analysis with non-missing values on sex, education, country of birth, civil status, and income are included. The classification of the occupational groups is based on the Swedish Standard Classification of Occupations (SSYK 96) using three digits resolution. SSYK 96 is based on ISCO-88 (COM), the European version of the International Standard Classification of Occupations.

Table A2: Test of the continuity of baseline covariates (additional results)

	(1)	(2)	(3)	(4)
	Mean below	Mean above	RD	RD-DID
<i>Panel A: Age 30</i>				
Female	.479	.480	.013	-.007
Born abroad	.091	.095	.027	.011
Non-Swedish citizen*	.057	.061	.020	.005
No high school education	.011	.010	-.001	-.000
High school education	.139	.145	.015	.007
Some tertiary education	.200	.202	-.014	.004
University graduate	.651	.643	0.001	-.011
Married*	.170	.222	0.025	.003
Any children, aged 0-17*	.203	.279	.030	.004
Gross earnings (in 2011 SEK)*	241,101	250,413	3,337	-6,531
Earned income (in 2011 SEK)*	255,424	266,304	4,488	-7,590
Disposable income (in 2011 SEK)*	194,491	200,368	3,323	-5,445
Any health event, previous year†	.220	.248	.019	-.006
Any health event, two previous years†	.310	.350	.028	-.007
Any health event, three previous years†	.358	.404	.028	-.007
Observations	49,272	53,076	102,348	182,326
<i>Panel B: Age 40</i>				
Female	.477	.476	.016	-.003
Born abroad	.091	.090	.008	-.003
Non-Swedish citizen*	.045	.043	.007	-.001
No high school education	.023	.025	.000	.000
High school education	.239	.248	.011	.001
Some tertiary education	.232	.235	-.003	-.004
University graduate	.506	.492	-.007	.003
Married*	.512	.524	-.006	-.010
Any children, aged 0-17*	.768	.781	.009	-.004
Gross earnings (in 2011 SEK)*	309,061	316,775	1,639	-879
Earned income (in 2011 SEK)*	327,450	333,442	-34	-1,614
Disposable income (in 2011 SEK)*	248,931	252,371	-1,252	-2,745
Any health event, previous year†	.267	.261	.003	.010
Any health event, two previous years†	.392	.389	.009	.019
Any health event, three previous years†	.476	.472	.008	.021
Observations	61,681	61,887	123,568	245,965

Notes: Column 1 and 2 show variable means in the 12-month interval 'below' and 'above' the actual threshold, which are used in the t-test in Table 2. Column 3 (4) shows the coefficient of the treatment indicator in a local linear RD (RD-DID) regression with separate trends using a 12-month bandwidth on either side of the actual (each) threshold; see Table 2 for the corresponding p-values. The regressions include fixed effects for calendar year and government agency and standard errors are clustered at the discrete values of the running variable multiplied by the calendar year. The data are based on the pooled sample for 1997-2011. † There are fewer observations for these variables, as no data for outpatient visits from before 2001 are available. \* These time-varying covariates are measured in year  $t-1$ , i.e., the year prior to treatment.

Table A3: McCrary density test

	(1)	(2)	(3)	(4)
<i>Dependent variable: Number of observations</i>				
<i>Panel A1: Age 30 - Full sample</i>				
Treatment effect	-985.8*** (293.7)	-1026.6*** (277.8)	-393.2 (368.7)	-381.1 (356.5)
R-squared	0.66	0.80	0.86	0.91
<i>Panel A2: Age 30 - Native-born sample</i>				
Treatment effect	-1014.6*** (271.0)	-1061.4*** (271.3)	-407.4 (342.3)	-395.0 (348.7)
R-squared	0.68	0.80	0.85	0.90
<i>Panel B1: Age 40 - Full sample</i>				
Treatment effect	-971.1** (364.2)	-996.0** (418.4)	51.8 (495.9)	5.5 (553.7)
R-squared	0.36	0.52	0.40	0.56
<i>Panel B2: Age 40 - Native-born sample</i>				
Treatment effect	-916.9** (352.1)	-936.6** (409.9)	70.4 (473.4)	35.3 (536.5)
R-squared	0.35	0.50	0.39	0.55
Observations	24	24	48	48
Bandwidth	12m	12m	2x12m	2x12m
Polynomial	NO	YES	NO	YES
Estimation	RD	RD	RD-DID	RD-DID

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each coefficient represents a separate regression. Robust standard errors are in parentheses. The bin size is one month. Estimations are based on a standard RD (RD-DID) design, which allows for separate trends on each side of the threshold(s) and in which the running variable is centered at zero at the threshold(s). A second-order polynomial specification is considered in columns (2) and (4).

Table A4: Robustness checks (age 30)

	Outpatient care	Inpatient care	Sick leave	Observations†
<i>Panel A: Main results</i>				
- Main specification & full sample	-0.0055 (0.0097)	0.0008 (0.0052)	-0.0007 (0.0046)	135,744 / 182,326
<i>Panel B: Model specification</i>				
- Controls included	-0.0052 (0.0096)	0.0013 (0.0052)	-0.0010 (0.0045)	135,744 / 182,326
- Second-order polynomial & controls included	-0.0090 (0.0144)	-0.0016 (0.0081)	-0.0011 (0.0068)	135,744 / 182,326
- Bandwidth = 6 months	-0.0026 (0.0136)	-0.0013 (0.0075)	0.0016 (0.0067)	67,804 / 90,886
- Bandwidth = 1 month	-0.0100 (0.0175)	0.0029 (0.0092)	0.0018 (0.0077)	10,339 / 14,013
- No fixed effects	-0.0080 (0.0097)	-0.0006 (0.0052)	-0.0013 (0.0046)	135,744 / 182,326
<i>Panel C: Sample specification</i>				
- Native-born sample	-0.0020 (0.0105)	0.0014 (0.0054)	-0.0020 (0.0048)	122,198 / 165,911
- Excluding employees with temporary secondary employments	0.0013 (0.0113)	0.0030 (0.0061)	0.0007 (0.0052)	104,011 / 139,593
- Excluding academic staff & managers	-0.0096 (0.0111)	-0.0097 (0.0069)	-0.0046 (0.0061)	100,209 / 100,209
- Years 1997-2006	0.0054 (0.0129)	0.0035 (0.0064)	0.0065 (0.0054)	74,709 / 121,291
<i>Panel D: Outcome specification</i>				
- Health in January-May	-0.0105 (0.0077)	0.0024 (0.0034)	0.0015 (0.0030)	135,744 / 182,326
- Health in June-August	-0.0016 (0.0059)	-0.0019 (0.0027)	-0.0007 (0.0024)	135,744 / 182,326
- Health in September-December	0.0026 (0.0075)	0.0000 (0.0034)	-0.0005 (0.0028)	135,744 / 182,326

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each coefficient represents the treatment effect from a separate regression. Standard errors in parentheses are clustered at the discrete values of the running variable multiplied by the calendar year (i.e., 360 clusters for inpatient care and sick leave, 264 clusters for outpatient care and the occupation-specific subsample, 180 (132 for outpatient care) clusters for the 6-month bandwidth, 240 (144 for outpatient care) clusters for 1997-2006, robust standard errors for the 1-month bandwidth). Estimations are based on regression model (1), and on model (2) for the second-order polynomial version. The controls included are dummies for sex, foreign country of birth, being a parent with children aged 0-17 years, and being married, an ordinal variable for highest completed level of education, and disposable income. Parenthood, marital status, and disposable income are all measured in year  $t-1$ . Observations with missing information on occupation are excluded in the occupation-specific subsamples. † The first figure refers to the number of observations for outpatient care, the second one to inpatient care and sick leave.

Table A5: Robustness checks (age 40)

	Outpatient care	Inpatient care	Sick leave	Observations†
<i>Panel A: Main results</i>				
- Main specification & full sample	-0.0096 (0.0084)	-0.0047 (0.0038)	0.0000 (0.0044)	190,498 / 245,965
<i>Panel B: Model specification</i>				
- Controls included	-0.0090 (0.0083)	-0.0046 (0.0037)	0.0002 (0.0044)	190,498 / 245,965
- Second-order polynomial & controls included	-0.0020 (0.0135)	-0.0084 (0.0058)	-0.0031 (0.0070)	190,498 / 245,965
- Bandwidth = 6 months	-0.0082 (0.0126)	-0.0078 (0.0055)	-0.0049 (0.0066)	95,359 / 123,100
- Bandwidth = 1 month	0.0018 (0.0145)	-0.0064 (0.0072)	0.0010 (0.0077)	14,897 / 19,263
- No fixed effects	-0.0089 (0.0086)	-0.0049 (0.0038)	0.0004 (0.0044)	190,498 / 245,965
<i>Panel C: Sample specification</i>				
- Native-born sample	-0.0084 (0.0086)	-0.0051 (0.0039)	-0.0003 (0.0047)	172,644 / 223,604
- Excluding employees with temporary secondary employments	-0.0121 (0.0092)	-0.0046 (0.0044)	0.0030 (0.0055)	145,179 / 187,779
- Excluding academic staff & managers	-0.0075 (0.0092)	-0.0055 (0.0046)	0.0012 (0.0056)	162,542 / 162,542
- Years 1997-2006	0.0066 (0.0109)	-0.0034 (0.0042)	0.0025 (0.0052)	98,702 / 154,169
<i>Panel D: Outcome specification</i>				
- Health in January-May	-0.0079 (0.0064)	-0.0063** (0.0025)	-0.0010 (0.0033)	190,498 / 245,965
- Health in June-August	-0.0052 (0.0058)	-0.0019 (0.0019)	-0.0014 (0.0022)	190,498 / 245,965
- Health in September-December	-0.0029 (0.0060)	0.0040* (0.0022)	-0.0016 (0.0027)	190,498 / 245,965

Notes: see Table A4.

Table A6: Placebo regressions

	(1)	(2)	(3)
	Outpatient care	Inpatient care	Sick leave
<i>Panel A: Age 29</i>			
Treatment effect	0.0064 (0.0108)	-0.0027 (0.0053)	0.0011 (0.0046)
Observations	116,067	156,869	156,869
R-squared	0.022	0.015	0.015
Pre-extension mean	0.2554	0.0730	0.0513
<i>Panel B: Age 39</i>			
Treatment effect	-0.0013 (0.0069)	0.0057 (0.0044)	0.0001 (0.0043)
Observations	190,127	244,951	244,951
R-squared	0.015	0.008	0.011
Pre-extension mean	0.2755	0.0660	0.0750

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each coefficient represents a separate regression. Standard errors in parentheses are clustered at the discrete values of the running variable multiplied by the calendar year. Estimations are based on regression model (1) using a 12-month bandwidth on either side of each placebo threshold. The pre-extension mean refers to the 12-month period before the actual placebo threshold.



Table A7: Non-health outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Gross earnings	Earned income	Disposable income	Any secondary employment	Marriage	Lone parenthood	Any children, 0-17 years
<i>Panel A: Age 30</i>							
Treatment effect	152.3 (1751.6)	761.3 (1429.9)	-2654.4 (3018.7)	0.0118 (0.0077)	0.0116 (0.0079)	0.0021 (0.0024)	-0.0112 (0.0081)
Observations	182,326	182,326	182,326	182,326	182,326	182,326	182,326
R-squared	0.182	0.199	0.004	0.025	0.028	0.009	0.066
Pre-extension mean	264,668	279,334	213,699	0.2337	0.2220	0.0145	0.2798
<i>Panel B: Age 40</i>							
Treatment effect	-1675.2 (2306.6)	-1563.3 (1961.1)	-454.8 (1805.1)	-0.0002 (0.0074)	-0.0031 (0.0082)	-0.0006 (0.0044)	-0.0110 (0.0070)
Observations	245,965	245,965	245,965	245,965	245,965	245,965	245,965
R-squared	0.152	0.178	0.142	0.033	0.012	0.011	0.017
Pre-extension mean	324,772	341,667	260,496	0.2371	0.5222	0.0764	0.7825

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each coefficient represents a separate regression. Standard errors in parentheses are clustered at the discrete values of the running variable multiplied by the calendar year. Estimations are based on regression model (1) using a 12-month bandwidth and not including any controls. The pre-extension mean refers to the 12-month period before the actual threshold.