The Effect of Paid Vacation on Health: Evidence from Sweden

Hofmarcher, Thomas

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LUND UNIVERSITY

PO Box 117
221 00 Lund
+46 46-222 00 00

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Department of Economics
School of Economics and Management

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Thomas Hofmarcher

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Thomas Hofmarcher*
Department of Economics, Lund University, Sweden

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Abstract

This study estimates the causal effect of receiving additional paid vacation days 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. Identification is achieved 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 no statistically significant changes in health (as proxied by specialized outpatient care visits, inpatient care admissions, and long-term sick leaves) induced by an extension of three paid vacation days at age 30 and four days at age 40. There is no evidence of significant effects by sex, being a (lone) parent, education level, or broad group of diagnoses. These findings challenge the historically grown health argument for additional paid vacation days.

JEL codes: I18, J22, J81, M52
Keywords: vacation, holiday, working time, health

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1 Introduction

Paid vacation and paid annual leave[1] 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.[2] Workers' health represented at best a secondary concern in all of these decisions. This is surprising as it contradicts the historical argument with 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 which emphasized the importance of vacation for workers' physical and mental health (SOU, 2001). Over time, mandated vacation entitlements have become increasingly more generous.[3] In 1993 the Council of the European Union adopted the so-called Working Time Directive (93/104/EC) which required member states to pass a law on, inter alia, 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 paper, I study the effect 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 which 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.

[1] In this study, "paid vacation" denotes the vacation leave to which a worker is entitled to in a year. 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, paid vacation days can be more or less freely distributed across the year by the worker. Whether workers stay at home or travel while on vacation is not considered here.

[2] 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. The Irish government capped paid annual leave for civil servants at 32 days in 2012 which previously 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.

[3] The content of the health argument for paid vacation has changed over time from the so-called protection motive to the recreation motive. The foremost reason for the introduction in Sweden 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).

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 in the calendar year an employee turns 40. The whole vacation entitlement is received in the beginning of each calendar year. This creates two sharp 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, a regression discontinuity (RD) design is combined 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 time-invariant factors, which might be related to health, between two consecutive birth cohorts and yields the true causal effect.

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 analysis. The results indicate that the effects of receiving three (four) additional vacation days at age 30 (40) on all measures of employees' health are small and statistically indistinguishable from zero. I investigate two dimensions of heterogeneity in the effects. There are no significant effects in the subsamples of men, women, (lone) parents, non-(lone) parents, 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 argument which surrounded the introduction and in particular the extension of paid vacation into question when put to the test in a modern context. It also means that recent policy changes aimed at reducing vacation days might have had limited consequences for workers' health.

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 reduce the stipulated annual working hours and endow workers with more leisure time though. Leisure time is usually assumed to be health-enhancing, and there is some empirical evidence that reductions in annual working hours improve health; see, e.g., Hummels et al. (2016). 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 vary with workers' age, family status, parenthood 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.

This paper is related to a rather large empirical literature in psychology and occupational medicine. 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 small but positive gains in health and well-being, as measured by, e.g., subjective life satisfaction, mood, exhaustion, health complaints. Second, these positive effects fade out quickly after returning to work; typically after one to four weeks.[4] 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. There are also several caveats in this type of studies. The usual 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.

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 paper 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. The studied increase in vacation days is 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. Five weeks of paid vacation per year (which translate into 25 work days) have been mandated

[4]This pattern has been dubbed vacation cycle, which consists of a vacation effect (i.e., the boost in well-being, happiness, 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).

by the Annual Leave Act (*semesterlagen*) since 1978. Despite these national laws regulating working time, collective agreements which cover almost the entire workforce govern the Swedish labor market. These agreements cannot undercut the statutory regulations however, 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). Most 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.[5] Regarding vacation entitlement, the ALFA stipulates the following age-based rule.[6] Until the year an employee turns 29, she receives 28 days of paid vacation 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 which lasts until retirement.[7] 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 sharp discontinuities; one at age 30 and one at age 40. For instance, in the former case people born on December 31st in calendar year $t-30$ receive three additional vacation days in year t, whereas people born on January 1st in calendar year $t-29$ just miss out on those days in year t. Also, the standard weekly working time stipulated by the ALFA has been 39 hours and 45 minutes ever since 1997. Thus, the only change to employees' regular working time in the period after 1997 is induced by the age-based change in the vacation entitlement.

Several additional provisions in the ALFA and the Annual Leave Act are noteworthy. First, to receive and make use of the full vacation entitlement an employee must be employed year-round.[8] Second, vacation days cannot be paid out in money.[9] Third, the

[5]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, 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.

[6]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.

[7]Note that vacation days correspond to work days in the standard five-day workweek stipulated by the ALFA. A 40-year-old employee receives thus exactly seven weeks of paid vacation per year.

[8]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.

[9]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

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.[10] 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.[11]

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 which 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, the analysis is based on pooled data from 1997 to 2011.

Central government employees[12] covered by the ALFA are identified via the Register-based Labor Market Statistics.[13] 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

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.

[10]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.

[11]For instance, provided a gross monthly salary of SEK 30,000 (USD 3750), the vacation pay is comprised of the ordinary salary per work day of about SEK 1350 (USD 169) (assuming 22 work days per month) and the supplement of SEK 132 (USD 17) (= 0.44% * SEK 30,000). Three (four) additional vacation days at age 30 (40) would thus increase annual earnings by a mere 0.11% (0.15%). In comparison, annual working time decreases by 1.35% (1.83%) at age 30 (40) given 250 work days per year and provided that the full vacation entitlement is used.

[12]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.

[13]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.

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.[14] 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 carry over 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. These employees might, however, 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' 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 later years). An admission to inpatient care (i.e., a hospitalization) entails almost always one or more overnight stays at a hospital. But 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).[15] Specialized outpatient care visits 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 main diagnosis (coded according to the tenth edition of the International Statistical Classification of Diseases and Related Health Problems (ICD-10)[16]) 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.

[14] 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).

[15] It is not possible to distinguish between acute and planned admissions in the data.

[16] 12.3% of all cases in the register in 1997 are classified based on ICD-9 and have been recoded accordingly. In all subsequent years more than 99.9% of all cases have a valid ICD-10 code.

Since 1992 sickness benefits are only paid out by the Social Insurance Agency starting after a sickness period of 14 calendar days[17] and hence recorded in the database.[18] 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 1 illustrates the age-specific means of the three measures for the considered sample of central government employees. Based on the main diagnosis for the health care contact or the long-term sick leave, I also break down the three health dummies into twenty broad groups of diagnoses, according to all "chapters" of the ICD-10 classification.

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 have been on long-term sick leave. This means that the analysis might 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.[19] The financial disincentives for selection into health care are thus small. Also, the use of (long-term) sick leave as a measure for health is not perfect, as it can be influenced by other 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. 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 the case of illness. Owing to the

[17]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.

[18]The first day of a sickness period is not remunerated. From day 2 to 14 the employer is obliged to pay sick pay set to 80% of the ordinary salary. The sickness benefits paid out by the Social Insurance Agency from day 15 onwards 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. To address the potential impact of this policy change, I only consider the period 1997-2006 in a robustness check.

[19]Every visit to outpatient care and every day spent at a hospital is subject to a patient fee (about SEK 200 (USD 25) and SEK 100 (USD 13), respectively), but a ceiling limits the total amount a patient has to pay during a 12-month period.

fact that a medical certificate is required, it is reasonable to treat the type of sick leave considered in this study as a measure of health.

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. Central government employees are highly educated, although the higher share of university graduates around age 30 than 40 is a product of a sizable number of PhD students being employed at that age. A larger share of people are married, have children, and are lone parents around age 40 than 30. Almost 10 percent are born outside Sweden, but only 5 percent do not possess Swedish citizenship (which is a requirement for employment in certain areas of the central government, such as the police, the armed forces, and in the judicial system). Table 2 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.

4 Empirical strategy

The fundamental challenge with estimating the causal impact of paid vacation on health is the non-random assignment of different vacation entitlements to workers. In a "naive" regression of the number of vacation days on health, the estimated effect on health 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 sharp 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 get treated with additional vacation days during that year.

4.1 Insufficiency of an RD design

There are certain complications in the Swedish context that pose a threat to the two key identifying assumptions in a standard RD design. First, all predetermined and observable covariates which influence employees' health should be continuous across the threshold. 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 schooling at the same time, and it is the norm that students who reach school leaving age finish the class 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 a relative age effect (i.e., a maturity difference) that affects students who are in the same class.[20] I formally check the continuity assumption for covariates in Table 3 by means of (i) a comparison of mean values in a 12-month interval on each side of the threshold and (ii) an RD regression. The results point indeed to significant discontinuities for certain covariates linked to health, such as sex, education, marriage, parenthood, and income.

Second, 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.[21] Figure 2 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[22] and fewer are born in December than in January in the four-year age interval shown around ages 30 and 40.[23] The McCrary (2008) density test in Panel A in Table 4 picks up this seasonal pattern and indicates a significant jump at the threshold.[24] It should be noted

[20] 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.

[21] 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.

[22] 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 6.

[23] Panel (a) in Figure 2 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 sizable 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 only including data from 1997 to 2006.

[24] A related concern is that central government employees who were born outside Sweden (and outside developed countries in particular) lack reliable information on date of birth; see, e.g., Torun and Tumen

though that the seasonal birth pattern can also be observed among the entire native-born population.

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[25] to identify the causal effect. 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). In doing so, time-invariant factors between two consecutive birth cohorts (such as all issues related to schooling and the seasonal birth pattern discussed above) are canceled out. The identifying assumption that ensures that the effect of vacation on health can be interpreted as causal is then that any covariate that affects health is either continuous across the threshold (as in the standard RD design) or its discontinuity is constant between consecutive birth cohorts (as 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.

Ordinary least squares (OLS) estimates of the following regression model provide the causal effect of paid vacation on health:

$$Health_{ijt} = \alpha_0 + \alpha_1 age^*_{ijt} + T_{ijt}(\beta_0 + \beta_1 age^*_{ijt}) + C_{ijt}[\gamma_0 + \gamma_1 age^*_{ijt} + T_{ijt}(\delta_0 + \delta_1 age^*_{ijt})] + \zeta_j + \eta_t + \epsilon_{ijt}, \quad (1)$$

where regressions are run separately at ages 30 and 40 on 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 get "treated" with additional vacation days. The normalized running variable, denoted $age^*_{ijt} = age_{ijt} - age_c$, is measured as age in months[26], 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 to zero at the counterfactual threshold, which is set to two years before the actual threshold. Given a maximum bandwidth of 24 months around each threshold, $age^*_{ijt} = [-11.5, ..., +11.5]$, 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[27] indicating whether treatment with

(2016). Those employees are excluded in panels (c) and (d) in Figure 2, 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 Panel B in Table 4. In a robustness check, I nevertheless exclude foreign-born employees.

[25] This kind of estimation strategy has been previously used by, e.g., Lalive (2008); Leonardi and Pica (2013); Grembi et al. (2016).

[26] This is the finest granulation of age available in the data.

[27] The treatment indicator is defined as

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

additional vacation days occurred in year t. C_{ijt} is another dummy variable[28] indicating whether an individual is located around the actual or the counterfactual threshold. The RD-DID estimator is δ_0, the interaction of both dummies. Figure 3 visualizes the empirical strategy.

Regression model (1) includes fixed effects for the government agency (ζ_j) and the calendar year (η_t). The default setup is a local linear regression allowing for different trends on each side of the two thresholds and using the maximum bandwidth of 24 months around each threshold. As a robustness check, I implement a more flexible functional specification allowing for quadratic trends.[29] I also consider smaller bandwidths of 12 months and 2 months[30] around 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). In order to reach a sufficient number of clusters in the estimations, 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 24-month bandwidth.

5 Validity of the RD-DID design

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 can be checked. Table 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 age 30[31] but disposable income, which is relevant for investing into ones health, is continuous. The implicit parallel trend assumption for baseline covariates can also be assessed visually at the two thresholds preceding the actual threshold which separate different birth cohorts (those turning 27 from those turning 28, and those turning 28 from those turning 29). The size of the discontinuities should be rather similar. Figure 4 shows indeed that this is the case for sex, education, and disposable income at ages 30 and 40. Furthermore, the modified McCrary (2008) density

[28] The counterfactual indicator is defined as

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

[29] The estimated regression model with quadratic trends on each side of the thresholds (i.e., d=2) is

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

Higher dimensional trends are not considered, following the recommendation by Gelman and Imbens (2017).

[30] The minimum 2-month bandwidth corresponds to a comparison of means between employees born in December in a certain year with employees born in January in the following year.

[31] This could stem from the fact that the earnings development is more erratic at a young age.

test no longer detects a significant jump in the number of observations at the actual threshold; see Table 4. As a result, the main assumptions for an RD-DID design to be valid seem to be fulfilled.

As mentioned above, Swedish register data do not contain information on 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. In terms of interpretation of the ITT estimates, the question is whether additional vacation days induce a corresponding jump in the days taken (i.e., whether a first stage exists). First of all, the fact that vacation days cannot be paid out in money means that it is likely that they are all taken. The restrictions on the minimum number of days to be taken every year and the maximum number of days that can be saved, see section 2, should also induce workers to take all days. There is only some suggestive evidence on the aggregate level on whether workers in Sweden forgo part of their vacation entitlement. It turns out that they take all vacation days that they are entitled to (Expedia, 2014). Hence, the first stage should be strong.

There might however be certain cases which comply imperfectly with the "treatment". In Sweden, all public universities and university colleges are run as government agencies and their employees belong therefore to the central government sector. Even though academic staff and PhD students on paper might take all their vacation days every year, it does not mean that they are actually used and that an increase in the vacation entitlement leads to fewer days worked. The same is probably true for high-level staff in other government agencies. It is also possible that employees take up a temporary secondary employment to spend their time on during vacation. I address these cases of 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.[32] 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 birthday effect is lacking for Sweden, but results from, e.g., Germany indicate no such effect (Kühne et al.,

[32]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 38) to SEK 150 (USD 19). It is doubtful that this reduction leads to swift changes in dental health that in turn affect overall health and would thus be picked up by the health measures used in this study. In a robustness check, I nevertheless only consider the period from 1997 to 2006. 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 19)). This routine started in a small part of Sweden in 1986 and has 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, if the test results come back positive and then entail inpatient care admissions and/or visits to specialized outpatient care for treatment. 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.

2015).

6 Results

6.1 Main outcomes

Table 5 presents the main estimates of the effect of receiving additional paid vacation days on employees' health. As the three health measures are dummy variables and the estimation is carried out with OLS, a linear probability model is effectively estimated. 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. Figure 5 depicts the main results graphically.

The results in Panels A and B in Table 5 indicate no statistically significant effects at ages 30 and 40 for any health measure. The point estimate for specialized outpatient care visits at age 30 suggests a 0.55 percentage points (pp) lower probability (corresponding to a 2% decrease, given a baseline mean probability of about 26.2%) of making such 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 (corresponding to a 4% decrease, given a baseline mean probability of about 27.1%). The 95% confidence interval around the two point estimates excludes decreases larger than 10% of the baseline mean probabilities. The point estimates for inpatient care admissions correspond to a 1% increase at age 30 and a 7% reduction at age 40 in the probability of being admitted relative to the baseline mean at the respective ages. The point estimates for long-term sick leaves correspond to a 1% reduction at age 30 and no change (0%) at age 40.

In general, the standard errors of all estimates are rather large but not sensitive to the way of clustering.[33] For specialized outpatient care, which has more variation than the two other health measures, the standard errors are distinctly smaller. However, as the estimated effect sizes are so small (they would be even smaller once standardized by the number of additional vacation days received), they are statistically indistinguishable from zero. Overall, there is no evidence that paid vacation affects health.

6.2 Results by diagnosis group

Table 6 reports estimates of regression model (1) for the ten largest groups of diagnoses instead of looking at all cases together.[34] For specialized outpatient care there are no

[33]Only when standard errors are clustered at the 24 distinct values of the running variable they decrease markedly, yet only the point estimate for specialized outpatient care at age 40 is rendered significant at the 10% significance level.

[34]Results for the ten smallest groups are overwhelmingly insignificant (results available upon request).

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 (corresponding to an 11% decrease, significant at the 10% significance level) of a visit due to factors influencing health status and contact with health services (ICD-10 code Z00-Z99). For inpatient care admission there are neither any significant results. Similarly, for long-term sick leave there are only two significant estimates (both at the 10% significance level) at age 30 of a 0.07 pp lower probability of a sick leave due to neoplasms (C00-D48) and a 0.12 pp 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.[35]

6.3 Results by subgroup

Table 7 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 1), the sex-specific results indicate no significant effect of receiving additional vacation days. Other subgroups considered are employees with and without any children aged 0-17 years, as well as lone parents and the full sample excluding lone parents. The results indicate no significant effect, except for a 3.14 pp lower probability (corresponding to an 11% decrease, significant at the 10% significance level) of making a visit to specialized outpatient care 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 at least once having been admitted to inpatient care or been on long-term sick leave in the three years prior to the year of treatment.

6.4 Robustness

I perform a battery of tests to check the robustness of the results; see Table 8. To start with, controls for sex, highest completed level of education, country of birth, being a parent with children aged 0-17 years, being married, and disposable income (measured in year $t-1$) are added 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)

[35]One threat to the RD-DID design is the start of the breast cancer screening program for women at age 40. However, for specialized outpatient care 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, or the diagnosis group C50 which encompasses breast cancer treatment. For inpatient care admission 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.

covariates influencing health should be continuous across the threshold. Different functional specifications and bandwidths are also tested. Since there is not necessarily a linear trend in the outcomes, a quadratic specification according to regression model (2) is implemented and controls added. The results remain statistically insignificant. Local linear estimates without controls relying on a smaller bandwidth of 12 months (i.e., 6 months on each side of the thresholds) indicate no significant effects. The same is true for the minimum bandwidth of 2 months. The exclusion of the two fixed effects for government agency and calendar year leaves the estimates insignificant.

I consider next whether potential non-compliers with the treatment drive the main results. In one specification only native-born employees are included, due to concerns about the accuracy of the birth dates of those born outside Sweden. These concerns seem to unfounded as the results in Table 8 do not change in any noteworthy way compared to the full sample. Another issue are employees with temporary secondary employments who might use these employments to work during vacation from their main job. Their exclusion leaves the estimates insignificant; see Table 8. Next, the occupational groups of college, university, and higher education teaching professionals as well as legislators, senior officials, and managers are excluded, as they may not comply with the provisions on vacation entitlement in general and hence are not expected to be affected by the change in the entitlement. The estimates remain insignificant; see Table 8. The final sample considered is the full sample for the years 1997 to 2006. This is done for four reasons. First, a rule change came into force in the beginning of 2007 that allowed a certain group of central government employees to negotiate individual terms on their vacation entitlement; see section 2. Second, in 2008 the maximum sick leave length paid for by the Social Insurance Agency was reduced; see section 3. Third, the coverage of cohorts born after 1980 in the SIP database is incomplete which could influence the results at age 30, but for the period 1997 to 2006 the coverage is complete; see section 4. Fourth, in 2008 an earmarked lump sum for dental care services was introduced that changes in size at age 30; see section 5. The results in Table 8 indicate no significant effect for any health measure, in line with the results for the full sample.

In the analysis above the health outcomes have always been measured during the entire year in which treatment with additional vacation days occurs. Panel (a) in Figure 6 shows that most vacation days are taken during June to August among the Swedish working population.[36] Provided that central government employees behave similarly and use the additional vacation days to prolong a vacation during the summer, an effect on health might be more concentrated during these months or the months thereafter. Panel (b) in Figure 6 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

[36]The concentration of vacation taken during the summer is due to a provision in the Annual Leave Act which gives every worker the right to at least four weeks of uninterrupted vacation leave during June to August.

between the timing of vacation and health events could be spurious, as (i) the health care personnel themselves are on vacation and hence fewer non-acute surgeries, examinations, and check-ups are scheduled, and (ii) many workers are vacationing abroad and hence do not use the Swedish health care system. Estimations in which the health outcomes are only measured during the months January to May, June to August, and September to December in year t are shown in Table 8. The results are insignificant, except for a 0.69 pp lower probability (corresponding to a 21% decrease, significant at the 5% significance 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 (corresponding to an 18% increase, significant at the 10% significance level) in September to December.

As a final robustness check, I run a set of placebo regressions. To this end, the actual and counterfactual thresholds are moved one year backwards. The running variable (age^*_{ijt}), the treatment indicator (T_{ijt}), and the counterfactual indicator (C_{ijt}) in model (1) are reprogrammed accordingly. Thus, instead of comparing employees turning 29 (39) to those turning 30 (40), those turning 28 (38) are 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 these thresholds. Therefore, there is no obvious reason for employees' health to change at these placebo thresholds. Table 9 shows results of the placebo regressions conducted for the main outcomes reported in Table 5. There are no significant effects for any of the three measures of employees' health. Placebo regressions have also been run on all diagnosis groups and subgroups considered above. The estimation results are overwhelmingly insignificant (results available upon request). In sum, the insignificant results from the placebo regressions lend strong support to the validity of the main results.

6.5 Non-health outcomes

There is a range of non-health outcomes that could also 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, it is 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 10 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, as well as disposable income. Another outcome is whether the probability of having at least one temporary secondary employment among central government employees changes as they receive more vacation days. There is no support for this in the data. In a similar way, the probability of getting married, becoming a lone parent, or having children does not change significantly at the thresholds; see Table 10.

7 Discussion

The analysis above provides ITT estimates, as it is not possible to observe how many vacation days an employee uses every year. The external validity of the findings, in terms of measuring the effectiveness of the age-based rule, is high though, as employees who receive an increase in vacation days might react in different ways; they might use them, save them for later, not use them at all, formally use them but then still work from home, work more during vacation in a secondary employment, or even take up a temporary secondary employment during their vacation. Possible explanations for the small and statistically insignificant ITT effects of additional vacation days on employees' health are discussed below along with certain limitations of the analysis.

There is some support in the previous literature for the findings in this study. The empirical literature in psychology and occupational medicine emphasizes the positive yet small and short-lived effects of a vacation (irrespective of its length) on health and well-being. If the effects are indeed only transitory and disappear 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 make a big difference. The size of the change in the number of vacation days compared to the baseline entitlement is arguably also important, i.e., where on the intensive margin the effect is identified. Adding three days on top of 28 days or four days on top of 31 days constitute comparatively modest changes which might be below employees' threshold of perception. 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.

In terms of mechanisms relating to the production of individual health in this study setting, any effect on health may only run through the time endowment channel, as additional paid vacation days leave annual earnings unchanged (the latter is support by the results in Table 10). 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 5, the first stage (i.e., whether additional vacation days are used) 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 the US and Canada empirical evidence does not support the existence of such a workload effect, as annual working hours decrease almost proportionally for every week of vacation actually used (Altonji and Usui, 2007; Fakih, 2014).

The context in which the results are obtained might be important. Swedish workers have, according to the OECD[37], a very good work-life balance, even in comparison to other wealthy countries. Apart from generous vacation entitlements, it is possible and

[37] `http://www.oecdbetterlifeindex.org/topics/work-life-balance/` (accessed January 30, 2017)

fairly common to work anything between 90% to 20% of full-time in Sweden.[38] In principal 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 ones preferences. The work hours constraint which has been previously linked to health (see, e.g., Bell et al. (2012)) might thus be barely affected by the receipt of additional vacation days. Furthermore, 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.

The empirical design and the data used in this study have some limitations. First, in RD designs based on "discontinuities in age with inevitable treatment" the notion of randomness of treatment is different; see Lee and Lemieux (2010). 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.

Second, owing to the age-activated treatment, only short-term effects on health can be identified. In this study there is a one-year time window to detect any effects, as employees in the control group switch after one year to the treatment group and those who had previously been in the treatment group get treated for a second time. Even if there truly is an effect of additional vacation days on health, if the effect takes longer than one year to materialize, it cannot be picked up by the RD-DID estimator. However, in the light of the findings of short-lived effects in the aforementioned literature in psychology and occupational medicine, there is no obvious reason to subscribe to this notion. It also means that it seems reasonable to estimate the health effects of paid vacation in the same year as the increase in the vacation entitlement occurs.

Third, the way health is measured is 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. The measures of health care utilization and sick leave considered in this study are however relevant in terms of economic evaluation of changes in the number of paid vacation days.

[38] Part-time workers cannot be identified in the SIP database.

8 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. This study attempts to provide causal evidence on the effect of an increase in the number of paid vacation days (i.e., a change at the intensive margin) on workers' health in a modern context. 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 are obtained among a sample of all central government employees in Sweden. The measurement of the effects on health is limited to the same year as the initial increase in the number of paid vacation days. The absence of significant effects during this period does not necessarily preclude the existence of longer-term effects. This study cannot answer the question on long-term effects and neither whether there are effects on the extensive margin of receiving paid vacation. Nevertheless, the findings of this study challenge the validity of the health argument for more paid vacation days among younger workers when put to the test in a modern context.

The results have also policy implications. Firstly, granting workers additional paid vacation days, if they already have a relatively generous vacation entitlement, is perhaps not the best policy instrument 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 workers who are more productive and have fewer sick leaves might be very limited.

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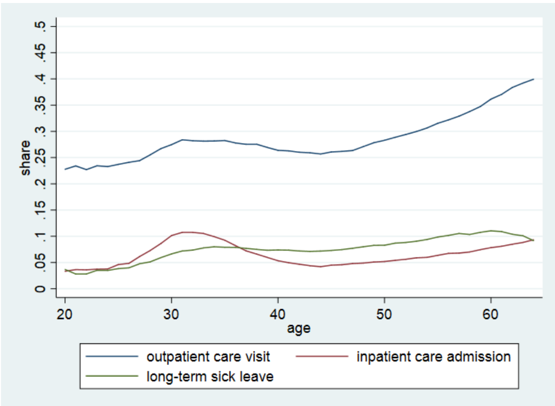
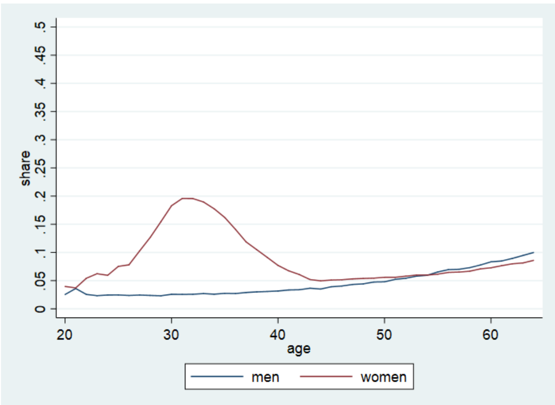
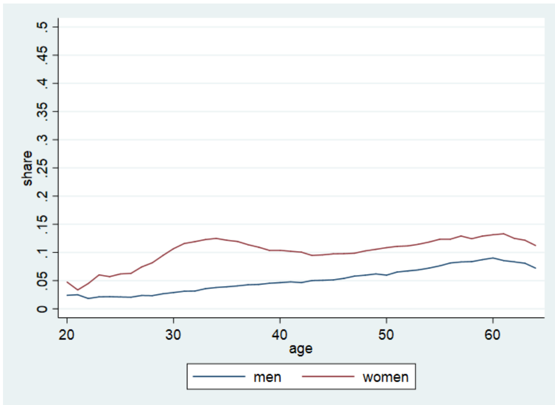


(a) All health measures

(b) Specialized outpatient care visit



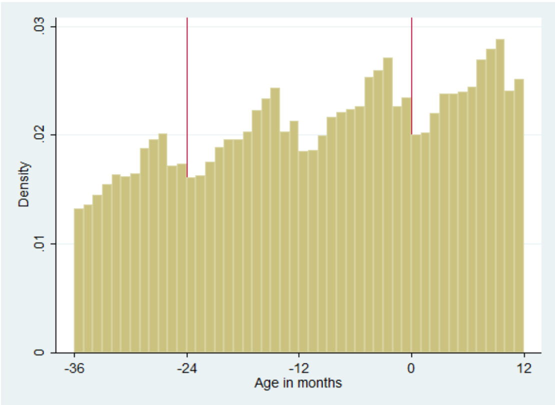
(c) Inpatient care admission



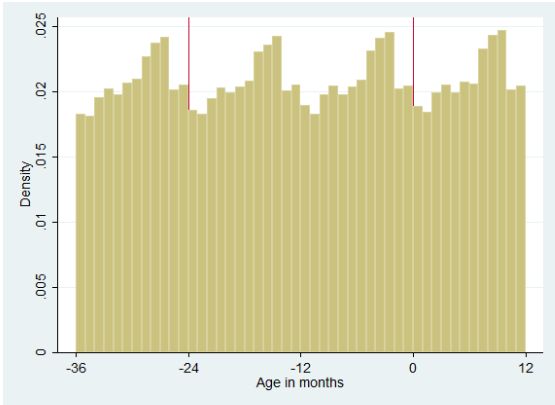
(d) Long-term sick leave

Figure 1: Age-specific share of central government employees with at least one outpatient care visit, inpatient care admission, or long-term sick leave for ages 20-64 years

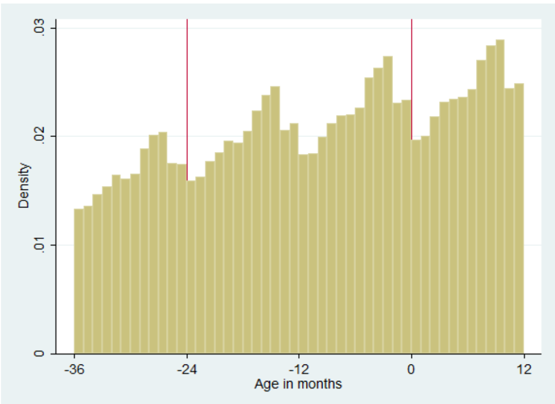
Notes: The data are based on the pooled sample for 1997-2011 (2001-2011 for outpatient care visits). All central government employees fulfilling the inclusion criteria defined in section 3.1 and without missing values on sex, education, country of birth, civil status, and income are included. The bulge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



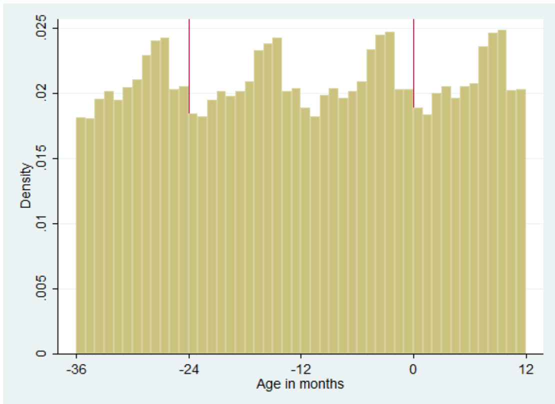
(a) Full sample (age 30)



(b) Full sample (age 40)



(c) Native-born sample (age 30)



(d) Native-born sample (age 40)

Figure 2: Density of the number of central government employees by their age in months
Notes: The bin size corresponds to 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.

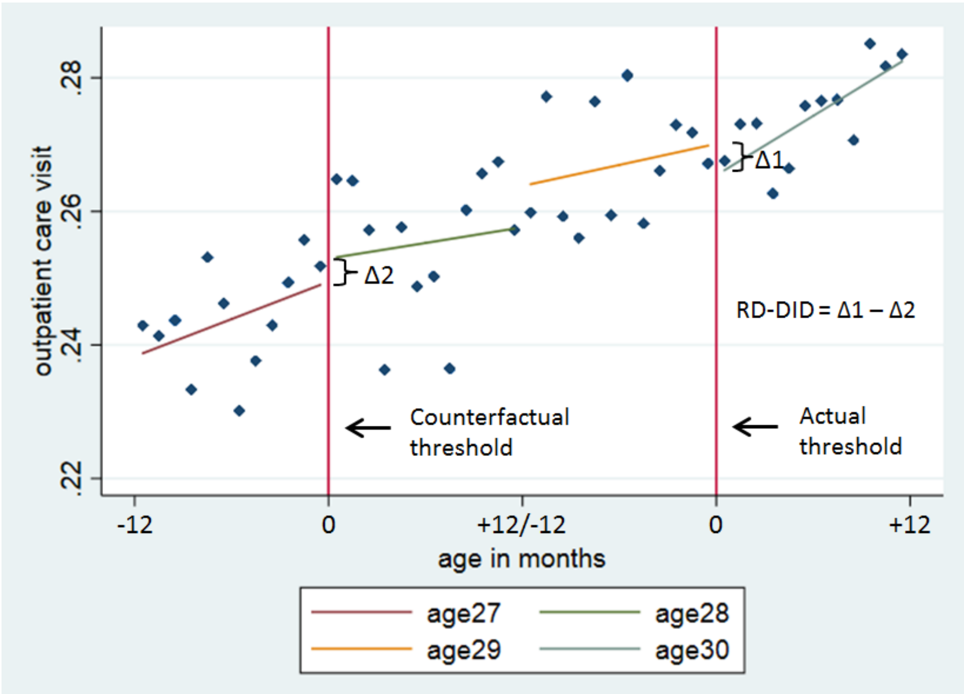
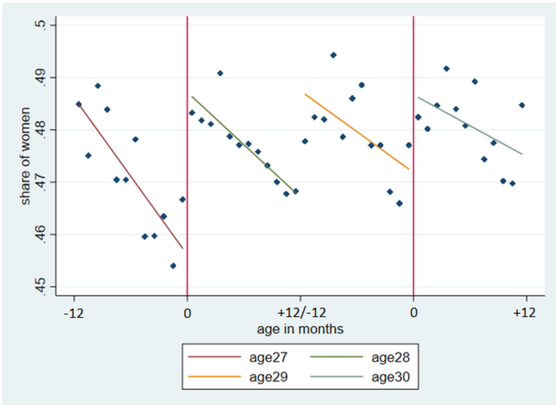
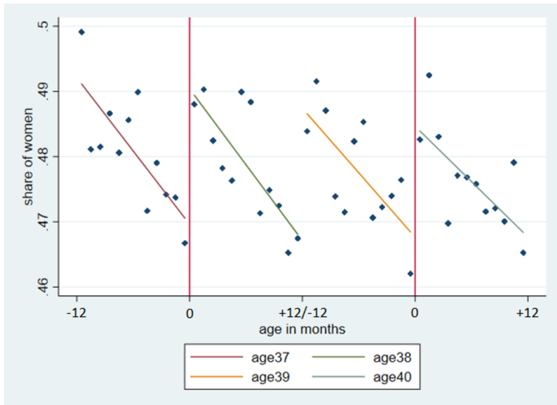


Figure 3: Visualization of the RD-DID design at age 30

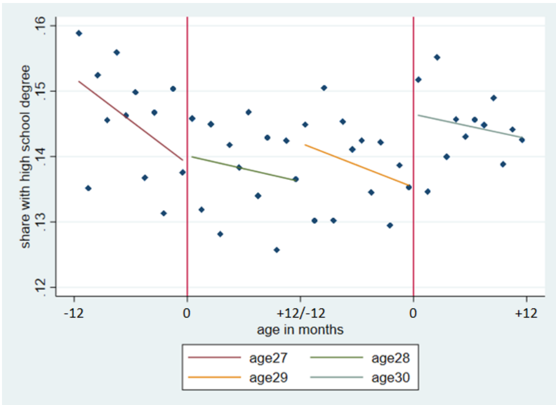
Notes: The RD-DID estimator takes the vertical distance between the fitted lines at the left (counterfactual) threshold and subtracts it from the vertical distance between the fitted lines at the right (actual) threshold. The resulting difference is the RD-DID estimator, δ_0, in regression model (1). The bin size corresponds to one month.



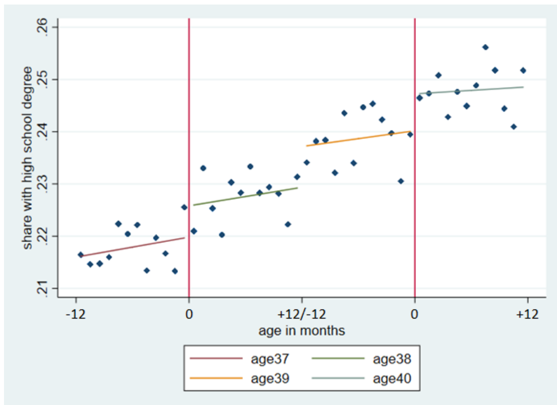
(a) Share of women (age 30)



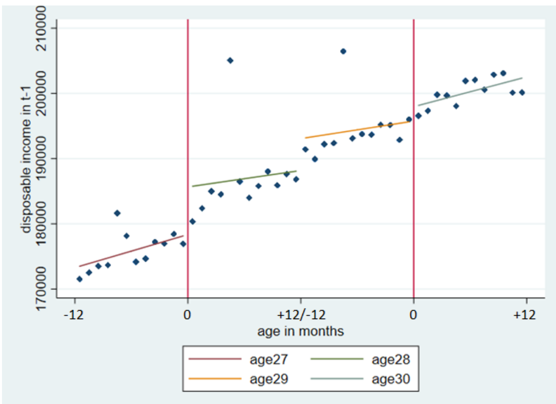
(b) Share of women (age 40)



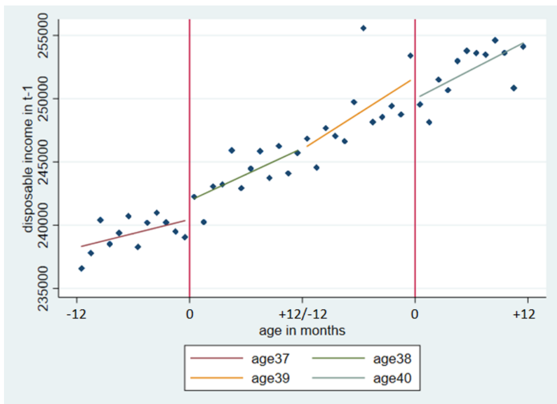
(c) Share of high school graduates (age 30)



(d) Share of high school graduates (age 40)

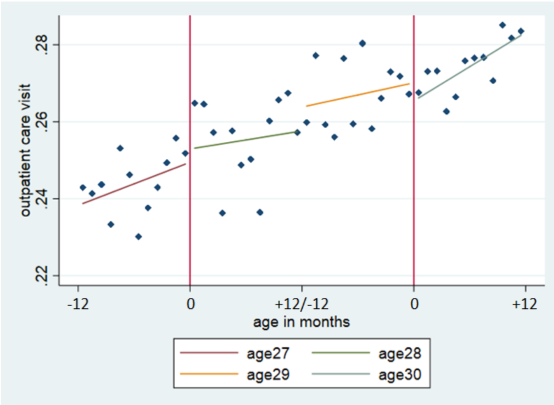


(e) Disposable income (age 30)

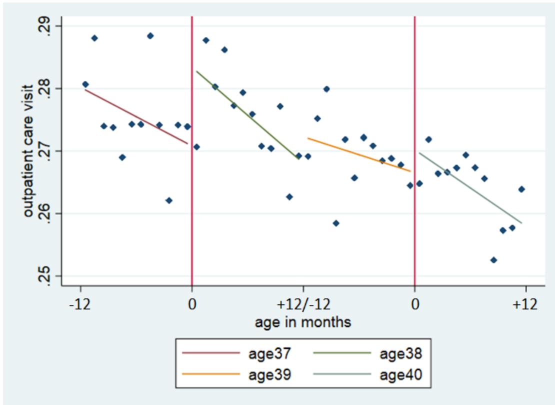


(f) Disposable income (age 40)

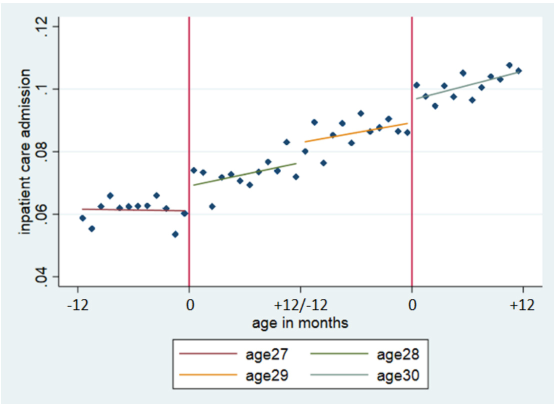
Figure 4: RD-DID plots for baseline covariates
Notes: The bin size corresponds to one month. The zeros on the x-axis mark the left (counterfactual) threshold and the right (actual) threshold. The whole four-year age interval used in the empirical analysis is shown.



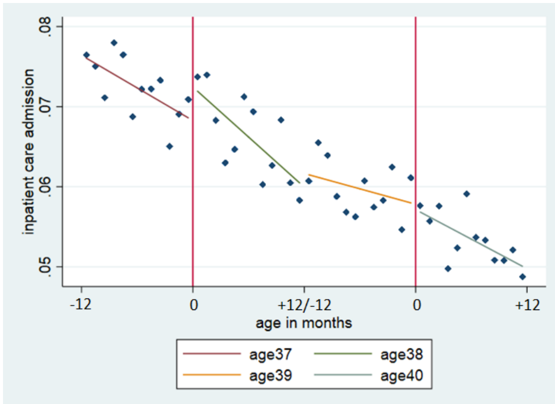
(a) Outpatient care visit (age 30)



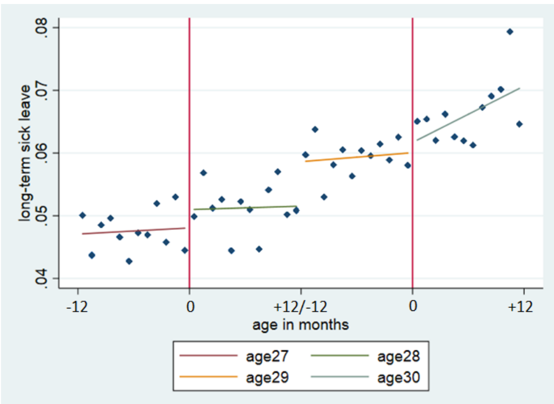
(b) Outpatient care visit (age 40)



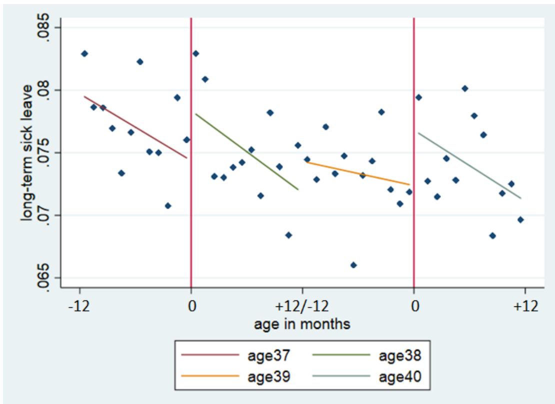
(c) Inpatient care admission (age 30)



(d) Inpatient care admission (age 40)

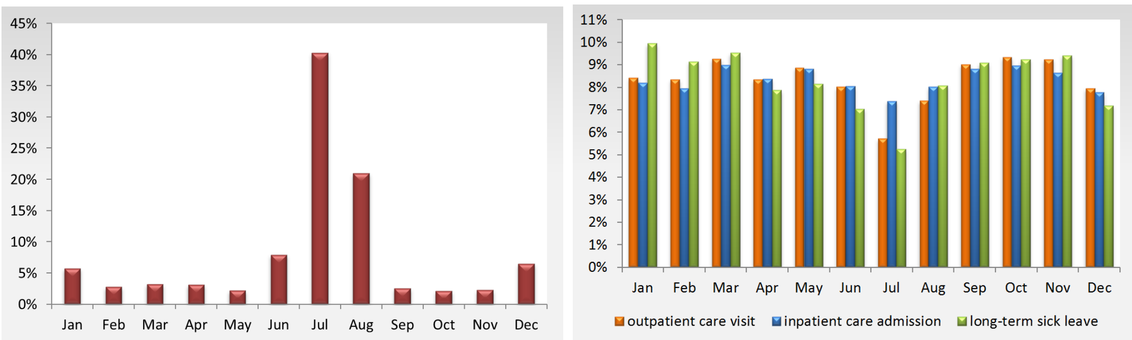


(e) Long-term sick leave (age 30)



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

Figure 5: RD-DID plots for main outcomes
Notes: The bin size corresponds to one month. The zeros on the x-axis mark the left (counterfactual) threshold and the right (actual) threshold. The whole four-year age interval used in the empirical analysis is shown.



(a) Vacation (b) Health

Figure 6: 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 specialized outpatient care visits, inpatient care admissions, and long-term sick leaves, respectively, for the years 2006-2011, based on register data from the SIP database.

Table 1: Descriptive statistics

	Mean (Standard deviation)	
	Age 30	Age 40
Year of birth	1975.4 (4.4)	1966.0 (4.4)
Sex (1=female)	.477 (.499)	.478 (.500)
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)
Country of birth (1=outside)	.090 (.286)	.091 (.287)
Citizenship (1=foreign)	.051 (.221)	.041 (.199)
Married	.206 (.404)	.518 (.500)
Lone parent	.014 (.118)	.073 (.259)
Any children, aged 0-17	.258 (.438)	.773 (.419)
Any children, aged 0-6	.251 (.434)	.486 (.500)
Any children, aged 7-12	.022 (.145)	.454 (.498)
Any children, aged 13-17	.000 (.019)	.173 (.379)
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 outpatient care visit†	.262 (.440)	.271 (.444)
Any inpatient care admission	.083 (.275)	.063 (.243)
Any long-term sick leave	.057 (.232)	.075 (.263)
Any recent health problem, previous year‡	.092 (.289)	.117 (.321)
Any recent health problem, two previous years‡	.150 (.357)	.208 (.406)
Any recent health problem, three previous years‡	.191 (.393)	.285 (.451)
Observations	182,326	245,965

Notes: Standard deviations are in parentheses. The data are based on the pooled sample for 1997-2011 and refer to 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 the variable outpatient care visit as no data from before 2001 are available. ‡ A recent health problem is defined as having had at least one inpatient care admission or one long-term sick leave in year $t-1$, $t-1$ and $t-2$, or $t-1$ to $t-3$.

Table 2: Ten most common occupational groups, 2001-2011

Age 30 (n=135,744)		
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%	

Age 40 (n=190,498)		
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%	

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 at each age 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 3: Test of the continuity of baseline covariates

	p-values					
	Age 30			Age 40		
	Means*	RD†	RD-DID†	Means*	RD†	RD-DID†
Sex (1=female)	.686	.013	.393	.644	.003	.681
No high school education	.617	.315	.840	.007	.997	.999
High school education	.006	.000	.229	.000	.023	.895
Some tertiary education	.435	.003	.589	.214	.486	.563
University graduate	.011	.875	.210	.000	.125	.685
Country of birth (1=outside)	.064	.000	.113	.561	.017	.489
Citizenship (1=foreign)‡	.009	.000	.406	.066	.000	.797
Married‡	.000	.000	.677	.000	.306	.206
Any children, aged 0-17‡	.000	.000	.595	.000	.065	.542
Any children, aged 0-6‡	.000	.000	.757	.000	.000	.601
Any children, aged 7-12‡	.000	.000	.010	.000	.157	.026
Any children, aged 13-17‡	.001	.521	.611	.000	.000	.071
Gross earnings (in 2011 SEK)‡	.000	.002	.001	.000	.249	.677
Earned income (in 2011 SEK)‡	.000	.000	.000	.000	.978	.367
Disposable income (in 2011 SEK)‡	.000	.002	.141	.000	.447	.160
Any recent health problem, previous year	.000	.006	.383	.004	.202	.104
Any recent health problem, two previous years	.000	.000	.923	.000	.140	.043
Any recent health problem, three previous years	.000	.000	.928	.000	.195	.031
Observations	102,348	102,348	182,326	123,568	123,568	245,965

Notes: The data are based on the pooled sample for 1997-2011. Observations with missing information on either sex, education, country of birth, civil status, or income are excluded. * The first p-value refers to a t-test of the difference in means in the 12-month interval on each side of the actual threshold. † The second (third) p-value refers to the coefficient of the treatment indicator in a local linear RD (RD-DID) regression with separate trends using a 24-month bandwidth around 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.

Table 4: McCrary density test

Dependent variable: Number of observations per age in months

	Age 30				Age 40			
Column	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
A. Full sample								
Treatment effect	-985.8***	-1026.6***	-393.2	-381.1	-971.1**	-996.0**	51.8	5.5
	(293.7)	(277.8)	(368.7)	(356.5)	(364.2)	(418.4)	(495.9)	(553.7)
R-squared	0.66	0.80	0.86	0.91	0.36	0.52	0.40	0.56
B. Native-born sample								
Treatment effect	-1014.6***	-1061.4***	-407.4	-395.0	-916.9**	-936.6**	70.4	35.3
	(271.0)	(271.3)	(342.3)	(348.7)	(352.1)	(409.9)	(473.4)	(536.5)
R-squared	0.68	0.80	0.85	0.90	0.35	0.50	0.39	0.55
Observations	24	24	48	48	24	24	48	48
Bandwidth	24m	24m	2x24m	2x24m	24m	24m	2x24m	2x24m
Polynomial	NO	YES	NO	YES	NO	YES	NO	YES
Estimation	RD	RD	RD-DID	RD-DID	RD	RD	RD-DID	RD-DID

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. The bin size corresponds to one month. A standard RD (RD-DID) design is used 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 5: Main results

	Outpatient care	Inpatient care	Sick leave
A. Age 30			
Treatment effect	-0.0055	0.008	-0.0007
	(0.0097)	(0.0052)	(0.0046)
Baseline mean	0.2621	0.0825	0.0571
Observations	135,744	182,326	182,326
R-squared	0.022	0.015	0.014
B. Age 40			
Treatment effect	-0.0096	-0.0047	0.0000
	(0.0084)	(0.0038)	(0.0044)
Baseline mean	0.2710	0.0628	0.0748
Observations	190,498	245,965	245,965
R-squared	0.014	0.006	0.011

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. 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 admission and long-term sick leave and 264 clusters for specialized outpatient care visit 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 24-month bandwidth around each threshold and including fixed effects for calendar year and government agency but no controls.

Table 6: Results by diagnosis group

	Outpatient care	Inpatient care	Sick leave
A. Age 30			
- All diagnoses	-0.0055	0.0008	-0.0007
	(0.0097)	(0.0052)	(0.0046)
- Neoplasms	-0.0002	-0.0002	-0.0007*
(C00-D48)	(0.0027)	(0.0006)	(0.0004)
- Mental & behavioral disorders	0.0021	-0.0004	-0.0012
(F00-F99)	(0.0021)	(0.0008)	(0.0019)
- Diseases of the respiratory system	-0.0014	-0.0002	-0.0004
(J00-J99)	(0.0027)	(0.0008)	(0.0007)
- Diseases of the digestive system	-0.0007	-0.0005	0.0005
(K00-K93)	(0.0025)	(0.0012)	(0.0007)
- Musculoskeletal diseases	-0.0017	0.0008	0.0015
(M00-M99)	(0.0032)	(0.0009)	(0.0013)
- Diseases of the genitourinary system	-0.0046	-0.0003	0.0004
(N00-N99)	(0.0039)	(0.0008)	(0.0003)
- Pregnancy & childbirth	-0.0083	-0.0033	0.0009
(O00-O99)†	(0.0075)	(0.0085)	(0.0034)
- Symptoms not elsewhere classified	-0.0013	0.0015	0.0012*
(R00-R99)	(0.0037)	(0.0013)	(0.0007)
- Injuries & poisoning	-0.0036	-0.0006	-0.0010
(S00-T98)	(0.0040)	(0.0014)	(0.0009)
- Other contacts with health services	-0.0034	0.0010	-0.0005
(Z00-Z99)	(0.0059)	(0.0008)	(0.0004)
Observations	135,744	182,326	182,326
B. Age 40			
- All diagnoses	-0.0096	-0.0047	0.0000
	(0.0084)	(0.0038)	(0.0044)
- Neoplasms	0.0033	-0.0009	-0.0003
(C00-D48)	(0.0029)	(0.0008)	(0.0006)
- Mental & behavioral disorders	0.0014	-0.0003	0.0008
(F00-F99)	(0.0018)	(0.0007)	(0.0020)
- Diseases of the respiratory system	-0.0013	0.0002	0.0003
(J00-J99)	(0.0023)	(0.0007)	(0.0010)
- Diseases of the digestive system	-0.0004	-0.0015	0.0000
(K00-K93)	(0.0024)	(0.0011)	(0.0006)
- Musculoskeletal diseases	-0.0011	-0.0003	-0.0013
(M00-M99)	(0.0033)	(0.0010)	(0.0015)
- Diseases of the genitourinary system	-0.0004	-0.0004	0.0000
(N00-N99)	(0.0039)	(0.0009)	(0.0005)
- Pregnancy & childbirth	-0.0033	-0.0061	-0.0013
(O00-O99)†	(0.0052)	(0.0061)	(0.0027)
- Symptoms not elsewhere classified	-0.0009	0.0014	-0.0000
(R00-R99)	(0.0034)	(0.0011)	(0.0007)
- Injuries & poisoning	-0.0005	-0.0005	0.0001
(S00-T98)	(0.0037)	(0.0011)	(0.0009)
- Other contacts with health services	-0.0087*	0.0012	-0.0002
(Z00-Z99)	(0.0050)	(0.0007)	(0.0004)
Observations	190,498	245,965	245,965

Notes: *** p<0.01, ** p<0.05, * p<0.1. 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) and the full sample using a 24-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 7: Results by subgroups

	Outpatient care	Inpatient care	Sick leave	Observations‡
A. Age 30				
- Full sample	-0.0055	0.0008	-0.0007	135,744 / 182,326
	(0.0097)	(0.0052)	(0.0046)	
- Women	-0.0067	0.0010	-0.0013	67,213 / 87,000
	(0.0144)	(0.0094)	(0.0080)	
- Men	-0.0061	-0.0015	-0.0004	68,531 / 95,326
	(0.0131)	(0.0043)	(0.0046)	
- Excluding parents†	-0.0168	-0.0027	0.0014	101,875 / 135,234
	(0.0106)	(0.0040)	(0.0040)	
- Parents†	0.0245	0.0193	-0.0013	33,869 / 47,092
	(0.0250)	(0.0178)	(0.0149)	
- Excluding lone parents†	-0.0042	0.0001	-0.0005	133,963 / 179,766
	(0.0099)	(0.0052)	(0.0045)	
- Lone parents†	-0.0477	0.0551	0.0065	1,781 / 2,560
	(0.0999)	(0.0660)	(0.0681)	
- With university degree	0.0006	0.0022	0.0003	91,280 / 118,136
	(0.0112)	(0.0064)	(0.0049)	
- Without university degree	-0.0221	-0.0047	-0.0042	44,464 / 64,190
	(0.0189)	(0.0093)	(0.0088)	
- Health problems during	0.0261	0.0084	-0.0165	25,968 / 34,868
three previous years	(0.0279)	(0.0158)	(0.0165)	
B. Age 40				
- Full sample	-0.0096	-0.0047	0.0000	190,498 / 245,965
	(0.0084)	(0.0038)	(0.0044)	
- Women	-0.0070	-0.0071	-0.0056	94,123 / 117,568
	(0.0118)	(0.0072)	(0.0073)	
- Men	-0.0092	-0.0006	0.0060	96,375 / 128,397
	(0.0105)	(0.0039)	(0.0049)	
- Excluding parents†	-0.0314*	-0.0028	-0.0137	43,284 / 55,933
	(0.0169)	(0.0066)	(0.0084)	
- Parents†	-0.0032	-0.0048	0.0042	147,214 / 190,032
	(0.0087)	(0.0049)	(0.0051)	
- Excluding lone parents†	-0.0113	-0.0052	-0.0006	176,854 / 228,109
	(0.0085)	(0.0040)	(0.0046)	
- Lone parents†	0.0138	0.0008	0.0039	13,644 / 17,856
	(0.0374)	(0.0166)	(0.0180)	
- With university degree	-0.0190	-0.0059	-0.0017	102,183 / 126,839
	(0.0121)	(0.0054)	(0.0061)	
- Without university degree	0.0016	-0.0041	0.0018	88,315 / 119,126
	(0.0125)	(0.0054)	(0.0070)	
- Health problems during	-0.0084	-0.0001	-0.0028	56,567 / 70,097
three previous years	(0.0168)	(0.0097)	(0.0105)	

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. 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 24-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.

Table 8: Robustness checks

	Outpatient care	Inpatient care	Sick leave	Observations†
A. Age 30				
- Full sample, main specification	-0.0055	0.0008	-0.0007	135,744 / 182,326
	(0.0097)	(0.0052)	(0.0046)	
- Controls	-0.0052	0.0013	-0.0010	135,744 / 182,326
	(0.0096)	(0.0052)	(0.0045)	
- Controls & polynomial	-0.0090	-0.0016	-0.0011	135,744 / 182,326
	(0.0144)	(0.0081)	(0.0068)	
- 12-month bandwidth	-0.0026	-0.0013	0.0016	67,804 / 90,886
	(0.0136)	(0.0075)	(0.0067)	
- 2-month bandwidth	-0.0100	0.0029	0.0018	10,339 / 14,013
	(0.0175)	(0.0092)	(0.0077)	
- No fixed effects	-0.0080	-0.0006	-0.0013	135,744 / 182,326
	(0.0097)	(0.0052)	(0.0046)	
- Native-born sample	-0.0020	0.0014	-0.0020	122,198 / 165,911
	(0.0105)	(0.0054)	(0.0048)	
- Excl. temporary secondary employment	0.0013	0.0030	0.0007	104,011 / 139,593
	(0.0113)	(0.0061)	(0.0052)	
- Excl. academic staff & managers	-0.0096	-0.0097	-0.0046	100,209 / 100,209
	(0.0111)	(0.0069)	(0.0061)	
- Years 1997-2006	0.0054	0.0035	0.0065	74,709 / 121,291
	(0.0129)	(0.0064)	(0.0054)	
- Health in January-May	-0.0105	0.0024	0.0015	135,744 / 182,326
	(0.0077)	(0.0034)	(0.0030)	
- Health in June-August	-0.0016	-0.0019	-0.0007	135,744 / 182,326
	(0.0059)	(0.0027)	(0.0024)	
- Health in September-December	0.0026	0.0000	-0.0005	135,744 / 182,326
	(0.0075)	(0.0034)	(0.0028)	
B. Age 40				
- Full sample, main specification	-0.0096	-0.0047	0.0000	190,498 / 245,965
	(0.0084)	(0.0038)	(0.0044)	
- Controls	-0.0090	-0.0046	0.0002	190,498 / 245,965
	(0.0083)	(0.0037)	(0.0044)	
- Controls & polynomial	-0.0020	-0.0084	-0.0031	190,498 / 245,965
	(0.0135)	(0.0058)	(0.0070)	
- 12-month bandwidth	-0.0082	-0.0078	-0.0049	95,359 / 123,100
	(0.0126)	(0.0055)	(0.0066)	
- 2-month bandwidth	0.0018	-0.0064	0.0010	14,897 / 19,263
	(0.0145)	(0.0072)	(0.0077)	
- No fixed effects	-0.0089	-0.0049	0.0004	190,498 / 245,965
	(0.0086)	(0.0038)	(0.0044)	
- Native-born sample	-0.0084	-0.0051	-0.0003	172,644 / 223,604
	(0.0086)	(0.0039)	(0.0047)	
- Excl. temporary secondary employment	-0.0121	-0.0046	0.0030	145,179 / 187,779
	(0.0092)	(0.0044)	(0.0055)	
- Excl. academic staff & managers	-0.0075	-0.0055	0.0012	162,542 / 162,542
	(0.0092)	(0.0046)	(0.0056)	
- Years 1997-2006	0.0066	-0.0034	0.0025	98,702 / 154,169
	(0.0109)	(0.0042)	(0.0052)	
- Health in January-May	-0.0079	-0.0063**	-0.0010	190,498 / 245,965
	(0.0064)	(0.0025)	(0.0033)	
- Health in June-August	-0.0052	-0.0019	-0.0014	190,498 / 245,965
	(0.0058)	(0.0019)	(0.0022)	
- Health in September-December	-0.0029	0.0040*	-0.0016	190,498 / 245,965
	(0.0060)	(0.0022)	(0.0027)	

Notes: *** p<0.01, ** p<0.05, * p<0.1. 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 admission and long-term sick leave, 264 clusters for specialized outpatient care visit and the occupation-specific subsample, 180 (132 for outpatient care) clusters for 12-month bandwidth, 240 (144 for outpatient care) clusters for 1997-2006, robust standard errors for 2-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 were 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 9: Placebo regressions

	Outpatient care	Inpatient care	Sick leave
A. Age 29			
Treatment effect	0.0064	-0.0027	0.0011
	(0.0108)	(0.0053)	(0.0046)
Baseline mean	0.2541	0.0701	0.0510
Observations	116,067	156,869	156,869
R-squared	0.022	0.015	0.015
B. Age 39			
Treatment effect	-0.0013	0.0057	0.0001
	(0.0069)	(0.0044)	(0.0043)
Baseline mean	0.2745	0.0700	0.0760
Observations	190,127	244,951	244,951
R-squared	0.015	0.008	0.011

Notes: *** p<0.01, ** p<0.05, * p<0.1. 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 24-month bandwidth.

Table 10: Non-health outcomes

	Gross earnings	Earned income	Disposable income	Any secondary employment	Marriage	Lone parenthood	Any children, aged 0-17 years
A. Age 30							
Treatment effect	152.3	761.3	-2654.4	0.0118	0.0116	0.0021	-0.0112
	(1751.6)	(1429.9)	(3018.7)	(0.0077)	(0.0079)	(0.0024)	(0.0081)
Baseline mean	261,638	276,259	209,369	0.2344	0.2060	0.0140	0.2583
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
B. Age 40							
Treatment effect	-1675.2	-1563.3	-454.8	-0.0002	-0.0031	-0.0006	-0.0110
	(2306.6)	(1961.1)	(1805.1)	(0.0074)	(0.0082)	(0.0044)	(0.0070)
Baseline mean	321,027	338,541	258,129	0.2366	0.5179	0.0726	0.7726
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

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors in parentheses are clustered at the discrete values of the running variable multiplied by the calendar year (i.e., 360 clusters). 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 24-month bandwidth around each threshold and including fixed effects for calendar year and government agency but no controls.