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Health Responses to a Wealth Shock: Evidence from a Swedish Tax Reform

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This essay contributes in two ways to the literature on the effects of economic circumstances on health. First, it deals with reverse causality and omitted variable bias by exploiting exogenous variation in inherited wealth generated by the unexpected repeal of the Swedish inheritance tax. Second, it analyzes responses in health outcomes from administrative registers. The results show that increased wealth has limited impacts on objective adult health over a period of six years. This is in line with what has been documented previously regarding subjective health outcomes. If anything, it appears as if the wealth shock resulting from the tax reform leads people to seek care for symptoms of disease, which result in that cancer is detected and possibly treated earlier. One possible explanation for this preventive response is that good health is needed for enjoying the improved consumption prospects generated by the wealth shock.

Keywords: inheritances, tax reform, wealth shock, objective health
JEL Codes: D10, I10, I12, I14, H30

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1 Introduction

It has long been recognized that there exists a positive relationship between many measures of economic wealth and a variety of health outcomes.[1]

This 'gradient' has become a significant concern for politicians and public health officials as it implies that inequalities between rich and poor do not only appear as differences in consumption and material well-being, but also in life-expectancy and quality of life. Unfortunately, any policy intervention targeted at reducing these inequalities, or promoting public health in general, suffers from the fact that we still know little about *if* and *how* economic wealth affects health.

Answering these questions is further complicated by the possibility that causation may go in the opposite direction, from health to wealth.[2] It could also be that unobserved factors, such as genetic endowment, early childhood exposures or time preferences, influence wealth and health in the same direction without a causal link.[3]

Given the practical constraints on randomizing people to receive different amounts of wealth, researchers have tried to solve these methodological challenges with quasi-experimental designs, in particular by exploiting exogenous variation generated from individual wealth or income shocks. Important examples include lottery winnings (Lindahl, 2005; Gardner and Oswald, 2007; Apouye and Clark, 2013; Cessarini et al., 2013), stock market fluctuations (Schwandt, 2012), inheritances (Meer et al. 2003; Kim and Ruhm 2012; Carman 2013) and unanticipated policy changes (Jensen and Richter, 2003; Case, 2004; Frijters et al., 2005; Snyder and Evans, 2006).[4] The general finding is that wealth and income have limited impacts on adult health in the short to medium run.

Previous studies are limited by the fact that they are based almost entirely on survey data on subjective general health status. Although it has been argued that general health status is a good predictor of future morbidity and mortality (Idler and Benjamini, 1997; van Doorslaer and Gerdtham, 2003), there are reasons to question general health status as a dependent variable in this context. Subjective health status is, for example, likely to be influenced by factors such as social norms regarding health, use of health care as well as understanding of the survey questions, which are themselves systematically related to wealth and income in such a way that the coefficient estimates are

[1] See Marmot (1999), Smith (1999), Deaton (2003), and Cutler et al. (2011) for reviews of the literature.
[2] For examples of studies investigating the impact of health shocks on labor market outcomes, see Lundborg et al. (2011), and on wealth, see Wu (2003).
[3] For studies discussing these issues, see for example Barker (1997), Almond and Currie (2013), Fuchs (1982) and Barsky et al. (1997).
[4] Other quasi-experimental designs in this context include IV estimators (see for instance Ettner, 1996) and Granger causality testing (see for example Adams et al., 2003 and Michaud and van Soest, 2008).

biased towards zero (see for example Murray and Chen, 1992, and Bago d'Uva et al., 2008). Moreover, subjective general health status does not separate between different aspects of health. For instance, it has been shown that improved wealth leads to, on the one hand, harmful behaviors like smoking and drinking and, on the other hand, to reduced obesity, lower stress and enhanced mental well-being, suggesting that important health effects may go unnoticed (Lindahl, 2005; Apouye and Clark, 2013; Kim and Ruhm, 2012).

This paper tackles causality by exploiting a previously untapped and policy-relevant source of exogenous variation in wealth; namely the repeal of the Swedish inheritance tax on December 17, 2004.[5] Heirs who received inheritance above the tax threshold from parents who passed away after the reform are defined as being treated as they experienced a favorable wealth shock equal to what their tax payments would have been had the decedent died before the reform. Calculations indicate that the wealth shock amounted to, on average, SEK 70,000 (about USD 9,500 in 2004 values), or 7 percent of initial wealth. The empirical strategy is to estimate the causal effect of the wealth shock on health by approximating the counterfactual outcome with the health experiences of heirs who received inheritance above the tax threshold before the reform date. The relevant sample is collected from an administrative database covering the entire population of heirs of deceased Swedes over the period 2003–2005. Results from several tests show that the treated and the controls are comparable in predetermined characteristics, including health, implying that any difference in health between the two groups following the inheritance could reasonably be attributed to the wealth shock. I also conduct placebo experiments which tests for responses in a sample of heirs who received parental bequests below the tax threshold and hence, for whom the reform should have no impact. The results from these tests support the validity of the empirical strategy.

The health outcomes are collected from medical records, death certificates and the Swedish sickness insurance register and share the feature that they are based on the medically qualified opinions of physicians. As far as I am aware, this is the first study to use objective health outcomes from administrative registers, other than mortality, to investigate the effects of increased economic resources on health.[6]

The main health outcome is an indicator of whether the individual has been hospitalized for any cause in a given year. Comparing the incidences of hospitalization between the treated and controls over time, ten years before and six year after the inheritance, show that the wealth shock increases the

[5] Eliason and Ohlsson (2013) use the repeal of the inheritance tax to study behavioral responses to taxation among individuals *leaving* inheritances.
[6] An earlier version of this paper appeared in my PhD dissertation "Economic Decisions and Social Norms in Life and Death Situations", see Erixson (2013).

probability of hospitalization by five percent. This is equal to the impact of being four years older. Tests for heterogeneous responses suggest that the effect is primarily driven by the relatively old, women and those with a low level of education.

At a first blush, the positive effect on hospitalization may be interpreted as if the wealth shock has detrimental consequences for health, especially since health care in Sweden is universal and basically free of charge.[7] Tests for heterogeneous responses across diagnoses reported in connection with the hospital admissions show, however, that the wealth effect is evident in only two diagnose categories: 'symptoms of disease' (e.g. shortness of breath, fever, general feeling of illness, etc.) and 'cancer'. Regarding cancer, previous studies document that improved wealth leads to more smoking and drinking, behaviors which are positively related with the disease. That the current wealth effect is operating through these channels seems unlikely, however, given the relatively limited time period over which it is estimated. If the wealth shock leads to more smoking and drinking I should rather see responses in diagnoses which are more immediately related with these risk factors (e.g. injuries, mental problems, respiratory diseases, etc.). Likewise, if the shock leads to reduced obesity or improved mental well-being (which has also been indicated by previous studies) I should be more likely to find a *reduction* in cancer incidence rather than an increase. A more realistic explanation is therefore that cancer has been detected during health care visits for minor health contingencies (i.e. symptoms of disease). That the wealth shock leads to more health care visits, although health care in Sweden is free, could potentially be explained by people demanding good health to fully benefit from their improved future consumption prospects.

To get a better understanding of how the wealth shock affects different aspects of health, I test for responses in (publicly insured) sick leave amounting to more than two weeks and in all-cause mortality, as these two health outcomes are likely to capture health events which are both less and more severe than those resulting in hospital admissions. The results show that the wealth shock does not have any statistically significant effects on either of the two outcomes. Although the insignificant wealth effect on sick leave may be attributed to the fact that the analysis is based on the working-age population (for whom the wealth shock has no detectable effect on hospitalization), the finding lends additional support for the conclusion that the wealth shock has negligible consequences for health. The insignificant effect on mortality is expected given the insignificant effect on the prevalence of diseases other than cancer (for which the impact is apparently too small to translate into mortality, at least over a period of six years).

In sum, the results show that more wealth has limited short to medium run consequences for objective adult health. This is line with what has been

[7] See Glengård et al. (2005) for an excellent description of the Swedish health care system.

found regarding subjective health. It appears however as if the wealth shock leads to preventive behaviors, which may have long-term beneficial consequences for health.

The outline of this paper is as follows. In Section 2, I discuss the theoretical predictions regarding the effect of wealth on health together with an overview of the previous empirical literature. Section 3 describes the inheritance tax, with a particular focus on the unexpected repeal. In section 4, I discuss the data used in the empirical analysis. Section 5 presents the empirical strategy and in section 6, I present evidence that the wealth shock is exogenous. Section 7 provides the results and section 8, finally, is a concluding discussion. Each section begins with a short summary of its main points.

2 Review of related literature

This section starts with a discussion on the theoretical arguments for why increased wealth may affect health. The second sub-section gives a review of the previous empirical literature. The general finding is that wealth shocks have a limited impact on self-assessed general health status and longevity. It appears, however, as if improved financial resources, on the one hand, leads people to engage more in health behaviors and lifestyles which are possibly detrimental in the long run (e.g. smoking and drinking) and, on the other hand, have beneficial consequences in form of reduced obesity, lower stress and improved mental well-being.

2.1 Theoretical arguments for causal effects of wealth on health

The common hypothesis in the literature is that improved economic resources lead to better health. Although it is largely motivated by stylized facts regarding the positive correlation between wealth and health, theoretical support for the hypothesis can be found in Grossman's model of health capital (Grossman, 1972, 2000).[8] According to this model, people demand health for the consumption benefits (good health gives utility), in addition to the production benefits (more healthy time available for work, consumption and health investments). Healthy time available for market and non-market activities depends on the stock of health capital, which depreciates over the lifecycle to a threshold where death occurs. The individual, however, may counteract the deterioration process by investing in her health. In accordance with Becker's household production model (Becker, 1976), health is produced by combining market goods and time.

[8] See Muurinen (1982) and Ehrlich and Chuma (1990) for extensions of the Grossman framework.

More wealth will make health investments subjectively cheaper, lead to increased demand for health and, eventually, improved health.

In recent years, additions to the health-capital model have been made to account for the possibility that the individual derives utility not only from health enhancing consumption (e.g. healthy foods and exercise), but also from consumption which is negatively correlated with health (e.g. drinking and smoking), see for example Galama and Van Kippersluis (2010) and Van Kippersluis and Galama (2013).[9] According to these models, improved economic resources will relax the individual's budget constraint allowing a higher level of both types of consumption. Nevertheless, as unhealthy consumption is associated with a cost in the form of reduced health and shorter lifespan, the rise in healthy consumption will be relatively larger.

2.2 Findings in the previous literature

Three previous studies have used inheritances to identify the effects of wealth on health. Meer et al., (2003) use data from the Panel Study of Income Dynamics to analyze the impact of wealth on self-reported health status. The authors use receiving an inheritance as an instrument for changes in wealth and find what they interpret as "a quantitatively small effect" and conclude that the wealth-health connection is not driven by short-term changes in wealth. There are two concerns regarding the identification strategy employed by Meer et al. First, inheritances need not randomly distributed, but correlated with unobserved determinants of health. Second, the interpretation of the effect is complicated by the possibility that inheritances are anticipated. If the heir has adjusted her health behavior or lifestyle in anticipation of the inheritance, the estimate will then understate the true effect. In a related study, Kim and Ruhm, (2011) compare health consequences of people in the Health and Retirement Study (HRS) who have received inheritances in excess of $10,000 with people who have inherited small amounts (<$10,000), which are assumed to not affect health. The authors attempt to account for unobserved individual heterogeneity by estimating models with large sets of observable characteristics, including lagged health, and they exploit data on the individual's subjective probability of receiving an inheritance in order to address the issue of possible anticipatory effects. The results show that the wealth shock has no effect on self-reported health status, but that it seems to lead to an increase in the prevalence and intensity of social drinking, in addition to a reduction in obesity. In a recent study, Carman (2013) contributes to the two previous

[9] These extensions are largely motivated by epidemiological research which documents that a large fraction of the socioeconomic disparities in adult health in developed countries can be accounted for by disparities in lifestyles and consumption (McGinnis and Foege, 1993; Mokdad et al., 2004; Contoyannis and Jones, 2004; Cutler et al., 2011).

studies by comparing the results from models with and without individual fixed effects to test for the influence of unobserved heterogeneity across individuals who receive and not receive inheritance in the PSID. Her first main result is that the inherited amount does not have any effect on self-reported health status, independently of model specification. Her second main result is that the effect of receiving inheritance (irrespectively of amount) is positive and significant in the specification without fixed effects, but not in the fixed-effects specification. This suggests that individuals who receive inheritances have better health than those who do not receive inheritances, but that there is no change in health following the receipt.

Another source of plausibly exogenous variation in economic resources is lottery winnings. Using data on lottery winners from the Swedish Level of Living Surveys, Lindahl (2005) finds that increased income is associated with improved health, measured by an index of self-reported illnesses and symptoms, as well as increased life expectancy. The income effect on health appears to be strongest for the oldest individuals. Moreover, Lindahl (2005) finds evidence of decreased obesity as a result of higher lottery winnings, suggesting that wealth may affect health through health-related consumption, such as exercise and healthy food. Unfortunately, however, the sample is limited to winners and contains no information on the frequency of lottery playing. In a related study, Gardner and Oswald (2007) focus solely on lottery winners in the British Household Panel Survey and identify causation with variation in the size of the prize. By doing so, they implicitly assume that winners of small and large prizes have similar unobserved characteristics, which is not obvious. Their results show that winning a large prize, compared to a small, enhance subjective mental well-being two years after winning. Apouye and Clark (2013) use the same dataset and identification strategy as Gardner and Oswald to test for responses, not only in mental well-being, but also in self-reported measures of physical and general health. Their results show that the wealth shock has no detectable effect on general health but that it produces better mental health. The authors explain the lack of effect in the former variable by showing that winning the lottery leads to more smoking and drinking, behaviors with plausibly detrimental effects on general health. The main objection against lottery winnings is that they are randomly assigned and only conditional on participation in the lottery and, thus, that the results may be confounded by selection bias (Van Kippersluis and Galama, 2013). More specifically, because lottery players tend to have lower incomes and less education than non-players, the empirical estimates are likely to generalize only to the lower segments of the socioeconomic distribution. In an unpublished paper, Cessarini et al. (2013) contribute to the previous studies by using a sample of around 3 million Swedish lottery players, covering individuals throughout the socioeconomic distribution. Another novel feature of the data is it contains information on the individual's expenditures associated with the lottery, allowing the authors to effectively control for the probability of winning the prize. The results show that the prize money has no detectable impacts on health care utilization and mortality over a period of ten years, casting doubt on the identification strategies in previous lottery studies. The study does find, however, that the wealth shock decreases the consumption of drugs related to mental health. This could potentially be interpreted as if increased wealth has an anxiolytic influence on stress.

Stock-market fluctuations constitute another source of variation in wealth which is unlikely to be induced by health (Smith, 1999). Schwandt (2012) exploits the wealth gains and losses generated in the US stock market during a time-period of 18 years. Using data on a sample of retirees from the HRS, he finds that a ten percent wealth increase over two years leads to a significant improvement in an index constructed of different survey measures of physical and mental health, as well as reduced mortality. It appears as if the wealth shock reduces the incidence of diseases of the heart, hypertension and psychiatric problems, suggesting that psychological factors may be the mechanism through which the wealth effect operates. As with lottery winnings, however, stock market swings are experienced by a specific subset of the population, which in this case tend to be relatively wealthy (Mankiw and Zeldes, 1991; Poterba and Samwick, 2003; Smith, 2004).

A second branch of studies in the field have exploited variation in income and wealth generated by changes in government policies. One advantage with policy changes is that they usually affect a larger segment of the population. Therefore, they may be more relevant from a policy perspective than individual shocks. Using cross-sectional data on self-reported health status of Black South Africans who had their income doubled due to a change in the pension system, Case (2004) finds evidence of improvements in general health. These, interestingly, not only manifest themselves for the recipient, but for all household members. Moreover, Case shows that the effect is likely to stem from improved sanitation, housing, health care as well as reduced stress. It is, however, unclear whether these results are applicable to a Western population. Jensen and Richter (2003) study a pension crisis in Russia during which many retirees did not receive their pensions for an extended period of time. The average decrease in income for this group was 24 percent. Examining the longitudinal effects of this adverse shock, the authors find evidence of reduced nutritional intake and utilization of health care in the short run. They also find that the likelihood to die in the two years following the crisis increased by five percent. Similarly, Snyder and Evans (2006) use a legislative change in the US Social Security system which unexpectedly lowered the benefits for people born after January 1, 1917 - the so called "Notch" generation. A comparison of five-year mortality rates after age 65 for males born in the first quarter of 1917 and the last quarter of 1916 show that the Notch had slightly lower five-year mortality rates than the previous cohort. The authors suggest that this countervailing finding is partly due to the fact that the people in the Notch cohort increased their post-retirement labor supply, which in turn had beneficial health effects through reduced social isolation. Fritjers et al. (2005) take advantage of the fact that the German reunification in 1990 resulted in large income transfers to the East German population but not to West Germans. As the collapse of East Germany was unanticipated, the authors could attribute differences in health consequences between the two groups to the resulting increase in real income. The results show a significant, but small, positive effect of the income shock on health satisfaction.

3 The Swedish inheritance tax and how it was unexpectedly repealed

This section begins with a short description of taxation of inheritance prior to the repeal. This is to get an understanding of the source of variation I use to identify the causal effect of wealth on health. After that, I discuss the way in which the tax reform was proposed, passed and implemented. The main point is that the decision to repeal the tax was largely unexpected and that the reform was enacted in a rapid way. This would imply that the affected population had limited incentives or abilities to react vis-à-vis the reform before it was implemented.

3.1 Taxation on inheritances before the reform

Prior to December 2004, legal heirs and beneficiaries of wills in Sweden were subject to inheritance taxation according to the laws stipulated in the *Inheritance and Gift Tax Ordinance*.[10] The inheritance tax, similarly, depended on the succession scheme of the relationship between the deceased and the heir.[11] For the deceased's descendants (i.e. the deceased's children and their descendants), amounts exceeding a basic deductible exemption of SEK 70,000 were taxed according to a progressive tax schedule consisting of three marginal tax brackets of: 10 percent, 20 percent and 30 percent. Table 1 reports the tax schedule for the deceased's descendants.

[10] See Ohlsson (2011) and Du Rietz et al. (2012) for excellent historical reviews of the inheritance tax.

[11] The law defined three classes of taxpayers. Class 1 contained the children and their descendants, and, before 2003, spouses and cohabiters. Class 2 constituted all other legal heirs, and Class 3 legal entities such as public institutions, charities and foundations.

Table 1: Tax rates on inheritances for the deceased's descendants.

Taxable inheritance	Tax rate
0-70	0
70-370	10%
370-670	30+20% within bracket
670-	90+30% within bracket

Note. All monetary values are in 1,000SEK.

3.2 The unexpected reform

Concerned with the growing criticism against the inheritance tax, the Social Democratic Government announced, in the Budget on September 20, 2004, that the *Inheritance and Gift Tax Ordinance* (AGL) was to be repealed starting January 1, 2005.[12]

The legislation had been criticized for complicating distributions of estates, especially those involving transfers of family firms. Escalating tax values on real estate in the early 2000's had also led to public criticism of the inheritance tax, as many heirs, especially widows, had difficulties affording the increasingly large tax payments. Although the general impression was that the legislation was in need of a reform, the Government's decision to completely remove the tax came as a surprise (Silfverberg, 2005). The tax on bequests to spouses had been removed in January 2004, but at that time there had been no indication of a removal of the tax for other heirs (SOU 2003:3). As late as in June 2004, The Property Tax Committee had presented its final report *Reform of inheritance and gift taxes* (SOU 2004:66). This report did not propose a complete removal of the tax, but rather a series of adjustments to the existing rules.[13] However, none of these were considered appropriate to implement at the time.

Unfortunately, there has been no systematic research undertaken on what factors contributed to the repeal of the inheritance tax (Du Rietz et al., 2012). According to Silfverberg (2005), the Government's "radical" decision to abolish the inheritance tax was probably a consequence of The Property Tax Committee's inability to review all rules in the AGL and work out a new modern legislation in time for the Budget. That the decision fell on the inheritance tax and not on the wealth tax, which had also been heavily

[12] The main motivation was that it would be impossible to tackle the criticism of the tax with other legislative changes. It was also emphasized that the inheritance tax generated low revenues relative to its costly administration.

[13] The report had been preceded by several governmental investigations of the Swedish tax system; none of which had proposed a complete abolition of the inheritance tax, but rather reductions of the tax rates and reforms of the valuation rules (see for example SOU 2002:52).

debated and evaluated by The Property Tax Committee, was, according to Lodin (2009), a result of a horse trade between the Social Democrats and the Left Party.[14]

After the announcement of the repeal, things happened very rapidly. The Ministry of Finance worked out a memorandum bill, the Tax Agency and the Appeal Court in Stockholm gave their comments, and on December 16, only three months after the initial announcement, the bill was passed in the Parliament. The Council of Legislation was critical of the quick manner in which the reform had been enacted and, in particular, of the limited preparation work that had preceded the bill. According to Silfverberg (2005), the swiftness of the legislative process was a contributing factor as to why the bill caused almost no political debate.[15]

The Parliamentary decision on December 16 was that the AGL would expire at the end of 2004. However, of concern of the bereaved relatives of the many Swedes who died in the Asian Tsunami on December 26, the Parliament passed a law in April 2005 on inheritance tax exemption for the period December 17–31, 2004, implying that the tax was affectively abolished on December 17.

A direct consequence of the repeal of the AGL is that inheritances from decedents who die after December 17, 2004 are exempted from taxation. Tax exemption also applies to inheritances which are received after December 17, but originates from a previously deceased parent who died prior to the reform (so-called postponed inheritances). However, if the tax liability occurred prior to December 17, the old law applies.

4 Data

In this section, the dataset is presented.[16] In the first subsection, I describe the construction of the working sample. I also describe how I separate between individuals who were affected and unaffected by the tax reform and

[14] According to Lodin (2009), Prime Minister Göran Person invited the Left Party leader Lars Ohly to a private discussion, during which he demanded that Ohly agree on removing the inheritance tax and the wealth tax. Ohly refused to abolish both taxes, but after Person issued an ultimatum—one of the taxes would in any case be removed—Ohly agreed to remove the inheritance tax.

[15] The limited debate which occurred focused mainly on the proposed date of repeal. The opposition parties argued that the tax should be abolished retroactively from 20 September 2004, i.e. from the day when the government announced the proposal in the Budget, as it would otherwise lead to an "inhuman situation" for heirs of decedents who would die in the last quarter of 2004. In its response, the Government argued that this would result in an unfair outcome because many (irreversible) cedes had already been made.

[16] Access to the data has been granted to the researchers at the Department of Economics at Uppsala University associated with project *Intergenerationella överföringar: orsaker och konsekvenser*. Due to its sensitive and confidential nature, the data cannot be exported from the closed server environment at Statistics Sweden.

particularly, how I approximate the heir's tax status using data on the deceased parent's net worth. The last subsection details the health outcomes used in the empirical analysis. These include: hospitalization, and the resulting diagnoses, insured sick leave and mortality.

4.1 The sample and approximation of tax status

Information on individuals who received inheritances before and after the repeal of the inheritance tax is collected from the Belinda database. This database consists of estate report data covering information on the entire population of heirs and beneficiaries of deceased Swedes over the period 2003-2005. The database contains around 1,120,000 individuals, but for the empirical analysis I restrict my attention to heirs who have received inheritance before (136,920) and after (76,992) the tax reform from parents who were widowed, divorced, or unmarried when they died and whose deaths resulted in an estate inventory report.[17] These sample restrictions more or less follow from the succession scheme default rules and yield a sample which is representative of the population of heirs in Sweden who receive parental bequests.

The main focus of the empirical analyses is on the heirs who were affected by the tax repeal, or, put differently, those with inheritances large enough to have rendered liability to pay the inheritance tax had the tax remained in effect. Unfortunately, the Belinda database only contains information on economic variables like the value of estate and the inherited amount for heirs who inherited before the tax reform, implying that I cannot directly observe which heirs who received inheritance exceeding the tax threshold after the reform. My solution to this problem is to approximate the heir's inheritance using data on the deceased parent's net worth from the *Swedish Wealth Register*. A novel feature of the wealth register is that the valuation principles are similar to those that apply to estates, i.e. assets and debts are valued at market values. This implies that heirs, for whom the product of the parent's net worth times the inheritance share exceeds the tax threshold, could be categorized as affected by the reform.[18]

I measure net worth three years before death for decedents who died both before as well as after the reform. This is to avoid that differential incentives for tax planning (or evasion) has resulted in systematic differences in

[17] Swedish citizens not residing in Sweden and with no assets in Sweden are exempted from this rule. Exemption from the rule is also given to the deceased's whose assets are only sufficient to cover funeral expenses and do not comprise real estate. In the latter case, a so-called estate notification should be established.

[18] Given that the sample is restricted to offspring, the inheritance share is calculated as one divided by the number of offspring appearing in the estate report, information which is available both before and after the reform.

characteristics between heirs inheriting before and after the reform.[19] To account for the possibility that economic conditions have affected the net worth for decedents dying on each side of the reform date differently, I adjust it with the annual official long-term central government borrowing rate.[20] Moreover, because the inheritance law stipulates that heirs can never be forced to pay the debts of estates in deficit, negative net worth is replaced with the value zero.

For each heir, I calculate the (gross) inheritances, referred to as *imputed inheritance*, as well as the corresponding tax payment (*imputed tax payment*) using the tax rates that applied before the reform, see Table 1. For deceased widows/widowers, the net worth may in some instances contain the inheritance of the previously deceased spouse, implying that the heirs of widowed decedents effectively receive two inheritances. To account for the fact both inheritances were subject to the deductible exemption I divide the net worth of widow/widowers into two equally sized parts, which I then distribute evenly between their children. This is in accordance with the schematic distribution applied by the Tax Agency. I then subtract SEK 70,000 from each of the two inheritances received by the heir before calculating the total tax payment.[21]

To test how well the imputed tax payment corresponds to actual tax payment, I calculate the correlation between the two measures for heirs inheriting before the repeal of the tax. (i.e. in 2003 and 2004). The raw correlation is 0.842 ($p<0.01$), suggesting that the imputed measure is a valid proxy for actual tax payment. I have data on inheritances for a representative sample of three percent of heirs of decedents who died in 2005. The correlation between the two tax measures in this sample is almost identical to that for heirs inheriting before the tax repeal (0.837, $p<0.01$). Moreover, the share of heirs with positive tax payments is very similar across the years. In sum, these calculations suggest that the imputed measure is valid both within and across the inheritance cohorts and that it can effectively be used to decide the heirs' tax status.

In total, 79,777 heirs received inheritances above the tax threshold. They are the main focus of the empirical analysis, hereafter referred to as *Main sample*. Heirs who received inheritance below the tax threshold (133,920),

[19] Recent studies show that people engage in estate tax planning (or evasion), both during life and shortly before death, and that this behavior tends to be positively correlated with wealth (Joulfaian, 2004; Nordblom and Ohlsson, 2006; Kopczuk, 2007; Eliason and Ohlsson, 2013).

[20] The estate three years before death is calculated as $\text{Estate}_{t-3}=\text{Net worth}_{t-3}*(1+i_{t-2})*(1+i_{t-1})*(1+ i_t)$, were i is the yearly official long-term central government borrowing rate and t denotes the year of death. The i:s during the considered years were: 5.34 percent (2000); 4.98 percent (2001); 5.15 percent (2002); 4.39 percent (2003); 4.30 percent (2004); 3.24 percent (2005).

[21] Because the distribution depends on the deceased's marital status, I restrict the sample to heirs whose decedents had the same marital status (i.e. widow, unmarried, or divorced) three years before death and at death.

however, are not omitted completely from the analysis. They are used in placebo experiments and in the estimation of wealth effects on mortality, hereafter referred to as *Placebo sample.*

4.2 Health outcomes[22]

The health outcomes in this paper are collected from three administrative registers: *the Swedish National Patient Register*, which contains detailed data on all hospital admissions (inpatient care), including data on diagnoses, concerning Swedish citizens, *the Integrated Database for Labour Market Research* (LISA), which contains information on sick spells covered by the national sickness insurance[23] exceeding fourteen days, and *the Cause of Death Register*, which contains data on the date and cause of death for all Swedes who die. Below, I describe the health outcomes which are obtained from these data sources.

- *Hospitalization* is an indicator variable which takes value one if the individual has been hospitalized, for any cause, at least once during the year, and otherwise zero. The variable is available for each year over the period 1993–2011, for all individuals. It should be noticed that *Hospitalization* captures health conditions severe enough to require the medical and technical expertise of hospitals.[24]

- *Diagnose* is represented by a set of indicator variables representing each of the 21 chapters in the *WHO's International Statistical Classification of Diseases and Related Health Problems* (ICD), see Table A1, Appendix A. More specifically, the indicator variables take value one if the individual, in the given year, has been hospitalized for *any* diagnosis appearing in the specific chapter, and

[22] Relevant demographic and socioeconomic variables like year of birth, sex, nationality, marital status, and education, are collected from *the Birth Register* and the *LISA* database, whereas data on incomes and wealth are gathered from population registers provided by the Tax Agency. The tax agency collects the information directly from relevant sources, such as personal tax files for incomes, and financial institutions and intermediaries for wealth. The variables are available for each year over the period 1999–2009 (except wealth which is available up to 2007).

[23] See Larsson (2002) and Hesselius et al. (2008) for informative reviews of the Swedish sickness insurance.

[24] Treatment of less severe conditions, medical check-ups and other forms of preventive care is a matter for the primary (outpatient) care. Since 2001, The Swedish Board of Health and Welfare keeps a register on outpatient care admissions. Unfortunately, these data are still of low quality and not recommended to be used for research purposes.

otherwise zero.[25] The reason for using this categorization is twofold. First, there is not enough variation to provide reliable estimates with respect to specific diagnoses. Second, it solves the problem of tractability of diagnoses before and after the reform of the ICD system in 1997, which replaced the previous ICD-9 system with the new ICD-10. The diagnose variables are available for each year over the period 1993–2011, for all individuals, and are used to investigate the reasons for the hospital admissions. The focus is on the ten variables with the highest pre-inheritance period incidences, see Table 2 (and variables in bold in Table A1). The remaining variables are grouped into one variable called *Others*.

- *Sick leave* is an indicator variable which takes value one if the individual has received sickness benefits for more than two weeks during the year, and otherwise zero. *Sick leave* could be considered an objectives measure of health since, in order to receive sickness benefits, the individual has to send in a doctor's certificate to the Swedish Social Insurance Agency verifying that the reduced working capacity is due to illness. The variable is available for each year over the period 1993–2009 for the working aged population (16–65) and functions as a complement to *Hospitalization* as it also captures minor health conditions, which are not severe enough to result in hospital admissions. A regression of *Hospitalization* on *Sick leave* yields a coefficient estimate of 0.51 ($p<0.001$) implying that the outcomes are partly correlated. This is in accordance with previous studies reporting that medically certified sick leave is a good predictor of clinically defined ill health (Marmot et la., 1995; Kivimäki et al., 2003).

- *Mortality* is represented by six indicator variables (*Mortality1*,…, *Mortality6*) which take the value one if the individual dies from any cause, *within* one up to, *within* six years after the inheritance, respectively and otherwise zero. The variables are available for all individuals. *Mortality*, similarly to *Sick leave*, functions as a complement to *Hospitalization*, but it captures the most severe state of ill health, namely death.

[25] The physician is required to report the diagnosis (mapped into ICD code) for the disease or symptom that the patient was treated for.

I have standardized *Hospitalization*, *Diagnose*, and *Sick leave* so that they are measured for the same number of years before (ten) and after (*Hospitalization*, *Diagnose*: six, *Sick leave*: four) the inheritance receipt for heirs inheriting in 2003, 2004 and 2005. Table 2 reports the annual incidences of the variables for the pre-inheritance years, as well as the share of heirs who die in any year over the six years following the inheritance (*Mortality6*).

To establish that the empirical estimates in this paper are not artifacts of the current dataset I estimate the cross-sectional relationship between wealth and health prior to the inheritance. The results, which are reported in Appendix B, show that the there is a statistically significant wealth gradient in *Hospitalization* as well as in *Sick leave*, implying that wealth is protective against ill health. This holds true both for the *Main sample* and the *Placebo sample*.

Table 2: Health outcomes, incidences, in percent.

Health outcome	Incidence
Hospitalization[a]	6.65
Diagnose[a]:	
Neoplasms	0.55
Mental	0.57
Nervous	0.26
Circulatory	0.77
Respiratory	0.31
Digestive	0.78
Musculoskeletal	0.52
Genitourinary	0.53
Symptoms	0.84
Injury	0.73
Others	0.79
Sick leave[a1]	13.3
Mortality6	3.51

Notes. [a]Incidence calculated as annual average over the ten years before the inheritance. [1]The incidence is calculated for the working-age population (16-65).

5 Empirical strategies

In this section, I present the empirical strategies to identify the causal effect of the wealth shock on the health outcomes discussed in the previous section.

A direct consequence of the repeal of the *Inheritance and Gift Tax Ordinance* is that offspring who received inheritances, amounting to more than the basic deductible exemption, from parents who died after December 17, 2004 experienced beneficial shocks to their inheritances equal in size to what their tax payments would have been had the parents died before that date.

The core of the empirical strategy is to estimate the causal effect of this wealth shock on health by approximating the counterfactual outcome (i.e. health in the absence of the wealth shock) with the health experiences of heirs who received inheritance above the tax threshold from parents who died before the reform date.

Due to the fact that it is essentially a random process determining whether an individual dies today or tomorrow, the ideal would be to compare the health of individuals whose parents died in the days surrounding the reform. This approach would be similar in spirit to a regression discontinuity design framework, where the forcing variable would be the parent's date of death. However, because only about 300 individuals die in Sweden each day, and even fewer with taxable estates, I would end up with a sample too small to provide enough power for statistical analysis in the close vicinity of the reform.

To have any hope in precisely detecting differences in health between the two groups, I define heirs receiving inheritances above the tax threshold (*Main Sample*) after December 17, 2004 and in 2005 as being *treated*, and heirs receiving inheritances above the tax threshold in 2004, before December 17, and in 2003 as *controls*. Heirs receiving inheritances below the tax threshold (*Placebo sample*) over these periods are referred to as *"treated"* and *"controls"*.

Table 3: Sample means with respect to inheritances, wealth shocks and *Hospitalization* (by time period), for *Main sample* and *Placebo sample*

			Hospitalization, by period:[3]			
	Inheritance[1]	Wealth shock[2]	Pre	Post	Post-Pre	N
			Main sample			
Treated	548,189	70,817	6.6	8.7	2.2	28,827
Controls	565,417	0	6.7	8.6	2.0	50,950
			Placebo sample			
"Treated"	32,923	0	7.6	10.1	2.4	48,165
"Controls"	34,671	0	7.8	10.1	2.3	85,967

Notes. Dummy variables are reported in percent. [1]Refers to imputed inheritance, see Section 4. [2]Approximated by imputed tax payment, see Section 4. [3]The means have been calculated as yearly average over the given period.

Table 3 illustrates the variation in inherited wealth generated by the repeal of the inheritance tax by reporting descriptive statistics on inheritances and the corresponding wealth shocks for the treated ("treated") and the controls ("controls"). The upper panel displays the statistics for the *Main sample* whereas the bottom panel displays the statistics for the *Placebo sample*.

It can be noted that the difference in inheritance between the treated and the controls is small. This is reassuring, as it suggests that the wealth shock is exogenous.[26] A similar finding is noted for the *Placebo sample*. Regarding the wealth shock (approximated by the imputed tax payment, see Section 4) it is, by definition, zero for the controls and positive for the treated subjects in the *Main sample* and zero for both groups in the *Placebo sample*. The mean of the shock for treated subjects in the *Main sample* is SEK 70,817.[27]

For health outcomes which are observable over time, before and after the inheritance receipt (i.e. *Hospitalization*, *Diagnose*, and *Sick leave*), I will estimate the effect of the wealth shock by comparing the difference in incidences before and after the inheritance for the treated subjects with the similar difference for the controls. The last three columns in Table 3 report descriptive statistics necessary to calculate these difference-in-differences (DID) with respect to *Hospitalization* (i.e. the incidences in the pre- and post-periods, as well as the change in incidence over time (Post-Pre) for each group). It can be noticed that the pre-period incidences are similar across treated and controls. This indicates that the counterfactual identifying assumption of parallel trends in the absence of the shock is satisfied.[28] A comparison of the change in *Hospitalization* (Post-Pre) between the treated and the controls suggests that the wealth shock has a positive, but small, impact on the incidence, around 0.2 percentage points. The question is, however, whether or not we could interpret this impact as a causal effect?

To place this issue in perspective, one can compare the change in *Hospitalization* over time across the "treated" and the "controls" in the *Placebo sample*. In contrast with what we should expect to see given that both these groups were unaffected by the tax reform, the implied DID is positive and indicates that the reform leads to a 0.1 percentage points increase in the outcome.

One possible explanation for this finding is that the DID:s obtained from Table 3 only accounts for biases from common trends in the outcome, such a health responses surrounding the death of the parent or an increasing trend in health over time, and not for the fact that the time periods over which the differences are calculated correspond to different calendar years for heirs

[26] In Section 7, I confirm this further by showing that the treated and the controls are balanced in predetermined characteristics, including health.
[27] See Table C1 in Appendix C for the sample distribution of the wealth shock
[28] In Section 7, I present graphical evidence showing that the trajectories of *Hospitalization* for the treated and the controls evolve similarly in the pre-inheritance period.

inheriting before and after the tax reform. This may be an issue, due to the fact that recent studies show that health tends to respond to temporary fluctuations in the economy (Ruhm, 2000; Adda et al., 2009; Gerdtham and Johannesson, 2005). The impact and severity of aggregate seasonal health shocks, such as the flu or the winter vomiting disease, may also differ between years. Although the influence of year-specific events is partly mitigated by using the average incidences for the pre- and post-periods, one may still be concerned by the possibility that the response in the outcome is the result of an adverse event taking place in the years surrounding the reform or events in a year in the beginning or in the end of the sample period, rather than the wealth shock. For instance, if something adversely impacts the health of the treatment group in the last (calendar) year of the sample period, we may wrongly conclude that a difference in health across the two groups is the consequence of the wealth shock. Likewise, an adverse event in 2004 would be picked up as a pre-period effect for the treatment group and as a post-period effect for the controls, implying that we may overestimate (underestimate) a positive (negative) effect of the wealth shock.

My strategy to account for this source of bias is to estimate panel data models with cohort, time and year effects of the following form:

$$(1) \qquad H_{i,j,t,z} = \lambda_j + \lambda_t + \lambda_z + \phi D_i 1[j = 2005: t \geq 0] + \varepsilon_{i,j,t,z},$$

where $H_{i,j,t,z}$ is outcome of individual *i*, of inheritance cohort *j* (j=2003, 2004, 2005) at time *t*, in year *z*.[29] λ_j, λ_t and λ_z are cohort, time and year fixed effects, respectively. D_i is an indicator variable which takes the value one (=1) from the year of the inheritance (t=0) and onwards for individuals whose parents died after the tax reform (j=2005), and zero (=0) in all years for individuals whose parents died in the years before the reform (j=2003, 2004), and $\varepsilon_{i,j,t,z}$ is an idiosyncratic error. The coefficient ϕ is the DID estimator which captures the average effect of the wealth shock over the years following the inheritance.

The fact that the heir has to be alive at the time of the inheritance to be included in the sample means that Model 1 cannot be employed to estimate the effect of the wealth shock on *Mortality*. Instead, I estimate the wealth effect by comparing the difference in the likelihood of mortality between treated and controls in the *Main sample* with the similar difference for heirs in the *Placebo sample*. This alternative difference-in-differences strategy will account for biases from time-invariant differences between the treated and the controls under the assumption that environmental conditions (i.e. aggregate health shocks) during life, before the inheritance, have similar impacts on mortality rates for offspring receiving inheritance above and

[29] Here, cohort j=2005 includes the offspring who inherit over the period December 17-30, 2004.

below the tax threshold.[30] Likewise, it will account for differential annual trends in mortality under the assumption that external exposures over the period after the inheritance have similar influences on mortality for heirs receiving inheritance above and below the tax threshold.

6 Exogeneity of the wealth shock and test of identifying assumptions

In this section, I present two informal tests of the identifying assumptions underlying the empirical strategy. The first test, which looks for differences in predetermined characteristics between the treated and the controls, suggests that the wealth shock is exogenous. The second test compares the dynamics of *Hospitalization* over the sample period between the treated and controls. Reassuringly, the trajectories evolve similarly in the pre-inheritance period, suggesting that the parallel trends assumption is satisfied. Taken together, the tests imply that any difference in health following the inheritance could reasonably be attributed to the wealth shock.

6.1 Test for differences in pre-determined characteristics between treated and controls

Table 4 compares the sample means across the treated and the controls along a number of different predetermined demographic and economic characteristics which are likely to be related with health. The first two columns report the sample means for the treated and the controls, respectively, and the last column (3) reports the p-values from *t*-tests of the difference in means between the groups.

As indicated in Section 5, the treated and the controls, by construction, inherit in different calendar years (2005 vs. 2003 and 2004). A direct consequence of this sample design is that the treatment group contains heirs of younger birth-cohorts than the control group, as indicated by difference in mean birth year between the two groups. What consequences do this have for other observable characteristics? It can be seen from Column 3 that there are no statistically significant differences ($p>0.10$) in the fraction women, fraction Swedish citizen, fraction with children in the household, fraction with lower secondary education, earned income, or net worth across the

[30] This difference-in-difference strategy is similar in spirit to that used by Snyder and Evans (2006), who estimate the effect of income on mortality by comparing mortality rates for men born in the first quarter of 1917 (the Notch generation) with mortality rates for men born in the fourth quarter of 1916, using women from the same two birth quarters as controls.

treated and the controls.[31] The differences in observed characteristics that do exist are in age, fraction married, fraction with primary education, and fraction with upper secondary or post graduate education. Although these differences are statistically significant (p<0.10) they are quantitatively small and can easily be explained by the disparity in birth-year between the two groups. It is generally acknowledged that younger cohorts tend to have higher education, be married to a lower degree, and receive inheritance later in life, than older cohorts. The econometrical model presented in Section 5 includes inheritance-cohort fixed effects, which should account for any unobserved heterogeneity related with birth-cohort across the groups.

In Table D1, Appendix D, I present similar descriptive statistics for the *Placebo sample*. The differences in sample means between heirs inheriting before and after the reform are comparable to the corresponding differences for the *Main sample*, again suggesting that unobservable (inheritance) cohort specific factors have not manifested into persistent differences in observable characteristics related with health.

Table 4: Comparison of sample means, predetermined demographic and socioeconomic characteristics, treated and controls, *Main sample*.

	Treated	Controls	p-value 1-2
	1	2	3
Birth-year	1951.4	1950.1	0.000
Age when inheriting	53.5	53.4	0.054
Woman	49.3	49.8	0.246
Swedish citizen	99.6	99.6	0.575
Married	55.9	57.3	0.000
Children in household[1]	38.3	38.6	0.346
Level of education[2]:			
Primary	18.1	19.1	0.001
Lower secondary	42.6	42.6	0.939
Upper secondary or post graduate	35.6	34.9	0.031
Earned income[3]	274,891	274,062	0.577
Net worth[4]	905,871	899,235	0.884
Number of obs.	28,827	50,950	

Notes. Characteristics other than *Birth-year, Age, Earned income* and *Net worth* are measured three years before the inheritance receipt. Indicator variables are reported in percent. [1]Refers to children younger than 18. [2]Highest achieved level of education. [3]The means are calculated on annual incomes (adjusted for the growth in nominal income, base year 2004) averaged over the available pre-inheritance years.[4]The means are calculated on annual net worth (adjusted to 2004 price level using CPI) averaged over the available pre-inheritance years.

[31] The means with respect to *Earned income* and *Net worth* have been calculated on the annual averages for the available pre-inheritance years to limit the influence of differential macroeconomic exposures. Moreover, *Earned income* is adjusted for nominal wage growth, in the Government sector (base year 2004) and *Net worth* is adjusted for inflation using CPI (base year 2004).

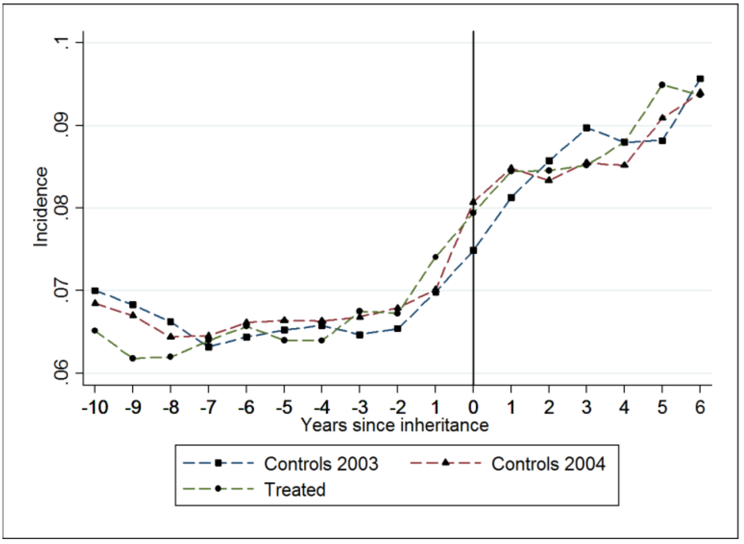
6.2 Test for parallel trends in health

Figure 1 displays the dynamics of *Hospitalization* over the sample period for the treated and the controls. Regarding the controls, I have separated between the heirs with respect to year of inheritance (2003 and 2004). The reason behind this division, rather than representing the dynamics for the controls with only one trajectory, is that it conforms better to Model 1, which includes controls for inheritance-cohort. It should, however, be emphasized that the graphs display the unconditional sample means by time period and hence, do not account for the fact that the periods corresponds to different calendar years for the treated and the controls.

The general pattern indicates that the incidence of *Hospitalization* is rather stable in the beginning of the sample period, increases sharply around two years before the inheritance and continues to do so thereafter. The increasing trend is expected given that the heirs become older. The sharp rise surrounding the parent's death (vertical line) may reflect increased illness related to mourning and psychological distress (Scharlach, 1991; Umberson and Chen, 1994; Kessler, 1997; Marks et al., 2007; Rostila and Saarela, 2001). Regarding the trajectories of the treated subjects and the two control cohorts, these display similar trends in the pre-inheritance period, suggesting that cohort specific influences have not manifested in persistent differences in health. The small differences in incidence between the groups that do exist could partly be explained by the fact that the years reported on the horizontal axis correspond to different calendar years for the groups, and these will be accounted for by the year controls in Model 1. The results nevertheless suggest that the parallel trends assumption is satisfied. It is, however, difficult to get an indication of whether or not the wealth shock has any effect on *Hospitalization* by comparing the trajectories over the post-inheritance years. If anything, the trajectory of the treated subjects appears to increase somewhat more sharply than those of the two control cohorts, but, as previously noted, one should be careful when interpreting this as causal effect since differential year trends are unaccounted for.

In Figure D1 in Appendix D, I report similar graphs for the "treated" and the "controls" in the *Placebo sample*. The parallel trends assumption appears to be satisfied for this sample as well. Moreover, a comparison of Figure 1 with Figure D1 suggests that the heirs receiving inheritances above and below the tax threshold experience similar health dynamics, although the incidences differ somewhat in levels. To the extent that trends in mortality are similar to trends in *Hospitalization*, this finding could be seen as supporting the identifying assumption underlying the estimation of wealth effects on mortality.

Figure 1: The annual incidence of *Hospitalization* for treated and controls, *Main sample*.



Note. The vertical line indicates the point in time when inheritance is received. *Controls 2004* does not include offspring receiving inheritance from a parent over the period December 17-31.

7 Results

In this section, the empirical results are presented. The first sub-section details the results with respect to the effect of the wealth shock on *Hospitalization*. It shows that the wealth shock increases the likelihood of hospitalization for any cause by five percent. The causal interpretation of the estimate is confirmed using a placebo test. The effect is more pronounced for women, the elderly and individuals with low education. In Section 7.1.1, I show that a non-trivial share of the effect in *Hospitalization* could be attributed to higher incidences of signs and symptoms of disease and cancer. Section 7.2 reports that the wealth shock does not have any effect on *Sick leave* or on *Mortality*.

7.1 The effect of the wealth shock on Hospitalization

The DID estimates in this section have been obtained from versions of Model 1 estimated using OLS. Given that *Hospitalization* is binary, the estimates should be interpreted as the percentage point difference in the probability of the outcome between the treated and the controls. In connection to the regression estimates, I report the mean of the dependent variable, in percent, for the post-inheritance period for the relevant control group (in brackets). Dividing the DID estimate by this statistic gives the percentage difference in incidence between the treated and the controls. In each specification, the standard errors have been clustered at the individual level to account for correlation within the individual over time.

Column 1 in Table 5 reports the DID estimate obtained from Model 1 without year controls. This is comparable to the naïve DID estimate implied by the statistics in Table 3. As expected, the estimate implies that the treated subjects have 0.2 percentage point higher probability of being hospitalized in the pre-inheritance period relative to the controls. Column 2 reports the DID estimate from Model 1 with year controls. The estimate, similarly to the estimate in Column1, is positive and statistically significant ($p<0.05$) but, notably, almost twice as large. The discrepancy suggests that the treated and the controls experience differential year trends and that year controls indeed are essential. Regarding the size of the effect, it suggests that the wealth shock leads to a five percent increase in the probability of *Hospitalization*.[32]

Table 5: Difference-in-differences (DID) estimates, impact of wealth shock on *Hospitalization* (in percent), *Main sample* and *Placebo sample*.

	Main sample		Placebo sample	
	1	2	3	4
DID estimate :	0. 222**	0.432**	0.201**	-0.144
	(0. 111)	(0.218)	(0.091)	(0.179)
	[8.63]	[8.63]	[10.09]	[10.09]
Year FE	No	Yes	No	Yes
N	79,802	79,802	134,172	134,172
N*T	1,356,634	1,356,634	2,280,924	2,280,924

Notes. Coefficient estimates are reported in percent. Standard errors (in percent) clustered at individual, in parentheses. Mean of dependent variable (in percent), post-inheritance period for control group, in brackets. * significant at the 10 percent level, ** significant at the 5 percent level, *** significant at the 1 percent level.

Is this a large or a small effect? To gain perspective on this issue, I compute the cross-sectional relationship between age (in years) and *Hospitalization*, as it is well-known that age has a large impact on health. It

[32] Since it was decided retroactively that inheritances received during the period December 17-31, 2004 would be exempted from taxation, it may be a source of concern that the anticipatory effects of heirs inheriting during this period are different to those of other heirs inheriting after the reform. Reassuringly, however, the DID estimate is unchanged when I estimate the model on a sample without these individuals. Moreover, recent studies have documented that people may postpone their death to save taxes, see Eliason and Ohlsson (2013) and Kopczuk and Slemrod (2005). Regarding the current reform, Eliason and Ohlsson show that deceased with taxable estates were more likely to have passed away on January 1, 2005, from when the tax was (supposed to be) repealed, rather than on December 31, 2004, compared to deceased without taxable estates. To account for the possibility that individuals whose parents died during the days surrounding the reform are systematically different from other heirs, I have redone the main analyses omitting heirs of parents who died over a period of up to two weeks following the reform. Reassuringly, the results from this exercise are similar to the main results.

turns out that the effect of the wealth shock equals the impact of being about four years older, suggesting that the wealth effect is non-trivial. However, when I relate the wealth effect to the impact of education, another factor related with health status (Lleras-Muney, 2005; Cutler and Lleras-Muney, 2010), I find that having primary or lower secondary education, as compared to upper secondary or post graduate education (i.e. the impact of having lower education) increases the probability of *Hospitalization* by 18 percent. This would suggest that the effect of a seven percent increase in wealth should be considered relatively limited.[33]

To establish that the estimate in Column 2 represents a causal relationship, and not just a spurious correlation, I estimate Model 1 on the *Placebo sample* (see columns 3 and 4 for results). An insignificant response in the outcome, or at least a DID estimate which is smaller in magnitude than the corresponding estimate for the *Main sample*, should be considered a validation of the casual interpretation of the main estimate. In accordance with what the statistics in Table 3 suggested, the DID estimate from Model 1 without year controls is similar to the corresponding estimate for the *Main sample*. However, in contrast to the corresponding main estimate, the estimate from the model with year controls (Column 4) is negative and statistically insignificant. This finding could be seen as lending additional support for the full version of Model 1. Taking the difference between the estimates in columns 2 and 4, as in a triple-difference estimator, suggests that, if anything, the wealth effect is underestimated.

I continue by testing for how the wealth effect varies with demographic characteristics. The first dimension I consider is age. The results, displayed in Table E1 in Appendix E, show that the effect is markedly higher for old heirs (above mean age) than for young heirs (below mean age). This finding corresponds with previous studies (e.g. Lindahl, 2005). I also test for responses for the working age population (16-65) over a period of four years following the inheritance. This is to obtain results which are comparable to those with respect to *Sick leave*. The DID estimate from this exercise (Column 3) is of the same order of magnitude as that for young heirs, and here too, statistically insignificant. Moreover, I find that the effect is primarily driven by women and not by men (see columns 3 and 4 in Table E1).[34] The results in columns 5 and 6 in Table E1 show that the DID estimate is positive and statistically significant for heirs with low education (primary or lower secondary education) and negative and statistically insignificant for heirs with high education (upper secondary or post graduate education). This

[33] One explanation for this is that education has been obtained early in life, and hence that its effect has had more time to accumulate into health.

[34] My result shows that women have a higher probability, relative to men, of being hospitalized in the pre- and post-inheritance periods. This is consistent with previous research on gender differences in health (see for example Case and Paxson, 2005).

could possibly suggest that highly educated individuals have more knowledge about, and are better at avoiding and managing, harmful health effects than their peers with lower levels of education (Goldman and Smith 2002). Models of health production, as well as previous empirical results, suggest that the wealth effect should be increasing relative to the size of the shock. To explore this in more detail, I estimate Model 1 separately for heirs receiving inheritance within the first, second, third and fourth quartile of the sample distribution. The results from this exercise are reported in Table E2 in Appendix E and suggest that the effect is quantitatively similar across the subsamples. The coefficient estimates, however, are imprecisely measured, which is probably a consequence of small sample sizes.

The results in Table 5 give us no sense of the dynamics of the wealth effect - whether it accelerates or stabilizes. To explore these dynamics, I estimate Model 1 with leads and lags of treatment. More specifically, I include interactions between the treatment indicator and time dummies for each of the ten years before the inheritance, the year of the receipt and for each of the six subsequent years. The results, which are reported in Column 1, Table E3 in Appendix E, show that the coefficient estimates on the lead indicators are statistically insignificant. This is comforting, as it suggests that the parallel trends assumption indeed is satisfied. As for the pattern of the lag structure, it shows that the difference in probability of *Hospitalization* between the treated and the controls increases sharply at the time of the inheritance receipt. This should be taken as additional support for the causal interpretation of the wealth effect. It should be noted, however, that the implied effect varies across the years and that it is only statistically significant for the second and fifth years after the inheritance. The lead and lag estimates obtained from the *Placebo sample* (Column 2) are quantitatively similar to each other (implying that the tax reform has no impact), do in general have the opposite sign to those for the *Main sample* and are, except for one lead indicator, statistically insignificant.

Taken together, it is evident that the wealth shock causes an increase in *Hospitalization.* At a first blush this finding suggests that increased wealth has detrimental effects on health. It should, however, be remembered that *Hospitalization* does not inform us about the reasons for the hospital admission. To place this issue in perspective, I therefore continue and test for heterogeneous response across the diagnoses reported in connection with the hospital admissions.

7.1.1 Explaining the wealth effect on Hospitalization

In this section, I report regression results for the effect of the wealth shock on the diagnose indicators detailed in Section 4.

Column 1 in Table 6 displays the DID estimates obtained from Model 1 (with year effects) estimated on the *Main sample.* It is noticeable that there are only two outcomes for which the DID estimate is statistically significant: *Neoplasms* and *Symptoms and signs*. The estimate with respect to *Neoplasms* implies that the wealth shock causes a 12 percent increase in the probability of the outcome, whereas for *Symptoms and signs*, the coefficient implies an increase of 11 percent. Taken together, they explain around 60 percent of the effect in *Hospitalization*. The fact that there is no significant response in any other variable (neither in the single diagnose variables nor in the variable *Others*) suggests that the wealth effect on *Hospitalization* is operating solely through *Symptoms and signs* and *Neoplasms.* Moreover, the corresponding estimates for the *Placebo sample* (see Column 2) are statistically insignificant, suggesting that the main estimates are causal.

What do the responses in these variables tell us about the mechanisms through which wealth affects *Hospitalization*?

The variable *Symptoms and signs*, as the name indicates, captures symptoms and signs of disease (e.g. irregular heart rate, shortness of breath, fever, senility, general feeling of illness, etc.) as well as unusual findings during medical examinations (e.g. blood and urine samples).[35] Given that the condition has resulted in a hospital admission, the response in the variable may, on the one hand, imply that the wealth shock leads to worse health, and potentially more so had we investigated the effects over a longer period of time.[36] On the other hand, the response could be interpreted as if the shock has made people more prone to seek care for health irregularities, possibly to reduce the likelihood of more severe conditions in the future. This is in line with previous studies which document that economic circumstances are positively associated with disease prevention (see for example Cawley and Ruhm, 2012)

Regarding *Neoplasm*, it contains diagnoses of cancers at different stages of development (i.e. benign, potentially malignant, and malignant tumors).[37] It is difficult to give an analytical explanation for why the wealth shock causes an increase in the likelihood of cancer, especially since it is commonly considered an equal opportunity disease (Smith, 2004). Although lifestyle factors such as smoking and drinking, which are reported to be positively related with improved wealth (Apouye and Clark, 2013; Kim and Ruhm, 2012), are linked to many types of cancers (e.g. lung, head and neck, pancreatic, liver, colon, gastric, etc., see for example Kushi et al., 2012) it seems unlikely that an increase in these factors would manifest into higher

[35] The results are similar when I exclude diagnoses due to abnormal clinical and laboratory findings (ICD-10: R70-R99 and ICD-9: 790-799).
[36] Minor medical problems generally heighten the odds of experiencing more severe health problems. This is the so-called progressive nature of disease (Smith, 2005).
[37] I have analyzed the effect on the wealth shock on cancerous tumors (malignant) and other tumors (benign and potentially malignant) separately, but the estimates are imprecisely measured, probably as a result of not enough variation (i.e. not enough non-zero observations) in the outcomes.

cancer incidence within a period of only six years. If the wealth shock has caused people to smoke and drink more we would rather expect to find responses in diagnoses which are more immediately related to these behaviors, such as injuries (e.g. alcohol poisoning), mental and behavioral disorders, diseases in the digestive system (e.g. liver cirrhosis), respiratory diseases (e.g. chronic obstructive lung disease) and circulatory diseases (e.g. coronary heart disease and stroke) (WHO 2002). Moreover, previous studies report that improved wealth leads to reduced obesity (Lindahl, 2005; Kim and Ruhm, 2012) and improved mental well-being (Gardner and Oswald, 2007; Apouye and Clark, 2013). But, if the wealth shock exploited here has led to reduced obesity or improved mental well-being we should expect to find, if anything, a *reduction* in cancer incidence, not an increase (Kushi et al., 2012; Chida and Steptoe, 2008).

A more realistic explanation for the positive response in *Neoplasm* is therefore that the wealth shock has led to more health care visits in general, as indicated by the results with respect to *Symptoms and sings*, and that cancer, which otherwise would have been diagnosed later, is detected and possibly treated earlier.

In sum, the results in this section suggest that the higher incidence of *Hospitalization* does not necessarily mean that the wealth shock has detrimental effects on health, but rather that it leads to more preventative actions against future morbidity.

Table 6: Difference-in-differences (DID) estimates, impact of wealth shock on diagnose categories (in percent), *Main sample* and *Placebo sample*.

	Main sample	Placebo sample
	1	2
Outcome:		
Neoplasms	0. 13*	-0.04
	(0.08)	(0.06)
	[1.13]	[1.26]
Mental	-0.05	-0.07
	(0.07)	(0.06)
	[0.64]	[0.75]
Nervous	-0. 03	-0.001
	(0.05)	(0.04)
	[0.40]	[0.47]
Circulatory	0.03	-0.09
	(0.09)	(0.08)
	[1.55]	[1.94]
Respiratory	0.04	-0.02
	(0.05)	(0.04)
	[0.48]	[0.62]
Digestive	0.02	-0.001
	(0.07)	(0.06)
	[0.98]	[1.18]
Musculoskeletal	0.001	-0.08
	(0.07)	(0.05)
	[0.88]	[1.12]
Genitourinary	0.05	0.02
	(0.06)	(0.04)
	[0.61]	[0.72]
Symptoms and signs	0.14*	0.08
	(0.07)	(0.06)
	[1.09]	[1.49]
Injury	0.06	0.03
	(0.07)	(0.05)
	[0.10]	[1.15]
Others	0.05	0.08
	(0.08)	(0.07)
	[1.46]	[1.62]
Year FE	Yes	Yes
N	79,802	134,172
N*T	1,356,634	2,280,924

Notes. Coefficient estimates are reported in percent. Standard errors (in percent) clustered at individual, in parentheses. Mean of dependent variable (in percent), post-inheritance period for control group, in brackets. * significant at the 10 percent level.

7.2 The effect of the wealth shock on Sick leave and Mortality

In this section, I complement the previous analyses by investigating responses in outcomes capturing health events that are both less and more severe than those resulting in hospital admissions. More specifically, I estimate the causal effect of the wealth shock on *Sick leave* (less severe) and *Mortality* (more severe).

Table F1 in Appendix F reports the DID estimates with respect to *Sick leave*. These have been obtained from Model 1, estimated on the working aged population over a period of ten years before and four years after the inheritance receipt. A comparison of the estimates from the model with and without year controls indicates that the treated and the controls experience differential year trends in the variable. This is in line with what I found for *Hospitalization*. Here, however, the DID estimate from the preferred specification of Model 1 (Column 2) is statistically insignificant, implying that the wealth shock does not have any evident effect on the likelihood of sick leave. The DID estimates for the *Placebo sample* (columns 3 and 4) are similar in terms of sign and statistical significance to the corresponding estimates for the *Main sample*, but the implied responses are quantitatively smaller. It should be noticed that I cannot rule out the possibility that the wealth shock has consequences for health events captured by *Sick leave* for heirs who are younger than 16 and older than 65. However, the fact that the wealth effect with respect to both *Sick leave* and *Hospitalization* are statistically insignificant for the working-age population lends additional support to the conclusion that the wealth shock generated by the tax repeal has no detectable consequences for health.

The causal effect of the wealth shock on mortality is estimated by comparing the difference in the probability of dying over the post-inheritance period between treated and controls in the *Main sample* with the similar difference for the *Placebo sample*.[38] The regression results with respect to each of the six mortality indicators (i.e. *Mortality1,..., Mortality6*) are presented in Table F2, Appendix F.[39] Neither the differences estimates (for any of the two samples) nor any of the DID estimates (which accounts for biases from time-invariant differences and year trends), are statistically significant at conventional levels. These results suggest that the wealth shock has no detectable effect on mortality within any year over a period of six years after the inheritance receipt. The results in Section 7.1.1 suggested that

[38] The analysis is based on heirs inheriting in the year before (2004) and in the year after the reform (2005). This is to limit the potential influence of confounding secular trends in mortality. I have, however, redone the analysis on samples including offspring inheriting in 2003 and obtained largely similar results.

[39] The differences and the difference-in-differences are estimated with linear probability models. The models include controls for age, age^2, gender, marital status, presence of children, level of education, earned income and net worth, measured three years before the inheritance and aimed at accounting for any remaining unobserved heterogeneity.

cancer is detected earlier as a consequence of the wealth shock. It is evident however that this potentially preventative effect is not sufficient to have any effect on all-cause mortality, at least not over a period of six years. I have also tested explicitly for the impact of the wealth shock on the likelihood of cancer mortality within the six year period but the estimate of the wealth effect is imprecisely measured.

8 Concluding discussion

In this paper I exploit the exogenous variation in wealth induced by the unexpected repeal of the Swedish inheritance tax to test for the impact of increased wealth on health outcomes commonly found in administrative registers.

The empirical analysis shows that the favorable wealth shock resulting from the tax reform has limited consequences for objective health over a period of six years following the shock. This is in line with what has been documented previously regarding subjective health outcomes. If anything, it appears as if the wealth shock leads to more health care visits for minor health contingencies, which in turn result in that cancer is detected and possibly treated earlier. One possible explanation for this preventive response is that people feel that their future consumption prospects have improved and that good health is necessary for enjoying these benefits. Even if these findings suggest that increased wealth does not have any direct consequences for health they should be of interest to policy makers, since prevention, and especially early detection of chronic diseases like cancer, has been brought forward as one of the most valuable instruments to reduce health care costs (see for example Kenkel, 2000). Preferably, one would like to complement the analysis with data on outpatient care to say more about the wealth effect on total number of health care visits and also, to pinpoint when in time cancer is initially discovered. Data on outpatient care of sufficient quality for the sample period is, unfortunately, not available however.

Although the wealth shock exploited in this paper is received by people who have suffered the loss of a parent – and therefore may be unhealthier than the general population – the results generalize to people who are in their fifties, as the death of a parent commonly occurs at this stage of life. The fact that I can replicate the stylized facts concerning the cross-sectional relationship between wealth and health also suggests that the empirical results are not specific for the current sample.

It should be noted, however, that I cannot rule out the possibility that potential effects of the wealth shock take more than six years to manifest into health. From a policy perspective the results nevertheless seem particularly relevant, suggesting that wealth changes that might be expected from tax reforms of similar magnitudes as the repeal of the Swedish inheritance tax, affecting this age-group, are unlikely to have any short or medium run consequences for health. The results, moreover, suggest that policies targeted at reducing socioeconomic inequalities in health are likely to be more usefully channeled toward interventions that directly improve health.

Appendix A: Description of diagnose variables

Table A1: Diagnose variables, corresponding ICD chapters, and ICD codes (by version).

Variable	ICD chapter	ICD-10	ICD-9
Infections	I. Certain infectious and parasitic diseases	A00-B99	001-139
Neoplasms	**II. Neoplasms**	**C00-D49**	**140-239**
Blood	III Diseases of the blood and blood-forming organs and certain disorders involving the immune mechanism	D50-D89	280-289
Endocrine	IV. Endocrine, nutritional and metabolic diseases	E00-E89	240-279
Mental	**V. Mental and behavioral disorders**	**F01-F99**	**290-319**
Nervous	**VI. Diseases of the nervous system**	**G00-G99**	**320-389**
Eye	VII. Diseases of the eye and adnexa	H00-H59	360-379
Ear	VIII. Diseases of the ear and mastoid process	H60-H95	380-389
Circulatory	**IX. Diseases of the circulatory system**	**I00-I99**	**390-459**
Respiratory	**X. Diseases of the respiratory system**	**J00-J99**	**460-519**
Digestive	**XI. Diseases of the digestive system**	**K00-K94**	**520-579**
Skin	XII. Diseases of the skin and subcutaneous tissue	L00-L99	680-709
Musculoskeletal	**XIII. Diseases of the musculoskeletal system and connective tissue**	**M00-M99**	**710-739**
Genitourinary	**XIV. Diseases of the genitourinary system**	**N00-N99**	**580-629**
Pregnancy	XV. Pregnancy, childbirth and the puerperium	O00-O99	630-676
Perinatal	XVI. Certain conditions originating in the perinatal period	P00-P96	760-779
Congenital	XVII. Congenital malformations, deformations and chromosomal abnormalities	Q00-Q99	740-759
Symptoms	**XVIII. Symptoms, signs and abnormal clinical and laboratory findings, not elsewhere classified**	**R00-R99**	**780-799**
Injury	**XIX. Injury, poisoning and certain other consequences of external causes**	**S00-T88**	**800-999**
External	XX. External causes of morbidity	V00-Y99	E01-E99
Fatcors	XXI. Factors influencing health status and contact with health services	Z00-Z99	V01-V82

Appendix B: Cross-sectional evidence of the wealth-health gradient

This appendix shows that the dataset can reproduce the positive cross-sectional association between wealth and health documented in the previous literature.

Table B1 presents estimates from linear probability models with *Hospitalization* and *Sick leave* as dependent variables and wealth as explanatory variable. To account for the fact that the relationship between wealth and (good) health is documented to be concave (see for example Ettner 1996, Smith 1999, Benzeval and Judge 2001), I apply the inverse hyperbolic sine transformation to wealth (Burbidge et al., 1988). This transformation is preferred because, unlike the log transformation, it accommodates zeros values in the variable. To account for the fact that negative values are not accommodated by the inverse hyperbolic sine however, I use the individual's gross wealth (i.e. the sum of real and financial assets, at market prices) instead of net worth. The coefficient estimate on wealth can nevertheless be interpreted as a as a semi-elasticity. The models also include controls for: a second order polynomial in age, sex, marital status, presence of children, level of education, as these have been used in the previous literature, as well as controls for year of inheritance. The outcomes as well as the covariates are measured three years before the inheritance receipt to assure that they are exogenous with respect to the tax reform. Columns 1 and 2 report the result with respect to *Hospitalization* and *Sick leave* for the *Main sample*, whereas columns 3 and 4 report the corresponding results for the *Placebo sample*. Regarding the *Main sample* the coefficient estimate on wealth is statistically significant at the one percent level, indicating that higher wealth reduces the likelihood of hospital admission. Divided by the sample mean, the estimate implies that a one percent increase in wealth, all else equal, reduces the likelihood of *Hospitalization* by four percent. Similarly, the coefficient estimate on wealth from the specification with *Sick leave* as dependent variable is statistically significant ($p<0.01$). The estimate implies that a one percent increase in wealth reduces the probability of the outcome with two percent. The results for the *Placebo sample* display a similar pattern as those for the *Main sample*: the coefficient estimates on wealth are negative and statistically significant on conventional levels for both outcomes.

Table B1: Linear probability estimates (in percent) of the cross-sectional relationship between wealth and *Hospitalization* and wealth and *Sick leave, Main sample* and *Placebo sample.*

	Main sample		Placebo sample	
Outcome:	Hospitalization	Sick leave[a]	Hospitalization	Sick leave[a]
	1	2	3	4
$\sin^{-1}$ Wealth	-0.31***	-0.40***	-0.33***	-0.32***
	(0.02)	(0.03)	(0.01)	(0.02)
Mean of dep. var, in percent	6.93	15.67	8.23	18.36
N	76,949	69,936	129,921	111,754

Notes. Robust standard errors (in percent), in parentheses. * significant at the 10 percent level, ** significant at the 5 percent level, *** significant at the 1 percent level. The specifications include control variables, measured three years before the inheritance. These are: age, age^2, gender, marital status, presence of children, and level of education. The specifications also include controls for year of inheritance. [a] The specification has been estimated on the working aged population (16-65).

Appendix C: Sample distribution of wealth shock, treated subjects, Main sample

Table C1: Distribution of wealth shock, treated subjects, Main sample

Mean	p5	p10	p25	p50	p75	p90	p99	Sd	Count
70,817	1,176	2,533	7,323	20,046	50,930	150,676	769,817	358,073	28,827

Notes. Wealth shock is approximated by imputed inheritance tax payment, see Section 4 for description.

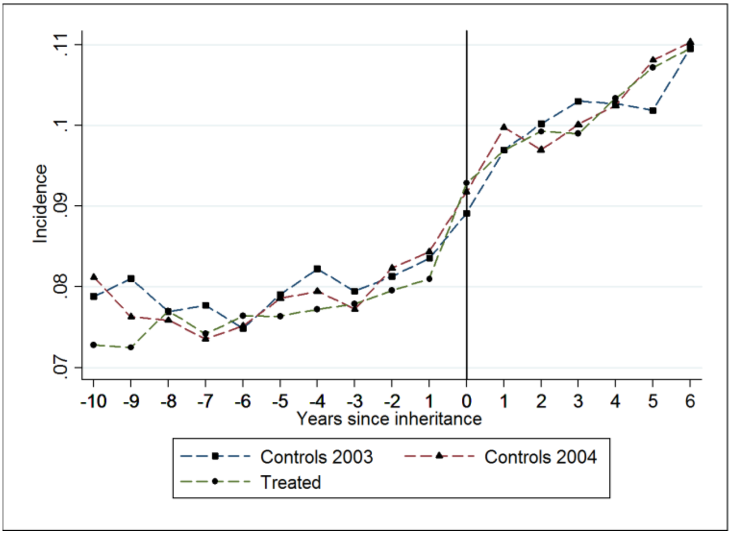
Appendix D: Sample characteristics, Placebo sample.

Table D1: Comparison of sample means, predetermined demographic and socioeconomic characteristics, "treated" and "controls", *Placebo sample*.

	"Treated"	"Controls"	p-value 1-2
	1	2	IV
Birth-year	1950.2	1948.8	0.000
Age when inheriting	54.8	54.7	0.234
Woman	49.9	49.7	0.423
Swedish citizen	99.4	99.4	0.330
Married	56.2	58.1	0.000
Children in household[1]	32.8	33.5	0.018
Level of education[2]:			
Primary	27.8	30.0	0.001
Lower secondary	45.9	45.2	0.008
Upper secondary or post graduate	22.8	21.6	0.031
Earned income[3]	242,410	240,805	0.062
Net worth[4]	488,363	470,437	0.024
Number of obs.	48,165	85,970	

Notes. Characteristics other than *Birth-year*, *Age*, *Earned income* and *Net worth* are measured three years before the inheritance receipt. Indicator variables are reported in percent. [1]Refers to children younger than 18. [2]Highest achieved level of education. [3]The means are calculated on annual incomes (adjusted for the growth in nominal income, base year 2004) averaged over the available pre-inheritance years.[4]The means are calculated on annual net worth (adjusted to 2004 price level using CPI) averaged over the available pre-inheritance years.

Figure D1: The annual incidence of *Hospitalization* for "treated" and "controls", *Placebo sample*.



Note. The vertical line indicates the point in time when inheritance is received. *Controls 2004* does not include offspring receiving inheritance from a parent over the period December 17-31.

Appendix E: DID estimates, heterogeneous effects, Hospitalization

Table E1: Difference-in-difference estimates, impact of wealth shock on *Hospitalization* (in percent), heterogeneous effects with respect to demographic characteristics, *Main sample*.

	Age			Sex		Education	
	Young, < mean age	Old, > mean age	16-65	Women	Men	< Upper secondary or post graduate	Upper secondary or post graduate
	1	2	3	4	5	6	7
DID estimate:	0.234	0.668**	0.275	0.621**	0.236	0.751***	-0.168
	(0.309)	(0.301)	(0.255)	(0.312)	(0.305)	(0.279)	(0.345)
	[6.68]	[10.12]	[7.58]	[8.66]	[8.59]	[9.07]	[7.79]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	34,465	45,336	62,514	39,577	40,224	51,758	28,043
N*T	585,905	770,712	937,710	672,809	683,808	879,886	476,731

Notes. Coefficient estimates are reported in percent. Standard errors (in percent) clustered at individual, in parentheses. Mean of dependent variable (in percent), post-inheritance period for control group, in brackets. ** significant at the 5 percent level, *** significant at the 1 percent level.

Table E2: Difference-in-difference estimates, impact of wealth shock on *Hospitalization* (in percent), heterogeneous effects with respect to wealth shock, per quartile of the distribution, *Main sample*.

	Wealth shock, by quartile of the distribution:			
	1st	2nd	3rd	4th
	1	2	3	4
DID estimate:	0.333	0.608	0.559	0.251
	(0.431)	(0.459)	(0.438)	(0.416)
	[9.14]	[8.81]	[8.48]	[8.07]
Year FE	Yes	Yes	Yes	Yes
N	19,949	19,950	19,951	19,951
N*T	339,133	339,150	339,167	339,167

Notes. Coefficient estimates are reported in percent. Standard errors (in percent) clustered at individual, in parentheses. Mean of dependent variable (in percent), post-inheritance period for control group, in brackets.

Table E3: Difference-in-difference estimates, impact of wealth shock on *Hospitalization* (in percent), dynamics of responses, *Main sample* and *Placebo sample.*

	Main sample	Placebo sample
	1	2
DID estimate by year since inheritance:		
-8	0.0167	0.275
	(0.314)	(0.255)
-7	-0.001	-0.302
	(0.346)	(0.286)
-6	-0.001	-0.098
	(0.352)	(0.292)
-5	-0.0136	-0.334
	(0.355)	(0.293)
-4	-0.115	-0.303
	(0.355)	(0.296)
-3	0.144	-0.245
	(0.357)	(0.299)
-2	0.154	-0.420
	(0.359)	(0.305)
-1	0.205	-0.687**
	(0.373)	(0.308)
0	-0.199	-0.455
	(0. 386)	(0. 316)
1	0.535	-0.200
	(0.393)	(0.322)
2	0.891**	0.258
	(0.396)	(0.324)
3	0.600	-0.380
	(0.398)	(0.327)
4	0.424	-0.632
	(0.404)	(0.670)
5	0.865**	-0.312
	(0.409)	(0.338)
6	0.320	-0.636
	(0.884)	(0.708)
Year FE	Yes	Yes
N	79,802	134,172
N*T	1,356,634	2,280,924

Notes. Coefficient estimates are reported in percent. Standard errors (in percent) clustered at individual, in parentheses. Mean of dependent variable (in percent), post-inheritance period for control group, in brackets. ** significant at the 5 percent level.

Appendix F: DID estimates of the effect of the wealth shock on Sick leave and Mortality

Table F1: Difference-in-differences (DID) estimates, impact of wealth shock on *Sick leave* (in percent), *Main sample* and *Placebo sample*.

	Main sample		Placebo sample	
	1	2	3	4
DID estimate :	-1.12***	0.311	-1.44***	0. 211
	(0. 201)	(0. 386)	(0. 174)	(0. 327)
	[13.96]	[13.96]	[15.62]	[15.62]
Year FE	No	Yes	No	Yes
N	61,584	61,584	93,961	93,961
N*T	911,750	911,750	1,394,978	1,394,978

Notes. Coefficient estimates are reported in percent. Standard errors (in percent) clustered at individual, in parentheses. Mean of dependent variable (in percent), post-inheritance period for control group, in brackets. *** significant at the 1 percent level.

Table F2: Differences estimates and Difference-in-differences (DID) estimates, impact of wealth shock on mortality (in percent).

	Differences estimates:		DID estimates:
	Main sample	Placebo sample	1-2
	1	2	3
Outcome:			
Mortality1	-0.06	-0.01	-0.06
	(0.08)	(0.06)	(0.10)
	[0.78]	[0.92]	
Mortality2	-0.05	0.06	-0.11
	(0.10)	(0.08)	(0.13)
	[1.33]	[1.50]	
Mortality3	-0.06	0.10	-0.16
	(0.12)	(0.10)	(0.15)
	[1.85]	[2.16]	
Mortality4	-0.06	0.08	-0.14
	(0.13)	(0.11)	(0.17)
	[2.39]	[2.84]	
Mortality5	-0.10	0.13	-0.23
	(0.15)	(0.13)	(0.20)
	[3.04]	[3.62]	
Mortality6	-0.06	0.04	-0.10
	(0.16)	(0.14)	(0.21)
	[3.66]	[4.49]	
N	51,835	86,733	138,568

Notes. Coefficient estimates are reported in percent. Robust standard errors (in percent) in parentheses. Mean of dependent variable (in percent), for control group in brackets. The estimates have been obtained from models with controls for: age, age^2, gender, marital status, presence of children, level of education (highest achieved), earned income and net worth, measured three years before the inheritance.

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