Wealth Effects on Labor Supply: Evidence From a Swedish Tax Reform*

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November 2019

Abstract

We exploit a Swedish tax reform to obtain quasi-experimental, tax-induced variation in wealth which allows us to identify the effect of wealth on labor supply. The reform that we study abolished the inheritance tax in Sweden; the random timing of death around the reform leads to exogenous variation in the net inheritance received by heirs. Using register data covering the full Swedish population of children heirs, we find that a 1 SEK shock to wealth reduces annual earnings in the near term by about 0.016 SEK – a magnitude which is in line with previous estimates of wealth effects – though the estimate is somewhat imprecise and sensitive to removing outliers. To our knowledge, ours is the first study to demonstrate effects on labor supply from plausibly random, tax-induced variation in wealth.

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1 Introduction

Many economic policy issues involve lump-sum payments in one form or another. A prominent example in recent years is the Universal Basic Income (UBI), which has been proposed and debated in many countries around the world. Poll taxes, which tax every individual by a fixed amount, are in some sense the opposite of a UBI, and have existed since the Roman Empire. Recent work (Saez & Zucman, 2019) argues that healthcare premiums in the United States should be considered a poll tax as well. And of course, inheritance taxes are also lump-sum taxes from the point of view of heirs, who do not have any influence over how much they inherit and have to pay in taxes.

In all of these cases, understanding agents' behavioral response to the lump sum tax or transfer is crucial for policy evaluation – for example, a commonly cited concern with the UBI is the risk that it will depress labor supply (Jones & Marinescu, 2018), which will matter in its own right, as well as due to its effect on the income tax base. Aside from these particular settings involving lump-sum policy instruments, income elasticities are also a key parameter when evaluating tax reforms more generally (Kleven, 2018).

In this study, we exploit the 2004 abolishment of the Swedish inheritance tax to estimate wealth effects on labor supply. This tax, which was progressive with a maximum marginal tax rate of 30%, was surprisingly and quickly abolished in late 2004, with effect starting in January 2005. The key to our research design is the variation in the wealth shock generated by this reform: Those individuals who inherit after the abolishment avoid the taxes they would have otherwise paid, which is a positive shock to their wealth compared to those who inherit earlier. We find observable differences between those who inherit before and after the abolishment to be small. Given that the two groups are determined exclusively by the random timing of their parents' death, there is no reason to believe that they differ in any unobservable ways either, and they are therefore ideal treatment and control groups. Using a triple-differences approach which exploits variation across time, size of the tax shock and treatment vs. control groups, we find statistically significant and negative effects of the wealth shock on labor supply: In our baseline specification, we estimate that an additional 1 SEK shock to wealth leads to decreased labor earnings of approximately 0.016 SEK in the short run, which is in line with previous estimates of wealth effects. The result, though statistically significant, is somewhat imprecise, and it is sensitive to removing extreme outliers from the sample. We therefore also estimate a specification in logs, which features precise effects and is very robust to dropping outliers, but for which the estimated coefficients do not have a natural interpretation.

To our knowledge, our study is the first to use quasi-experimental, tax-induced variation in wealth to estimate labor supply responses. A few studies (Imbens *et al.*, 2001; Cesarini *et al.*, 2017) have used quasi-experimental evidence on lottery winners to estimate the wealth effect on labor supply. While these studies are often seen as the current gold standard for evidence on wealth effects, they do have the drawback of being estimated only on a self-selected sample of lottery players. Furthermore, it is not entirely clear that the psychological effect of receiving a positive shock to wealth through winning the lottery generalizes to effects induced by the tax system. We thus view our work as being very relevant for policy analysis. Indeed, Kopczuk (2013), writing about taxation of intergenerational transfers states: "The literature on the effect of wealth shocks has provided evidence suggestive of negative effects on labor supply of donors, though much remains to be done. None of this work has provided evidence derived from tax variation so that the tax policy relevant effect [...] has not yet been carefully studied." With this study, we are able to provide such evidence. Similarly, Kindermann *et al.* (2018) note a lack of existing studies exploiting random variation in inheritances.

While being most closely related to the aforementioned quasi-experimental studies using lottery data, our study adds to a vast literature on labor supply which is impossible to summarize succinctly here. Subsets of this literature are surveyed by Hausman (1985), Congressional Budget Office (1996), Blundell & MaCurdy (1999) and Keane (2011). While most estimates of the income effect on labor supply in the literature point towards a fairly small effect, there is no clear consensus on this and little quasi-experimental evidence. The earlier empirical literature, such as Hausman (1981), would often use survey or census datasets to estimate income and substitution effects on labor supply. The identifying variation to estimate the income effect would come from variations in e.g. capital income, the spouse's income (in the case of married women), or the virtual income generated by kinks in the tax schedule, none of which can be safely assumed to be exogenous. More recently and specifically studying income effects, Jones & Marinescu (2018) use the synthetic control method to show that part-time employment increased following the introduction of a universal basic income in Alaska, while the effect on overall employment was negligible.

While the classic labor supply literature often uses self-reported hours worked as the outcome variable, it has been more common in the empirical literature to instead study taxable income responses, after Feldstein (1999) showed that the elasticity of taxable income (ETI) is a sufficient statistic for welfare effects of income tax reforms. Many papers in this literature, including Gruber & Saez (2002) and Kleven & Schultz (2014), estimate both substitution and income effects, the latter of which has generally been shown to be small. Like the ETI literature, we use income as our outcome variable rather than hours worked, which is not observable in our register data.[1]

Finally, our study is also related to a body of work that more specifically assesses the effects of inheritances on labor supply. Classic papers in this literature include Holtz-Eakin *et al.* (1993) and Joulfaian & Wilhelm (1994). The hypothesis that receiving an inheritance, especially a large one, leads to reduced labor supply, is often called the Carnegie Conjecture. While this may seem to be just a special case of the more general wealth effect on labor supply, there are certain reasons why we might not expect this to be the case: First, the death of a parent or another near relative may induce labor supply responses on its own due to e.g. emotional responses. Second, an inheritance cannot be considered to be a pure wealth shock, since people will often have a rough how much they will inherit, even if they cannot anticipate the timing or the exact amount. Many recent studies in this literature have controlled for either of these two effects through the choice of the research design. By identifying the labor responses from a sample of heirs who inherit different amounts, it is possible to control for emotional responses (e.g. Elinder *et al.*, 2012), but the results will not identify the pure wealth effect, since these groups of heirs may have expected to inherit different amounts and have therefore made different choices prior to inheriting, and may also have different unobserved characteristics related to their parents' wealth. Conversely, by comparing individuals who inherit their parents to similar individuals who have not yet inherited (e.g. Nekoei & Seim, 2019), one can control for the size of the expected wealth shock, but these estimates are still confounded by the potential grief effect. To our knowledge, our study is the first to exploit unanticipated, quasi-random variation in the amount inherited, allowing us to control for both of these effects at the same time and isolate the pure wealth effect.

[1] We study the effects of wealth shocks on labor earnings only, rather than total taxable income, for multiple reasons. First, as Feldstein (1999) showed, it is the *compensated* ETI which is a sufficient statistic for welfare effects of income tax reforms. This parameter can be identified from marginal tax rate variation alone without the need to separately estimate the income effect. Second, recent literature on income effects including Imbens *et al.* (2001) and Cesarini *et al.* (2017) focus mostly on labor supply.

Inheritance (SEK)	Marginal tax rate
0-70,000	0 percent
70,000-370,000	10 percent
370,000-670,000	20 percent
670,000+	30 percent

Table 1: The Swedish inheritance tax system prior to 2004. These brackets were in place for spouses and direct descendants. For other heirs, which we do not study in this paper, different schedules applied.

2 Institutional context

2.1 The Swedish inheritance tax prior to 2004

Prior to the tax reform that we study, Sweden imposed a tax on inheritances received by heirs and beneficiaries of wills. Unlike the estate tax used in countries like the US, where the tax is paid by the estate of the deceased, the Swedish tax was imposed on each individual heir separately. The tax scheme followed a progressive system with a base amount being fully deductible, followed by brackets with marginal tax rates of 10, 20 and 30 percent, respectively. The precise cutoffs for the brackets varied depending on the relationship between the decedent and the heir. However, most heirs were taxed according to the scheme given in table 1, which covered the decedent's children and their descendants.[2]

Along with the inheritance tax, Sweden also taxed gifts. In practice, gifts given by an individual less than 10 years prior to their death were considered an advance on the inheritance. When calculating the ultimate amount of inheritance tax due, these gifts would therefore be added to the amount inherited by the heir at the time of death, and the sum of these would be considered to be the taxable inheritance.

As a feature of the Swedish law on inheritance tax, heirs could choose to voluntarily relinquish their right to some or all of their inheritance, which would then pass on in equal shares to their own children. This was used as a legal means of tax avoidance, since each child would be taxed separately on the amount they received, again according to the scheme in table 1. Throughout our analysis, we study pre-relinquishment inheritances and implied tax liabilities. Although some heirs can avoid some or all of their tax liability by passing on inheritance, they are foregoing personal income to do so. Assigning a "tax shock" of zero to these individuals is thus not appropriate, since their income would have been larger, had the tax not existed.

[2]Other relatives and beneficiaries not related to the decedent were taxed according to schemes featuring the same marginal tax rates, but lower cutoffs for each of the brackets. These heirs are omitted from the sample we study.

In addition, prior to 2003, Sweden also imposed an inheritance tax on surviving spouses. This tax scheme also featured the same marginal tax rates as for other heirs, but with a substantially wider starting bracket with zero tax liability. The spousal inheritance tax was repealed in 2003 with effect from January 2004, i.e. a year prior to the main reform we study.[3]

2.2 The abolishment of the inheritance tax

On September 20th, 2014, the Social Democratic government of Sweden first announced its plans to abolish the inheritance and gift tax effective January 1st, 2005. Although the inheritance tax had been the subject of some debate and criticism for complicating the division of estates and the transfer of family-owned firms, the total abolishment nevertheless came as a major surprise to the general public (Silfverberg, 2005). Within a few months after the initial proposal, the repeal of the inheritance tax was ultimately passed in Parliament on December 16, 2004, with effect starting January 1st.

On December 26, 2004, the Asian Tsunami caused the death of 543 Swedes - the largest number of casualties from any western country in that disaster.[4] Out of consideration for the family members of those who died in the tsunami, the Swedish Parliament passed a law in April 2005 which retroactively abolished the inheritance also for those who had died December 17-31, 2004. In our study, we exclude all heirs of decedents who died during this period, for multiple reasons. First, those who died in the tsunami are likely not representative of the Swedish population in terms of wealth and age. Second, the dramatic circumstances of the tsunami may have caused particular grief among family members which could impact their behavioral response for reasons unrelated to the inheritance tax. And finally, the fact that the inheritance tax on these individuals was only removed months later could also have affected the behavioral responses of the heirs, who might have made decisions believing they would be paying the tax.

[3] The repeal of the spousal inheritance tax did create similar variation in wealth to the reform that we study. However, we abstract away from surviving spouses for two reasons: They are very likely to already be retired when their partner dies, which means they will not add much power to our study. Furthermore, it is likely that they consider all wealth to be shared within the household, such that inheriting their spouse doesn't represent a positive wealth shock to them. The inheritance tax, prior to 2003, would then be a pure negative wealth shock, which might cause very different behavioral responses than the variably sized positive wealth shocks that are the main focus of this study. We note that this might be an interesting topic for future research.

[4] Source: https://www.thelocal.se/20141222/the-wave-sweden-will-never-forget

3 Data

We construct a sample of heirs using register data from Statistics Sweden, which covers the entire Swedish population. For data on inheritances and the identities of the heirs, we use the Belinda database which covers all heirs of deceased Swedes in the years 2002-2005; a total of 960,000 individuals. In our analysis, we only study heirs who are children of the deceased, since we need to impute the inherited amount in some cases (as described below), and the division of the inheritance is much harder to predict when there are no children present. Note that among those who leave behind children, leaving aside any surviving spouse, they bequeath around 99% of wealth to their children (Elinder *et al.*, 2019). We exclude surviving spouses from our sample of heirs. We also restrict the sample to exclude any children whose parents are still married when the first parent dies, since only a partial estate division happens in these cases.[5] We impose the further restriction that the deceased individual must not have changed their marital status in the year prior to their death, in order to avoid problems when imputing the wealth at the time of death, as described below. We exclude individuals who died December 17-31, 2004, to counter the risk of any bias related to the retroactive abolishment of the inheritance tax over this period, as discussed in the previous section. Finally, we exclude any individuals who received multiple inheritances during the three years covered by the database, since this could lead to confounding effects. This leaves us with a study population of 364,618 individuals.

The register data allows us to observe income data for all heirs. Our outcome of interest is labor supply, which we proxy with labor income, as is common in the literature. Our main measure of labor income combines wage earnings and self-employment income, although we also show results separately for these two variables. When analyzing intensive and extensive margin responses, we treat an individual as having positive labor supply if their annual labor income exceeds SEK 38,600 (USD 4,000).[6]

3.1 Imputation of unobserved inheritances

For the years 2002-2004, prior to the inheritance tax repeal, the Belinda database contains register data on both the estate of the deceased, the amount inherited by each heir and the tax liability on

[5]Specifically, joint children of a married couple do not actually receive their inheritance until the last parent dies. However, if the parents are divorced or never married, the children will receive their inheritance from the first parent at the time of his or her death.

[6]This reflects the "prisbasbelopp" at the time, an amount used widely across different Swedish government agencies when setting income limits for e.g. eligibility for certain transfers.

said amount. However, for the vast majority of those who inherit after the repeal of the reform, neither the inherited amount nor the estate of the deceased are encoded in Belinda.[7] To address this issue, we impute the inherited amount using data from the Swedish Wealth Register which captures the net worth of the decedent in the years prior to their death. While this would seem to be the best proxy we have for actual wealth at death, unfortunately the correspondence between the wealth register and the encoded estates is not as solid as we might have wished. This is illustrated by Figure 1, which shows scatterplots of the wealth on December 31st the year before death vs. the actual encoded estate, for individuals who died 2002-2004. We notice several things. First, while there seems to be a fairly linear relationship between wealth and estate at death, the slope of this relationship is not 1. Estate amounts at death are lower than recorded wealth in life throughout the distribution, even though both come from population registers. For large estates especially, this discrepancy is too large to be plausibly caused by individuals consuming away part of their wealth before death.[8] Second, at each level of wealth, there is substantial variation in the subsequent estate left at death. This makes imputation of estates difficult, especially because other available data do not explain this variation particularly well. Both of these issues may in part be caused by differences between the two registers (wealth and estates) in how the valuation of assets is recorded.[9]

Although the wealth prior to death is not as ideal a proxy of the estate as we might have hoped, it is still the best that is available in our data. We impute estates by semiparametrically regressing estate on pre-death wealth, income and other variables for individuals where we observe the estate, i.e. those who died 2002-2004, and then generating a *predicted estate* for all decedents based on regression parameters. Specifically, our semiparametric regression is obtained by first binning decedents into quantiles based on their pre-death wealth, and then, within each bin, regressing the actual estate on net worth on Dec. 31st the year before death (including quadratic and cubic terms

[7]This is because Belinda is based on data from the Swedish Tax Authority which stopped registering the amounts once they were not needed for tax calculations.

[8]And Erixson & Escobar (2018) find little evidence of accelerated consumption prior to death.

[9]For both the wealth tax and the inheritance tax, the calculation of the tax was based on the *tax-assessed value* of the assets, which differed from the *market value*. However, while the estate tax register records all amounts in terms of tax-assessed value, the wealth register records the actual market value. The difference between the two is important because it varies in significant ways across asset classes. For cash and personal items, the tax-assessed value is equal to the market value. However, for real estate for example, the tax-assessed value is equal to 75% of the market value. For listed shares, tax-assessed value is calculated as 80% of market value, while for unlisted shares, it is only 30%. More details in Waller (2000) and Henrekson & Du Rietz (2014). Since the estate tax register reports these lower amounts, it is not surprising that estates in our data are generally lower than wealth levels. And part of the variance may also be explained by the fact that individuals with the same market value of assets may have very a very different mix of assets, and thus different tax-assesed values. In future extensions of this work, we hope to gather and incorporate information on the mix of assets held to improve the precision of our imputation.

to capture possible nonlinearities within the bin), total annual income in the year prior to death, as well as demographic variables (indicator variables for age, gender, number of children and marital status).[10] For individuals in the control group where we observe both actual and predicted estates, Figure 2 illustrates the results of the imputation. The left panel is a repeat of panel B from Figure 1 and simply shows how estates and pre-death wealth correlates. The right panel shows how our imputation essentially "adjusts" the data points in the left panel to restore a linear 1:1 relationship between the predicted estate and the conditional mean of the actual estate - the slope of the line of best fit in panel B is approximately 1. Our imputation thus deals with the fact that estates are systematically lower than pre-death wealth holdings, but it cannot deal very well with the substantial amount of variation in estates at each pre-death wealth level. Other available variables do not explain much of the variation in wealth levels conditional on pre-death wealth.

Note that while the relative imprecision of our imputation method means that our estimates in the subsequent section will be less precise, there is no reason to think that it biases our estimates. Indeed, we would risk introducing significant bias if our estimation of wealth effects were to use actual estates for the control group and predicted estates for the treated group. However, our estimation will be based on the predicted estate variable for *all* individuals, both in the treatment and control groups. We can therefore think of the results we estimate below as being the effect of a *predicted* wealth shock on labor earnings. While we do expect this to cause our estimates to be less precise, we hope to obtain additional wealth data which can improve the precision of our imputation and thus our estimates of the wealth effect in future extensions to this work.[11]

4 Estimation strategy

The shock to wealth in our quasi-experimental setting is the *tax shock*: Individuals who inherited wealth from their parents during the years 2002-2004 (the control group) were paying tax on the received inheritance, if it was above a cutoff of SEK 70,000. Those who inherited in 2005 (the treatment group) avoided paying this counterfactual tax. Our estimation strategy is essentially a

[10] We use one bin for individuals with negative wealth before death, one for those with zero wealth before death, and then split those with positive wealth into 20 quantiles, for a total of 22 bins.

[11] One possible objection to this is that the predictions for the control group are in-sample predictions, while those for the treatment group are out-of-sample. If this were to introduce some degree of overfitting for the treatment group, we might be worried that predicted estates are more accurate in the treatment group, and that this causes biased estimates. We are not particularly worried about this, since our regressions have relatively few independent variables and the sample is very large. Only for extremely large estates might this be an issue, as the sample is much thinner at the tails. As a robustness check to our results, we will consider specifications where we drop extreme values.

triple-difference estimator: We compare individuals before and after the death of their parents according to the size of their tax shock and their status as part of the treatment or control group. Our main results thus indicate how increasing the tax shock by one additional SEK impacts labor earnings of treated individuals (relative to the control group) after the death of their parents (relative to before). We argue that these two groups are valid as treatment and control groups: Since the timing of parents' death can be assumed to be random, there is no reason to believe that either group has very significant characteristics from the other. Indeed this is confirmed by table 2, which compares observable characteristics between the two groups. Although the differences are in some cases statistically significant, they are small enough that the samples are still very similar. Note that all values are measured in the year before inheritance receipt for each individual.[12] This may explain some of the observable differences across cohorts: For instance, net worth and capital income may be higher for the treated group because asset prices were rising over this period. This motivates a more fundamental concern: That the timing of external events could bias our results because they affect our treatment and control groups in different years relative to their inheritance receipt. One major concern in this regard is the 2008 financial crisis, which was mild in Sweden but could still very plausibly affect labor market outcomes differently across time for the treatment and control groups. We address this concern by including calendar year fixed effects in all our specifications, which will absorb the effects of exogenous time-varying events like this.

Given the predicted estate which was calculated as described in the previous section, we obtain the tax shock as follows: First, we approximate the division of the predicted estates by assuming they are split equally among the children, according to the default rules set out by Swedish law. This assumption only introduces a very minor measurement errors: As documented by Erixson & Ohlsson (2019) using the same dataset as in the current study, over 97% of parents split their assets almost exactly equally among their children.[13] We then apply the schedule in Table 1 to the inheritance of each heir. This value is referred to as the tax shock. For the control group, it is the tax they pay on their (predicted) inheritance. For the treated group, it is the counterfactual tax that they save, and thus a positive shock to their wealth.

[12] Except age, which is measured in the year of inheritance receipt.

[13] "Almost exactly equally" means that the division deviates by less than $\pm 2\%$ from an exactly equal split, in line with the definition used by Wilhelm (1996). Erixson & Ohlsson (2019) show that 86.5 percent of decedents die intestate. Of the 13.5 percent with a will, 84 percent nevertheless split their assets completely equally among their children, while another 1.7 percent divide unequally, but within the $\pm 2\%$ margin.

Our main estimating equation is the following:

$$
\begin{aligned}
y_{it} = & \sum_s \beta_s \left(\tau_i \times T_i \times I_s^t\right) + \sum_s \gamma_s \left(T_i \times I_s^t\right) \\
& + \sum_s \delta_s \left(\tau_i \times I_s^t\right) + \sum_s \theta_s I_s^t + \kappa_c + \nu_i + \epsilon_{it},
\end{aligned} \tag{1}
$$

where t denotes the year relative to receipt of inheritance for individual i,[14] y_{it} is the outcome of interest, τ_i is the tax shock, T_i is an indicator for individual i belonging to the treatment group (i.e. inheriting someone who died in 2005), and I_s^t is an event time indicator which is one if $s = t$ and zero otherwise. κ_c is a calendar year fixed effect and ν_i is an individual fixed effect. We cluster standard errors at the individual level.

Our parameters of interest are the β_s's, which measure the effect of the tax shock on the treated across time. This of course relies on the usual parallel trends assumption: In the absence of the inheritance tax repeal, y_{it} would have evolved the same over time for individuals with the same tax shock who inherit in 2005 and prior to 2005. Note that including an individual fixed effect and lower level interactions of τ_i, T_i and I_s^t makes our specification very rich: The identification of the tax shock effect comes from differences in how y_{it} covaries with the tax shock differentially over time between treated and untreated individuals. The individual fixed effect means that we do not need to control for any individual characteristics, since this effect already absorbs all time-invariant differences between individuals.

5 Results

In this section, we present results of our estimation. Our main outcome of interest is labor earnings, which we can further decompose into wage earnings and self-employment earnings. We will also look at extensive margin responses, where we define an individual as being "employed" if she has annual labor and earnings above SEK 38,600.[15] In our view, it is most natural to estimate equation (1) with both the tax shock and outcomes measured in *levels*, such that the β_s's show the average effect of one additional SEK in tax shock on earnings measured in SEK the given year. This specification is also prevalent in previous literature, e.g. Cesarini *et al.* (2017). However, a weak-

[14] Note that we do not observe the times at which inheritances are actually transferred to heirs, so $t = 0$ is simply the year in which an individual's parent dies.

[15] This amount is a commonly used earnings ceiling for determining e.g. eligibility for benefits in the Swedish administrative system.

ness of this specification is that it is very susceptible to the effects of extreme outliers, both in the dependent variable and the independent variable. To deal with outliers in the dependent variable, we winsorize outcome variables at a 0.5% level unless otherwise noted. To deal with outliers in the inherited amount/tax shock dimension, we consider alternative specifications where we drop some of the values on the upper tail. As we will see, this can have a fairly large impact on our estimates. For this reason, we also consider a specification where both the outcome and the tax shock are measured in logs.[16] The effects we measure will be much clearer and more robust as a result of this, but the estimated parameters do not have a natural interpretation.

5.1 "First stage": Effect of the tax shock on net worth

In order for our research design to be plausible, we need the tax shock to actually impact the wealth of the heirs. Figure 3 shows parameter estimates for equation (1) with net worth as the outcome variable.[17] For our results to be plausible, we should expect to see assets increase for the treated group after inheritance relative to the control group. Panel A shows results using winsorized net worth as the outcome variable, where we see a hint of a small positive effect, though imprecisely estimated. Part of the reason why this effect is so small might be due to our winsorization: Given that the outcome variable is capped, the impact of extremely large tax shocks on assets may be underestimated. In panel B, we therefore show the same regression with non-winsorized net worth as the outcome variable, and here we see a much larger effect, again positive, which is consistent with our expectations. That the peak occurs in year 1 is not necessarily surprising: Year 0 is the year in which the decedent dies, but depending on processing times, the inheritance may not actually be paid out to the heir until the following year. However, the estimates in panel B seem too big: we would generally not expect a coefficient above 1. An explanation for our results here is that certain extremely rich individuals might dominate everything else. The fact that there is measurement error due to estates being imputed should not in itself bias our estimates, since this is the case for both the treatment and the control group, but if for instance we are slightly more likely to underestimate the tax shock for super large estates in the treatment group, this could explain part of the effect.[18]

[16] To be precise, we replace y_{it} and τ_{it} in equation (1) with $\log(1 + y_{it})$ and $\log(1 + \tau_{it})$ such that individuals with a value of zero are not dropped from the regression.

[17] We only have three post-event years in this specification because the wealth tax was abolished in 2007, and the wealth register stopped recording individuals' wealth after this year.

[18] Although this possibility might bias our estimates with net worth as the outcome variable, we are less concerned about similar bias when examining the impact on labor supply. While we expect net worth in the short run to respond

In Table 3, we further explore the estimated impact on net worth across a variety of specifications where we drop individuals on the upper tail of the tax shock distribution. These estimates are from a simplified version of our main regression which includes year fixed effects, but where the year dummies in all interaction terms have been replaced by an indicator for post-inheritance. Although results vary in magnitude and statistical significance, they confirm that we do indeed see a positive effect of the abolishment of the inheritance tax on assets, as we would expect.

5.2 The wealth effect on labor supply

We now move to our main results, estimating the effect of a tax-induced wealth shock on labor supply. Figure 4 shows the marginal effect of increasing the tax shock by 1 SEK on annual labor earnings for the treatment group relative to the control group. It can therefore be interpreted directly as a wealth effect on labor earnings. Because the individual fixed effects also control for the size of the inheritance, it is purely the tax-induced shock to wealth that induces this effect. We see a somewhat positive, but not statistically significant pre-trend, which supports the usual parallel trends assumption. The plot provides compelling evidence that the tax variation we study does indeed induce a wealth effect: After receiving their inheritance, treated individuals reduce their labor earnings significantly more as their tax shock increases, relative to the untreated. The fact that there is no effect in year 0 (when the parent dies) is not necessarily surprising: We do not have data on processing times for estate division, so individuals may in many cases not receive their inheritances (and thus the effect of the tax shock) until the following year.

We estimate that at the peak, an additional wealth shock of 1 SEK leads to an annual earnings decrease of roughly 0.015-0.017 SEK. This is consistent with estimates from the previous literature using different research designs (Cesarini *et al.*, 2017; Holtz-Eakin *et al.*, 1993; Nekoei & Seim, 2019). This effect seems to peter out somewhat in year 4 after inheritance receipt, which is also in line with previous studies that have looked at the long run wealth effects of inheritance.[19]

While our finding of a modest reduction in labor supply following a positive shock to wealth may not be surprising, it is less clear intuitively which type of income we would expect to respond more strongly: Wage income or self-employment income. On one hand, studies (e.g. Kleven & Waseem, 2013) often show that self-employed individuals respond more strongly to features of the

roughly linearly to the wealth shock, we do not expect labor supply to respond as strongly for those receiving extremely large inheritances and tax shocks, simply because there are limits to how much labor supply can be reduced.

[19] While our current dataset ends in 2009, we are hoping to get access to a different dataset extending to the present day, which would allow us to study the longer run effects of the tax reform and calculate an approximate elasticity of lifetime labor supply with respect to lifetime earnings.

tax system since they have more freedom to adjust their earnings, and so we might expect any wealth effect to manifest itself more strongly for them. On the other hand, a positive wealth shock might provide a source of capital that the self-employed can invest and use to grow their business. In Figure 5, we examine the effect of the tax shock on the two components of labor income: Wage income (Panel A) and self-employment income (Panel B). We see that the effect on wage income shows much the same overall pattern as the one for overall income, although the magnitude of the estimated effect is somewhat smaller, and errors are large, so none of the estimates are statistically significant. In panel B, we see essentially a zero effect for self-employment income, which is also rather precisely estimated. This could be due to the wealth effect and the "reinvestment effect" for business owners balancing out on average, or it could be that people select into self-employment based on characteristics that are correlated with a smaller wealth effect.[20]

Finally, we consider extensive margin effects. Figure 6 shows the results of our main specification with an indicator for annual labor earnings above SEK 38,600 (approximately USD 4000) as the outcome variable.[21] The tax shock variable has also been rescaled so it captures the effect of a shock of SEK 100,000 (Around USD 10,000). In spite of this rescaling, the estimated response is tiny, even though it is statistically significant.

5.3 Robustness and alternative specifications

While our main results are compelling, there is also reason to believe that observations in the extreme right tail of inheritance size may have an outsize impact on our estimates. This would make sense: The distribution of estates and inheritances is very skewed, and given the fact that we have carried out our estimations with both the tax shock and the outcomes in levels, it is clear that the behavioral responses of individuals with tax shocks in the tens of millions will have a very large impact on the parameter estimates. This can be problematic given that individuals at the tail end are likely to display a more modest earnings response relative to the typical individual: Estates are more skewed than incomes, and those who receive the largest inheritances do not necessarily have the highest incomes. There is therefore an upper limit to how much they can possibly respond to their wealth shock.

[20]Our results are consistent with Escobar (2019), who also studies the effect on business owners of receiving an inheritance (but not using tax-induced variation). He finds that while the survival rate of businesses increases, the income of their owners does not. They are also in line with the findings of Cesarini *et al.* (2017), who find a very small, statistically insignificant wealth effect on self-employment income.

[21]This reflects the "prisbasbelopp" at the time, an amount used widely across different Swedish government agencies when setting income limits for e.g. eligibility for certain transfers.

To examine this issue, we consider various alternative specifications where we drop individuals with tax shocks on the extreme right tail. Figure 7 shows the outcome of the same estimating equation as in Figure 4, the only difference being that we have dropped observations with estimated tax shocks of over 3 million SEK (Panel A) and 1 million SEK (Panel B). This drops only 81 and 489 individuals from our sample, respectively. As we see, the cut at 3 million SEK does not substantially change the pattern that we saw in Figure 4 – We still see a relative dip in labor earnings for the treated individuals following the tax shock, of a slightly larger magnitude, although standard errors are now also bigger. But moving to Panel B, where we have dropped a slightly larger number of extreme observations, we now see that the effect is even larger, even less precise, and also somewhat worryingly, does not feature a clear jump at time 0, but rather just seems to grow larger and larger over time. This does raise some concerns that our estimates are very sensitive to individual observations at the top, and that they get more noisy once we drop those few individuals who have an outsize impact on the overall pattern. Perhaps there is no clear wealth effect in our general population, but only in the extreme tail?

To address this issue, we estimate a version of equation (1) in which we measure both the tax shock and income in logs.[22] This specification has the advantage that it naturally limits the impact of extreme values in both the dependent variable and the tax shock on our estimates. However, it does not have a natural interpretation: There is no particular reason to believe that a percentage change in the tax shock should correspond to a certain percentage change in labor earnings.[23]

Figure 8 shows the results of this specification: Panel A for total labor income, panel B for wage income, panel C for self-employment income, and panel D for extensive margin responses. As we see, we now have very clear negative effects for labor and wage income, including a sharp change following the tax shock, and then a levelling off after a few years. This is consistent with our main specification and alleviates concerns that the effect is entirely driven by few individuals who experience very large tax shocks. In panel C, we see no effect on self-employment income – again consistent with previous results. For the extensive margin effect, we also have the negative response that we would expect.

As we would expect, these specifications measured in logs are very robust to dropping observations at the right tail. The corresponding graphs when dropping individuals with predicted tax

[22]We add 1 to both the tax shock and the labor income, such that zeros do not get dropped from the sample, but included in the estimation with a value of zero. Also note that the variables we use are not winsorized in this case. When we estimate the effect on retirement, the outcome dummy variable for retirement is still measured in levels.

[23]If we were able to measure the tax shock as a share of lifetime wealth, a specification in logs would be more natural to interpret, since we could then think of the estimate directly as a wealth elasticity.

shocks over 1 or 3 million are not shown here, but are practically identical to those shown in Figure 8.

5.4 Heterogeneous responses by gender

Having shown the presence of an overall impact of wealth on labor earnings, we now explore how this effect differs between men and women. The traditional view in public finance has been that women respond more strongly to *marginal* tax rates than men,[24] but it is less clear whether there are any differences in responses to wealth shocks.

In Figure 9, we explore this question by showing our estimates of labor responses to the tax shock separately for men and women. The top two panels show results for men and women from the full sample. The middle two panels show results when we drop individuals with a tax shock of over 3 million, while the bottom two panels show the results when we drop individuals with a tax shock of over 1 million. The patterns that we see are interesting: When we include the full sample, the wealth effects on labor supply that we find seem to be driven entirely by men in the sample, whereas women barely respond. When we drop observations at the top, this pattern reverses and it is the women who respond to the shock. This would seem to suggest nonlinear effects that differ by gender, such that men are more responsive when receiving large shocks to their wealth, while women are relatively more responsive to smaller shocks. However, it might also be due to a mechanical effect: The correlation between the inherited amount and pre-inheritance labor earnings is larger for men than for women (0.052 vs. 0.034), which means that men who receive large inheritances tend to have larger baseline incomes and therefore also have more scope to decrease their labor earnings.

Analogously to above, we also show the results of a specification in logs separately by gender, which again is much more robust to dropping extreme observations. This is shown in Figure 10. The results here show clear and significant earnings responses by both genders, although the estimated magnitude of the effect is slightly larger for women.

In future extensions to this work, we would like to explore other heterogeneous responses, including cross-effects within households, i.e. whether and by how much people reduce their labor supply in response to their spouse receiving a positive shock to their wealth. While our current dataset does not allow us to link spouses within households, we would be able to do so

[24] See for instance Blundell & MaCurdy (1999). Note, however, that this finding is mostly from US studies of married women in the 1970s and 80s, a time when female labor market participation was generally increasing. As pointed out by Blau & Kahn (2007), female labor supply elasticities in the US have been declining since the 1980s.

with an alternative dataset which we are hoping to get access to.

6 Conclusion

By studying a Swedish inheritance tax reform and exploiting the randomness of the timing of death around the time of the reform, we have carried out the first quasi-experimental analysis of labor supply impacts from tax-induced pure wealth shocks. We thus provide a tax policy relevant wealth effect on labor supply, which has not previously been done in the literature.

The tax reform generates variation in wealth shocks which is plausibly uncorrelated with any unobserved heterogeneity, since assignment to the treatment and control group happens purely based on the random timing of parents' death. Our sample is representative of the Swedish population by construction, since nearly everyone will inherit either nothing or a positive amount during their lifetime. Our results show clear wealth effects on labor supply: In our main specification, we estimate that an additional 1 SEK shock to wealth causes labor earnings to fall by approximately 0.016 SEK – in line with previous estimates using different research designs – though our estimate is somewhat imprecise and sensitive to outliers. The effect is almost entirely due to a reduction in wage earnings, with no effect on self-employment earnings.

While our results clearly indicate that a positive wealth shock does indeed affect labor earnings negatively, the exact magnitude of our estimates is somewhat uncertain. This may be due to the fact that we use predicted rather than actual estates in our estimation, as we do not observe actual estates for individuals who died in 2005. We use the predicted estate for both our treatment and our control group, so this should not lead to any bias, but it may lower the precision of our estimates. In future extensions to this work, we hope to incorporate additional information on agents' assets, in order to increase the precision of our prediction method for estates. We also hope to get aceess to a different dataset which includes more years of data, so we can analyze the longer-run effects of the tax shock on labor supply.

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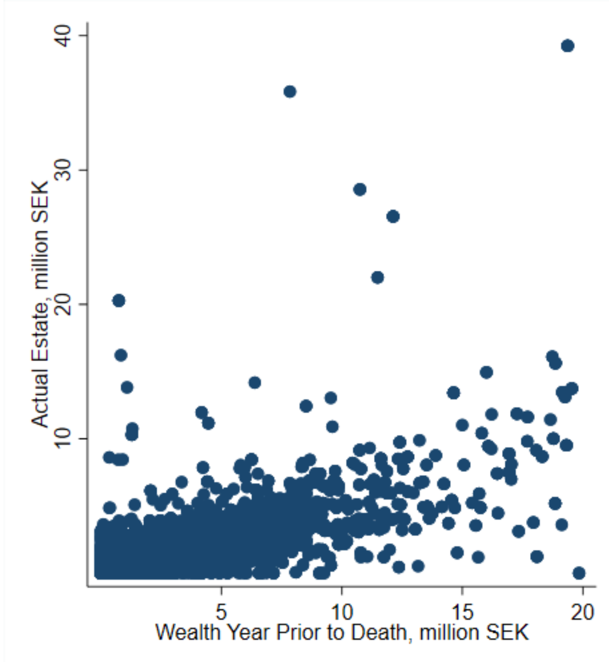
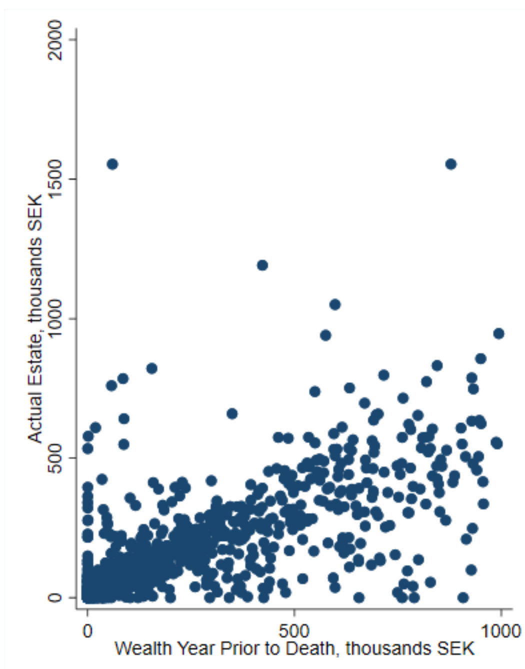
Figure 1: Wealth Before Death and Actual Estates

A: Large estates

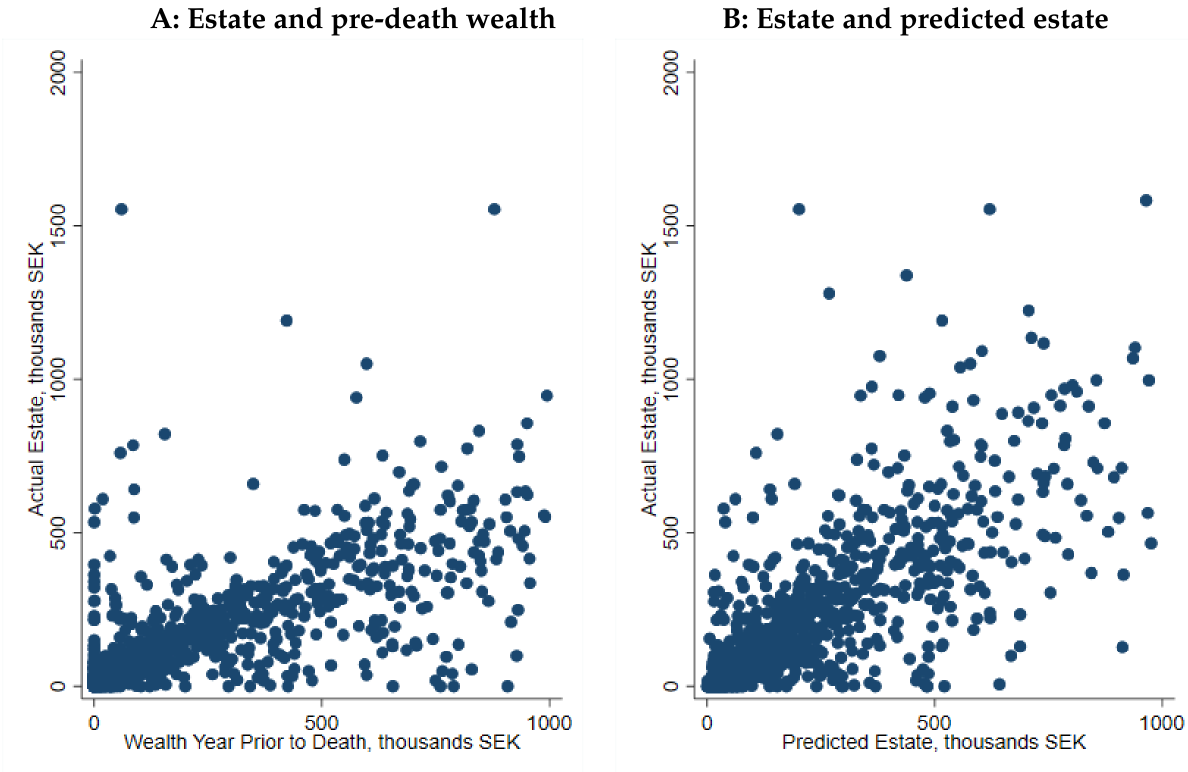


B: Medium-sized estates



Notes: These scatterplots show the joint distribution of actual estates and wealth at the end of the calendar year before death for decedents in our sample. Only decedents who died in the years 2002-2004 (the control group) are included, as these are the individuals for whom we have information on the actual estate at death. Panel A shows results for the full population, but exludes a few extreme outliers along both axes in order to better show the distribution for the bulk of the population. Panel B shows the same plot for individuals with wealth less than 1 million SEK prior to their death, but restricted to a random sample of 30,000 individuals to better see the patterns in the data. Here, a few outliers along the vertical axis have also been exluded.

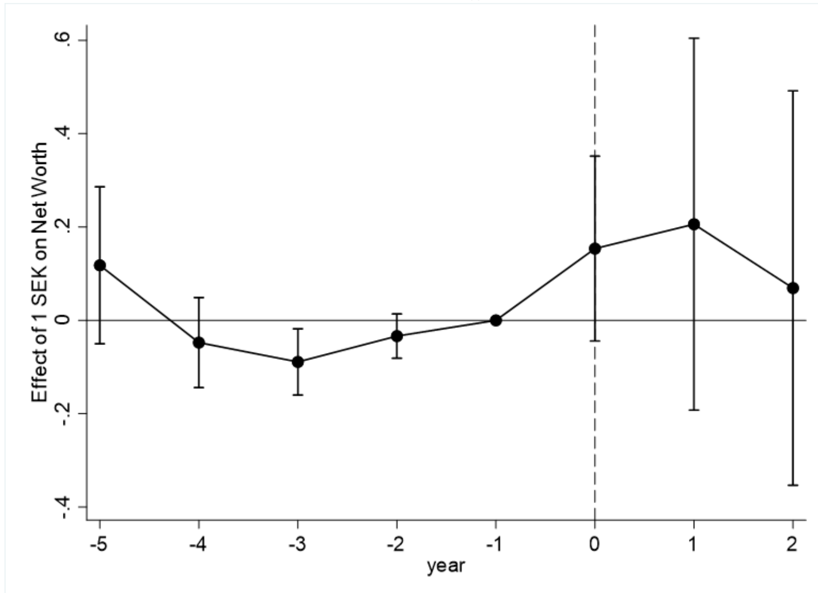
Figure 2: Prediction of Estates from Pre-Death Wealth



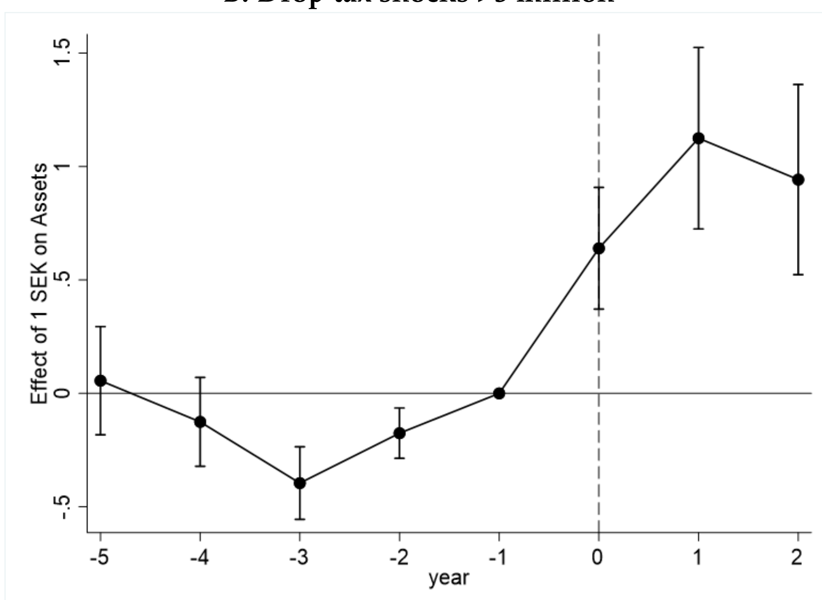
Notes: Panel A shows the joint distribution of actual estates and wealth at the end of the calendar year before death for a random sample of 30,000 decedents in our sample who died in 2002-2004. We have restricted the scatterplot to individuals with wealth of less than 1 million SEK before their death, and the sample excludes a few extreme outliers along the vertical axis. Panel B shows the outcome of our procedure to predict unobserved estates. Here again, we only display individuals who died in 2002-2004, for whom we have information on their actual estate. We plot the actual, observed estate against the estate predicted by our model based on pre-death wealth, income and demographic variables.

Figure 3: Impact of the tax shock on net worth

A: Full sample

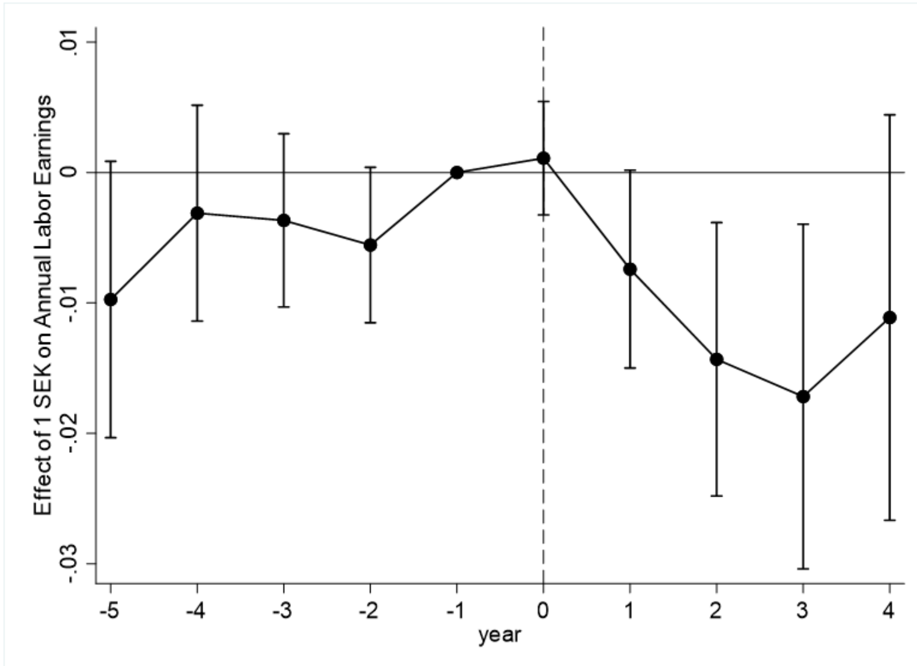


B: Drop tax shocks >3 million



Notes: This figure shows the estimated coefficients $\hat{\beta}_{-5}, \hat{\beta}_{-4}, \ldots, \hat{\beta}_2$ of our main estimating equation (1), with net worth as the outcome variable. Panel A estimates the equation on the full sample, 364,618 individuals in total, while Panel B drops 81 individuals with a tax shock over 3 million SEK. The coefficients show the effect of an additional 1 SEK in the tax shock on net worth each year for the treated group relative to the untreated group. The net worth variable is winsorized at a 0.5% level in both panels. Standard errors are clustered at the individual level.

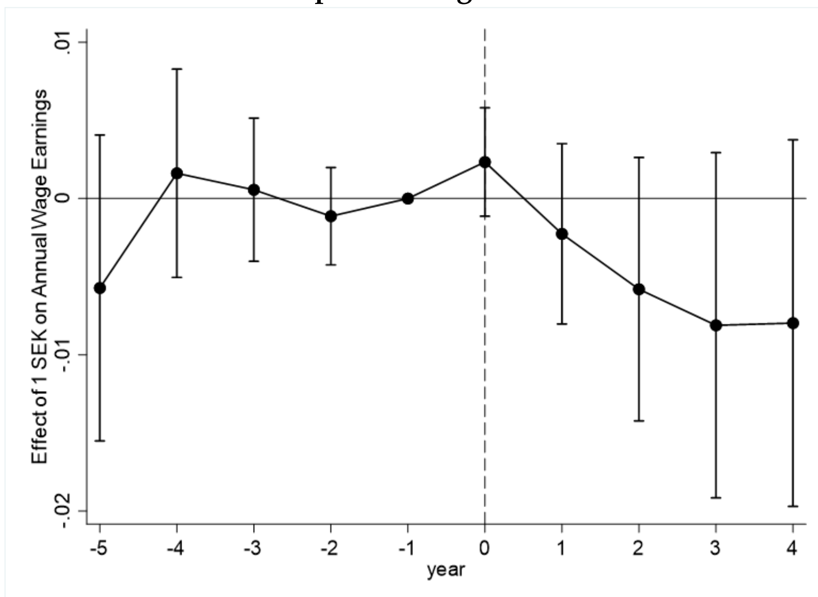
Figure 4: Impact of the tax shock on labor earnings



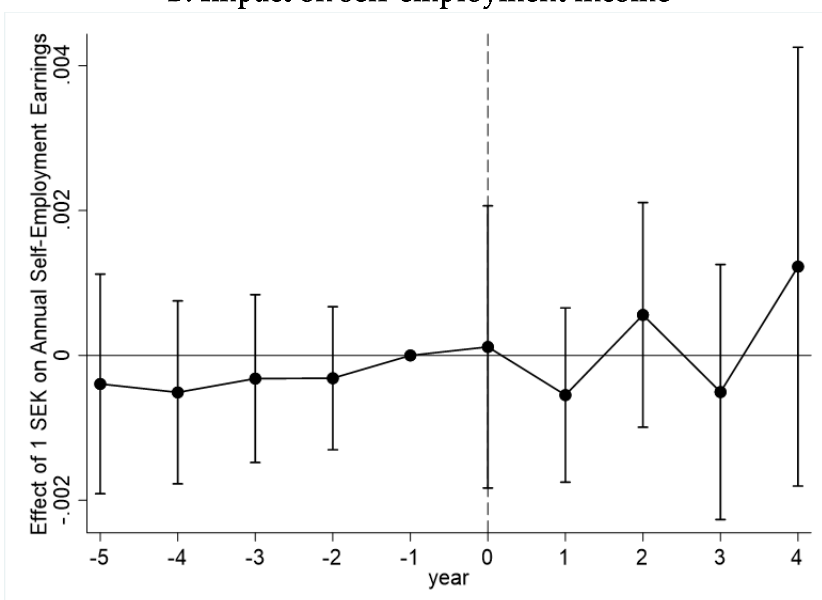
Notes: This figure shows the estimated coefficients $\hat{\beta}_{-5}, \hat{\beta}_{-4}, \ldots, \hat{\beta}_4$ of our main estimating equation (1), with labor earnings (wage and self-emloyment combined) as the outcome variable. The coefficients thus show the effect of an additional 1 SEK on labor earnings each year for the treated group relative to the untreated group. The labor earnings variable is winsorized at a 0.5% level. This graph includes all individuals in our full sample, 364,618 individuals in total. Standard errors are clustered at the individual level.

Figure 5: Impact of the tax shock on wage and self-employment income

A: Impact on wage income

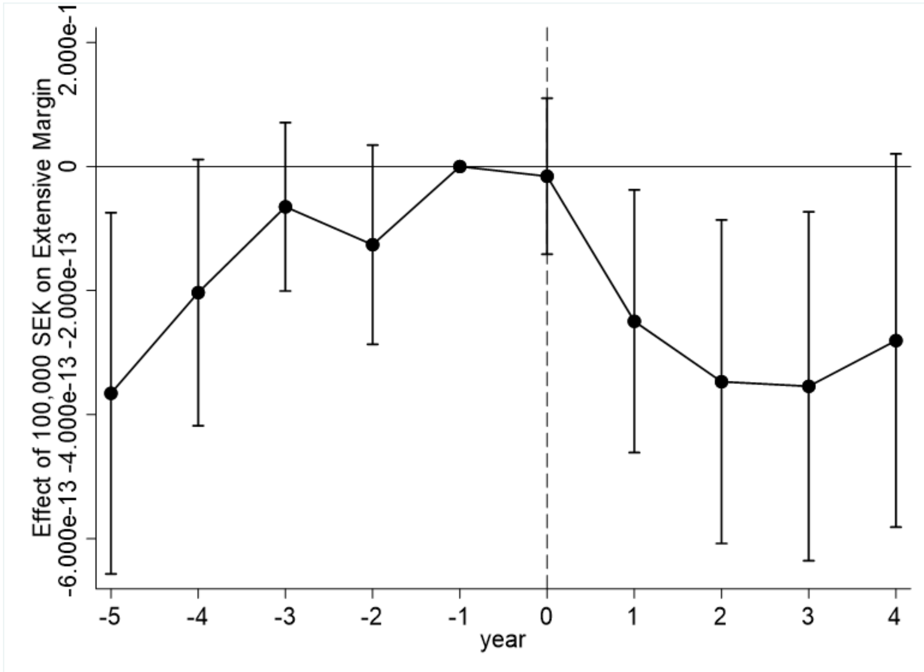


B: Impact on self-employment income



Notes: This figure shows the estimated coefficients $\hat{\beta}_{-5}, \hat{\beta}_{-4}, \ldots, \hat{\beta}_4$ of our main estimating equation (1), with wage earnings (Panel A) and self-employment earnings (Panel B) as the outcome variable. The coefficients thus show the effect of an additional 1 SEK on earnings each year for the treated group relative to the untreated group. The wage and self-employment earnings variables are winsorized at a 0.5% level. This graph includes all individuals in our full sample, 364,618 individuals in total. Standard errors are clustered at the individual level.

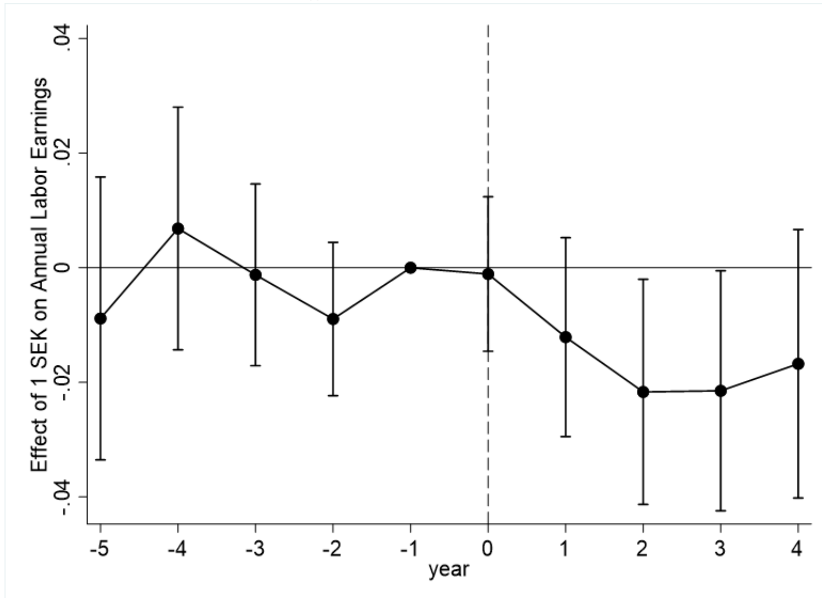
Figure 6: Impact of the tax shock on the extensive margin



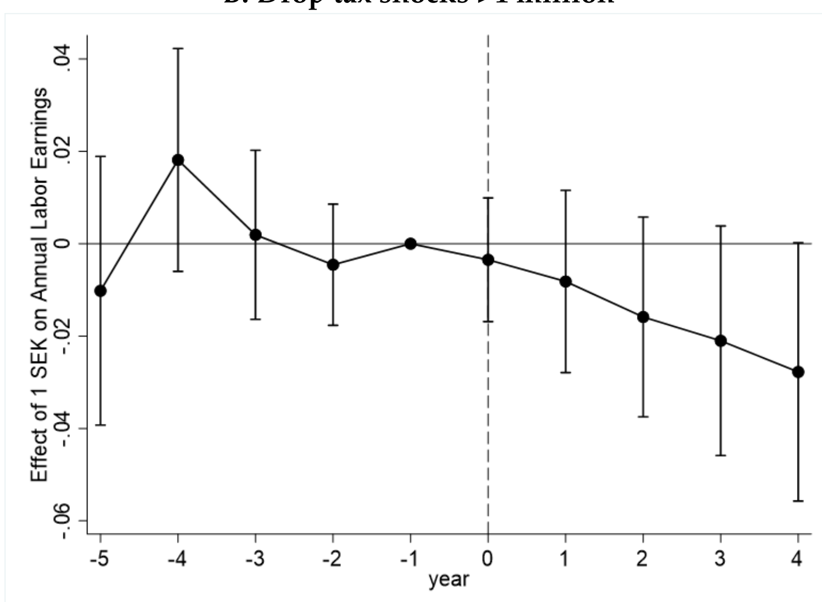
Notes: This figure shows the estimated coefficients $\hat{\beta}_{-5}, \hat{\beta}_{-4}, \ldots, \hat{\beta}_4$ of our main estimating equation (1), with an extensive margin indicator as the outcome variable. This indicator takes a value of 1 if annual labor earnings are greater than SEK 38,600, and zero otherwise. The coefficients thus show the effect of an additional 1 SEK on the probability of earning labor income above this threshold each year for the treated group relative to the untreated group. This graph includes all individuals in our full sample, 364,618 individuals in total. Standard errors are clustered at the individual level.

Figure 7: Impact on labor earnings – robustness to dropping outliers

A: Drop tax shocks >3 million

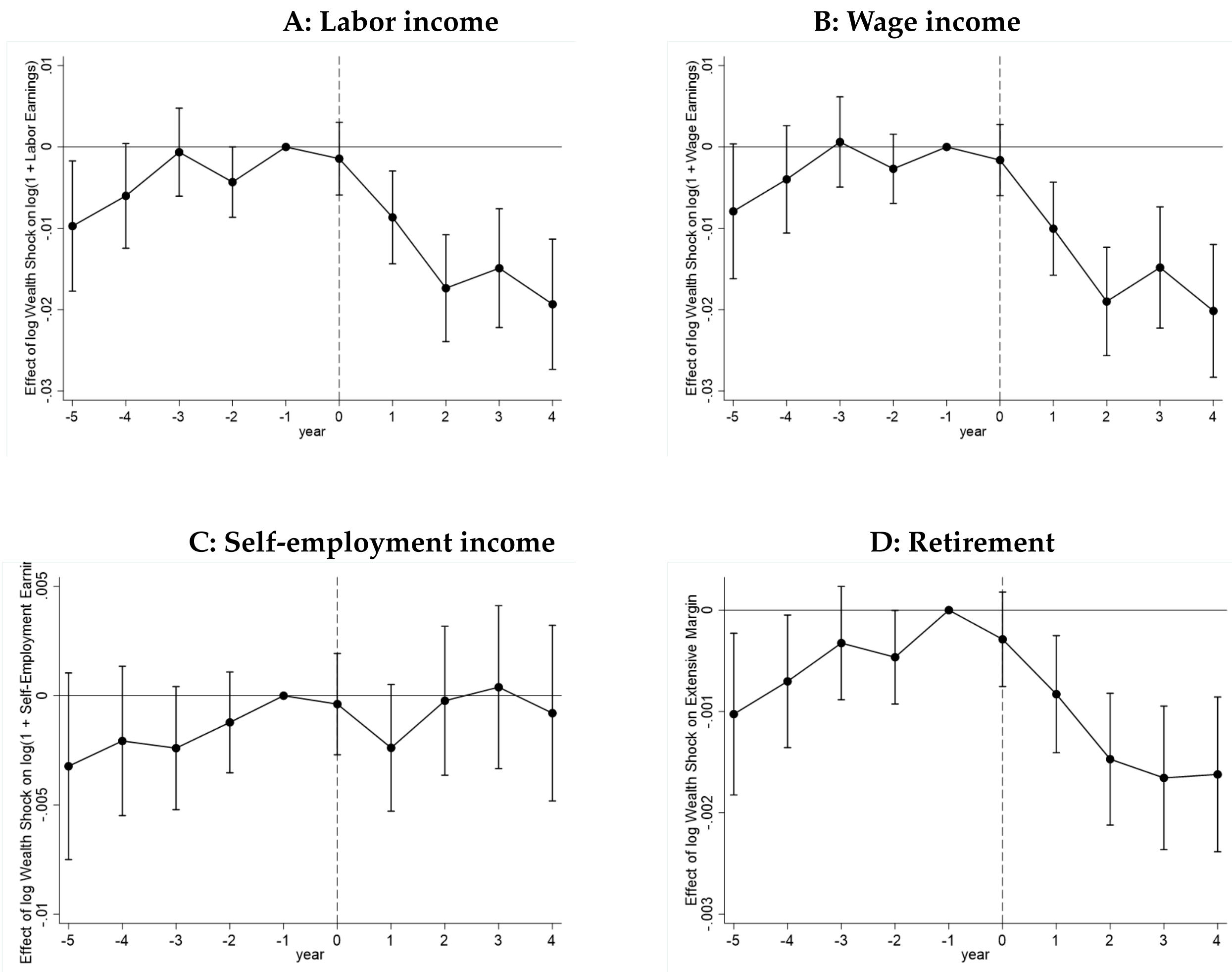


B: Drop tax shocks >1 million



Notes: This figure shows the estimated coefficients $\hat{\beta}_{-5}, \hat{\beta}_{-4}, \ldots, \hat{\beta}_4$ of our main estimating equation (1), with labor earnings (wage and self-emloyment combined) as the outcome variable. Panel A estimates the equation on a sample where we drop 81 individuals with a tax shock over 3 million SEK, while Panel B shows the same equation for a sample where we drop 489 individuals with tax shocks over 1 million SEK. The labor earnings variable is winsorized at a 0.5% level in both panels. Standard errors are clustered at the individual level.

Figure 8: Impact of tax shock - specification in logs

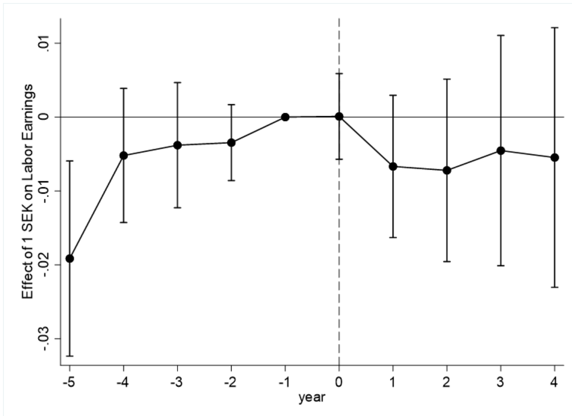


Notes: This figure shows the estimated coefficients $\hat{\beta}_{-5}, \hat{\beta}_{-4}, \ldots, \hat{\beta}_4$ of four different versions of our main estimating equation (1), where the tax shock variable has been replaced by $\log(1 + \tau_i)$, and where the outcome variables are – Panel A: log(1 + labor earnings), Panel B: log(1 + wage earnings), Panel C: log(1 + self-employment earnings), and Panel D: An indicator for labor earnings above SEK 38,600. None of the outcome variables are winsorized in these equations. All panels include all individuals in our full sample, 364,618 individuals in total. Standard errors are clustered at the individual level.

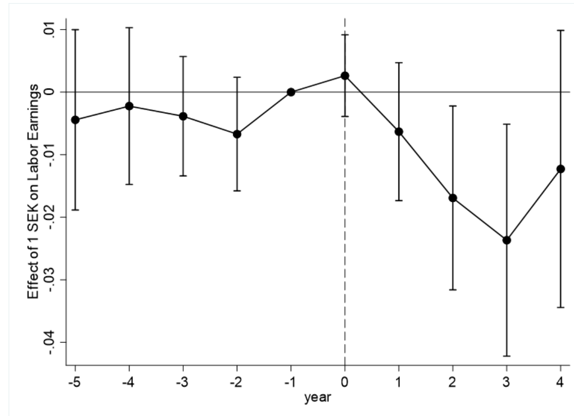
Figure 9: Differential impacts on labor income by gender

A: Full Sample

A1: Women

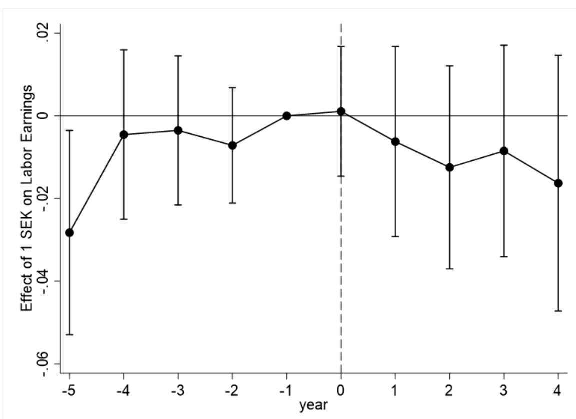


A2: Men

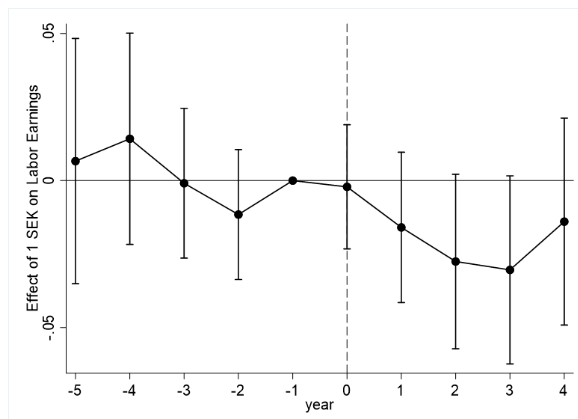


B: Drop tax shocks >3 million

B1: Women

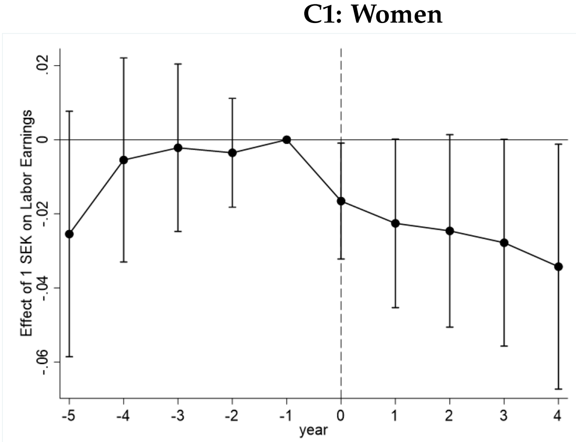


B2: Men

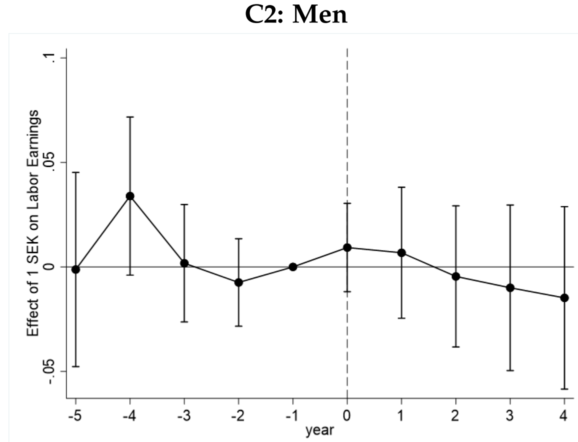


C: Drop tax shocks >1 million

C1: Women



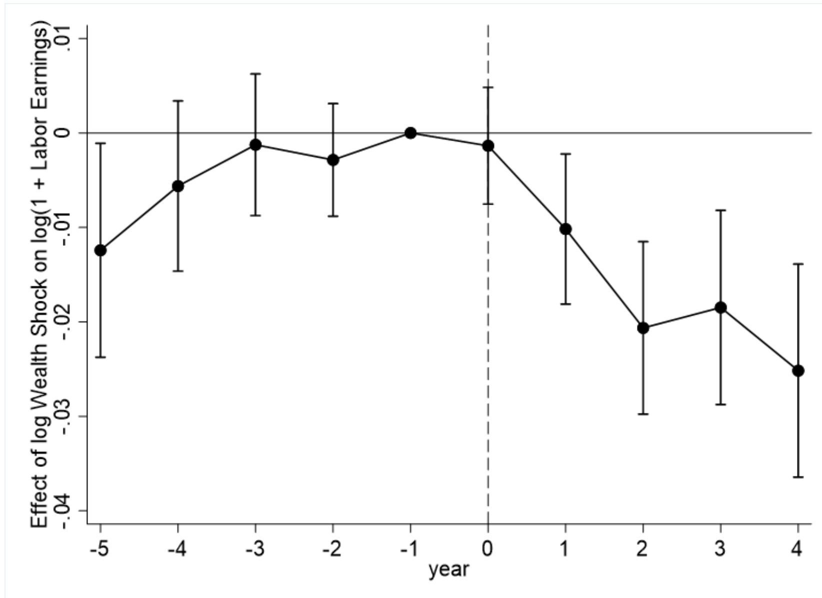
C2: Men



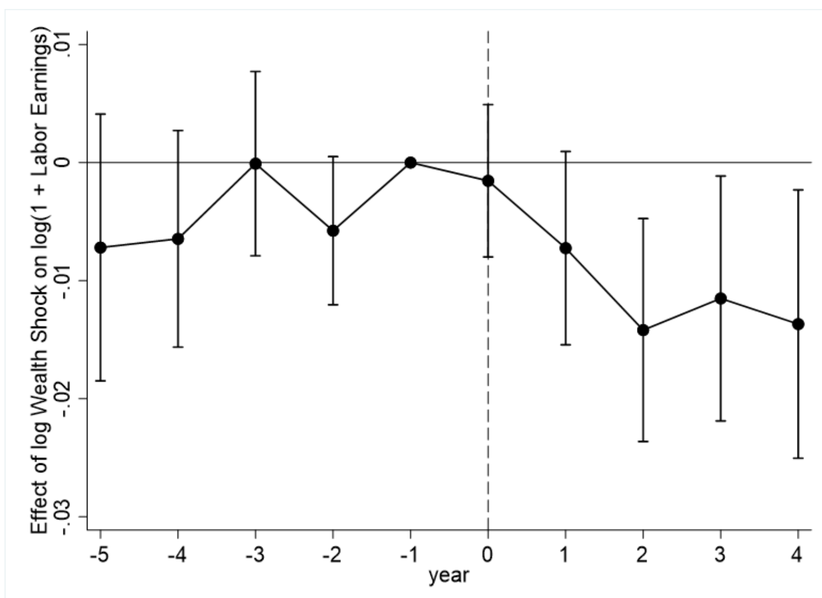
Notes: This figure shows the estimated coefficients $\hat{\beta}_{-5}, \hat{\beta}_{-4}, \ldots, \hat{\beta}_{4}$ of our main estimating equation (1), with labor earnings (wage and self-emloyment combined) as the outcome variable. The graphs on the left restrict the sample to female heirs only, while the ones on the right restrict the sample to male heirs. Panel A estimates the equation on the full sample, Panel B drops individuals with a tax shock over 3 million SEK, and Panel C drops individuals with a tax shock over 1 million SEK. The labor earnings variable is winsorized at a 0.5% level. Standard errors are clustered at the individual level.

Figure 10: Differential impacts on labor income by gender – log-specification

A: Women



B: Men



Notes: This figure shows the estimated coefficients $\hat{\beta}_{-5}, \hat{\beta}_{-4}, \ldots, \hat{\beta}_4$ of a version of our main estimating equation (1), where the tax shock variable has been replaced by $\log{(1+\tau_i)}$, and where the outcome variables is log(1 + labor earnings). Panel A estimates this equation for a sample of female heirs only, while Panel B restricts the sample to male heirs only. The outcome variable has not been winsorized. Standard errors are clustered at the individual level.

Table 2: Summary statistics by treatment and control group

	(1) Control (2002-2004)	(2) Treated (2005)	(3) Difference (2)-(1)	(4) p-value
Age	52.1	53.0	0.900	0.000
Female	0.495	0.494	0.000	0.834
Married	0.539	0.530	-0.010	0.000
Primary educ.	0.269	0.249	-0.020	0.000
Lower secondary educ.	0.447	0.457	0.010	0.000
Upper secondary or postgraduate educ.	0.257	0.267	0.010	0.000
Net worth	625,764	696,222	70,459	0.002
Capital income	6,655	10,765	4,109	0.223
Wage earnings	172,192	175,502	3,309	0.000
Self-employment earnings	6,559	6,719	159	0.289
Employment indicator	0.718	0.707	-0.011	0.000
Estate	257,603	.	.	.
Inheritance	116,465	.	.	.
Inheritance tax payment	11,834	.	.	.
N	273,801	90,817		

Notes: The first two columns show summary statistics for individuals in the control group (those who inherit 2002-2004) and the treatment group (those who inherit in 2005). The numbers are means of each variable, measured for each individual in the year prior to inheritance receipt, except age, which is measured in the year of inheritance receipt. Column (3) shows the difference between the first two columns, and column (4) displays the p-value from a t-test for equality between the two groups.

Table 3: Tax shock impact on net worth for various specifications

	(1) Full Sample	(2) Drop >3m	(3) Drop >1m	(4) Drop top 1%
Impact of tax shock×treated×post-inh. on:				
Net Worth	0.188	1.124***	1.683***	1.941***
	(0.183)	(0.165)	(0.175)	(0.196)
Assets	0.168	1.096***	1.675***	1.923***
	(0.193)	(0.181)	(0.183)	(0.203)
Debts	-0.043**	-0.033	-0.082**	-0.104**
	(0.018)	(0.027)	(0.035)	(0.0409)
Observations	2,448,717	2,448,179	2,445,985	2,441,043
Number of individuals	364,543	364,462	364,129	363,386

Notes: For various outcomes and sample restrictions, this table shows estimates of the coefficient β from the estimating equation $y_{it} = \beta\,(\tau_i \times T_i \times P_t) + \gamma\,(T_i \times P_t) + \delta\,(\tau_i \times P_t) + \sum_s \theta_s I_s^t + \kappa_c + \nu_i + \epsilon_{it}$. This is a version of our main estimating equation (1) in which the year dummies in all interactions have been replaced by a simple dummy for post-inheritance, P_t, which takes value 1 if $t \geq 0$ and 0 otherwise. The coefficient β captures the average effect across all post-inheritance years, relative to pre-inheritance years, of increasing the tax shock by 1 SEK, for the treated group relative to the control group. Note that each cell in the table provides the estimate $\hat{\beta}$ from a different regression. The rows in the table correspond to different outcome variables, and the columns correspond to different sample restrictions. The first column estimates the equation on the full sample, the second column drops obervations with estimated tax shocks greater than 3 million SEK, the third column drops tax shocks greater than 1 million SEK, and the fourth column drops the top percentile of positive tax shocks, corresponding to shocks greater than SEK 518,000. All outcome variables are winsorized at the 0.5% level, and standard errors are clustered at the individual level.