Inheritance tax planning: Spousal bequests and under-reporting of inheritances in Sweden

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Abstract

People’s planning to evade the inheritance tax curtails its merits. However, the extent of planning remains a matter of argument. According to popular belief, it is widespread, but few estimates have been presented. This study estimates the extent of estate size under-reporting, a form of inheritance tax planning, using the repeal of the Swedish tax on spousal bequests, in 2004, and a regression discontinuity design. The results show that, on average, estate sizes were 17 percent lower, and that the share of estates that completely escaped tax payments was 26 percent larger due to under-reporting. As a consequence, government revenues from the tax were only half of what they would have been without under-reporting. Moreover, preferences and means for under-reporting were not only prevalent among the wealthy, but also among those receiving relatively small inheritances. The study contributes to a growing literature on tax avoidance and evasion by estimating the extent of estate size under-reporting, its effect on government revenues and by showing that it was widespread in the population.

Keywords: Estates, intergenerational transfers, inheritance taxes, tax evasion

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1 Introduction

The growing interest in wealth and wealth inequality has created new attention to the inheritance tax. The tax is often seen as a means of promoting equality of opportunity and raising revenue. It can also contribute substantially to the overall progressivity of taxes (Piketty and Saez 2007). However, there is no consensus on whether or not the tax is desirable. Recent studies show that the inheritance tax may increase relative wealth inequality (Elinder, Erixson, and Waldenström 2016). It may also distort labor supply and savings (Joulfaian 2006). The relative importance of these benefits and drawbacks depends largely on whether or not people are able to evade the tax through planning. This paper shows that inheritance tax planning—and in particular estate size under-reporting—is widespread and, indeed, a valid concern.

Estimating the extent of tax planning is made difficult by lack of data. People are reluctant to answer questions regarding their planning, and there are no records on the amount of taxes withheld from the tax agency. The problem is not specific to studies on inheritance tax planning, but is also present in studies on planning for other taxes. To overcome the problem, it is common to look for traces of specific planning strategies, or to try to determine what the tax payments and the tax base would have been in the absence of planning (see, for instance Pissarides and Weber 1989 and Engström and Hagen 2016). An expanding branch of the literature does this by using quasi-experimental designs or randomized field experiments (see, for instance, Slemrod, Blumenthal, et al. 2001, Gorodnichenko et al. 2009, Kleven et al. 2011, and Pomeranz 2015). The virtue of these methods is that they address several potential sources of bias and are thus more likely to only capture tax planning.

However, quasi-experimental designs and randomized field experiments remain scarce in studies on inheritance tax planning, which instead have resorted to less causal methods. For instance, some studies have tried to determine the extent of inheritance tax planning by comparing the value and asset composition of deceased’s estates with the value and asset composition of living individuals’ wealth (Wolff 1996, Poterba 2000, Eller, Erard, et al. 2001, Poterba and Weisbenner 2003). But differences between estates and wealth may depend on factors other than just planning, such as end-of-life consumption or medical expenses close to death. Other studies compare reported estate sizes before and after the estates

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2. See Slemrod and Weber 2012 for further discussion on the merits of these strategies.
3. For a discussion on the literature, see Kopczuk 2013.
have been audited (Eller and Johnson 1999). This is certain to reflect planning, but it only captures planning strategies that can be discovered in the audit, which are not necessarily all planning strategies available. Yet another example of how tax planning can be measured is provided by Kopczuk (2007). He measures the extent of tax planning using terminal illness, assuming that people procrastinate on decisions related to death, but that the onset of terminal illness reminds them of their mortality and induces planning, which in turn reduces the reported estate size and tax payments. Thus, the longer the terminal illness, the more time there is for planning, and the more the reported estate size and tax payments may be reduced. Kopczuk indeed finds that estate sizes are lower for those who passed away from a lengthy terminal illness; a finding that supports the presence of tax planning. However, it is not possible to say with certainty how much the lower estate size depends on responses in planning and how much it depends on other responses to the illness, such as medical expenses and end-of-life consumption.

In trying to ensure that only responses in tax planning are captured, this study follows the quasi-experimental development of the broader tax planning literature and uses a regression discontinuity (RD) design. The design exploits variation in tax planning incentives created by the repeal of taxes on spousal bequests in Sweden, a repeal effective on January 1, 2004. Before the repeal, bequests larger than SEK 280,000 between spouses were taxed according to a progressive tax schedule with tax rates of up to 30 percent. Thus, there were strong incentives to plan in order to reduce tax payments before the repeal. After the repeal, however, there were no such incentives. This setting, with a sudden and immediate change in incentives, is suitable for estimation using a RD design. The design compares the estate of individuals passing away just before and just after the tax repeal. It is assumed essentially random who passes away just before and who passes away just after the repeal. Other factors possibly affecting the estate, including the decedent’s actual wealth, are accounted for under that assumption, meaning that differences in the estate come only from the changed incentives for planning.

The choice of identification strategy has implications for the interpretation of the results. By focusing on individuals passing away close to the tax repeal, long-term planning is likely to be kept constant, meaning that mostly planning carried out close to the decedent’s demise—and primarily after it—is captured. The reason for this is that decedents passing away closely before the repeal were unsure as to which tax regime that would apply to their estate, and therefore unlikely to make any last minute changes in their planning. Similarly, those passing away closely after the repeal had little time to adapt to the new regime, and were therefore also

4. Approximately USD 32,000, with a conversion rate of 8.75 SEK/USD.
unlikely to make any last minute changes. The surviving spouse, on the other hand, who may carry out planning after the donor’s demise, knows which tax rules apply, and can thus respond to them. Another implication of this is that the planning captured most likely is estate size under-reporting, as this is the only tax planning strategy available after the donor’s demise, given the institutional setting. I carry out several tests supporting this interpretation. The interpretation implies that the estimates should be seen as lower bounds on the full extent of tax planning, as under-reporting is only one of many possible strategies to lower tax payments. However, they are likely to capture the full extent of the specific tax planning strategy under-reporting as this strategy only may be carried out after the donor’s demise.

The analysis is made possible by individual level registry data on estate inventory reports. The data cover the universe of estates in Sweden during the studied period and contain essential information on the estate, such as its value and the existence of a written will or a marital agreement. The information allows me to measure the extent of tax planning, as well as to carry out a number of tests for heterogeneity in planning across heirs and to support the validity of the identification strategy.

The results show that estate size under-reporting was extensive; reported estate sizes were, on average, 17 percent lower, and about 26 percent more people reported estate sizes to be below tax liable values (SEK 280,000), as a result of tax planning. In addition, the under-reporting implied that the government’s revenues from the tax were reduced with up to 55 percent. The estimates are large, but similar to those found in earlier studies. For instance, Kopczuk (2007) finds that planning following the onset of terminal illness reduced estate sizes with 15–20 percent. The estimates of this study translate into a high elasticity of the estate size with respect to the net-of-tax rate. A high elasticity usually means that the estate size is sensitive to taxation and that the tax leads to people saving less. The elasticity of this study, however, does not capture such responses, only planning, which changes its interpretation. The interpretation of the large elasticity is instead that the tax is easy to plan for and that we can expect small adverse effects of the tax on people’s savings, because when the tax is easy to evade, the effective tax rate is lower and its distortions on savings are smaller. This is not to say that the tax is without problems: the planning may still imply a dead weight loss, and the fact that we only tax honest individuals is problematic in itself.

Further exploring the tax and the planning behavior, the study shows that the preferences and means for estate size under-reporting were widespread in the population; both those receiving large and small inheritances under-reported. However, the under-reported amount was larger for heirs receiving larger inheritances.
results complement the findings of previous studies, which tend to concern a small and very rich share of the population. The finding is also in line with tax evasion models, such as Yitzhaki (1987), showing that the under-reported amount increases with the tax rate. The results also show that heirs with children under-report more than heirs without children. This could suggest that there is a bequest motive for under-reporting, because to under-report when one inherits implies that there is more to leave for the child when you yourself pass away. It is also feasible that the surviving spouse, who is usually the sole heir of a married decedent, received help from the children in carrying out the under-reporting, which thereby became more successful. Additional results on heterogeneity show that heirs with high wealth appear to under-report more than other heirs, but that education and income matter little for the amount under-reported.

The outline of the paper is as follows. Section 2 describes the institutional setting. Section 3 describes the data, and Section 4 describes the empirical strategy. Section 5 presents the results, which are discussed in Section 6. Finally, Section 7 summarizes the conclusions.

2 Institutional setting

This section presents the institutional setting of this study. The purpose is to provide the background necessary for understanding the results and conclusions of the paper. It provides a short description of the Swedish succession rules and the Swedish inheritance tax. It also describes how the tax on spousal bequests was repealed and the means available to the decedent and the heirs to reduce tax payments.

2.1 Succession rules

According to the Swedish default rules of succession, the joint property of the decedent and the surviving spouse is divided in equal parts when a married person passes away. Half of the property becomes the property of the surviving spouse in the same way as if they had filed for divorce. The other half of the joint property as well as the decedent’s separate property constitute the estate. The estate is transferred in its entirety to the surviving spouse. The spouse does not inherit the estate with full ownership, only with the right of free disposal. This implies that the spouse can consume the estate, but never testate it. The reason for this

5. For a more detailed description of Swedish succession rules and inheritance tax law, see Brattström and Singer (2011) and Elinder, Erixson, Escobar, et al. (2014).
is that other default heirs of the decedent (primarily his or her children) receive a postponed right of inheritance at his or her demise, meaning that what remains of the estate when the surviving spouse eventually passes away is to be transferred to them.

A written will may set aside the default rules regarding the transfer and division of the estate. The decedent can impose another division between his or her heirs, or include other heirs to the estate, through a will. It is also possible to stipulate that a spouse should inherit with full ownership, and thus be able to give away or testate the received inheritance. However, the default heirs, typically the children, are always entitled to half of what they would have received in the absence of such a will. To enforce this rule, gifts that the decedents have made before passing away are considered inheritances in advance and deducted from the inheritance lot. Note that wills could be written for other reasons than to circumvent the default succession rules, for instance to determine that an heir should receive a certain asset.

2.2 The tax on spousal bequests

Before the Swedish inheritance tax repeal on January 1, 2004, bequests between spouses were liable to taxation. Tax liability was determined based on the date of death. A bequest was liable to taxation as long as the decedent leaving it passed away before January 1, but not if the decedent passed away on or after that date. The tax was an inheritance tax, meaning that it was based on the inheritance received by the spouse. However, it was similar to an estate tax as the default rule was for the estate to be transferred in full to the surviving spouse. The tax was calculated by the tax agency, using the estate inventory report. The heirs were responsible for writing and filing the report and were required to do so within three months of the decedent’s passing away.

The inheritance tax schedule was progressive. It had a basic deductible exemption that varied with the heir’s relationship to the decedent. For spouses, the exemption was SEK 280,000. The inheritance received by the spouse, net of this exemption, was taxed at a rate of 10 percent for amounts up to SEK 300,000, at 20 percent for amounts up to SEK 600,000 and at 30 percent for amounts larger than that.

The basic rule for valuation of estates was that all assets and debts should be taxed at their market value. However, the rule had several exceptions. For instance, small businesses were taxed at 30 percent of the value of their assets and inventories, net of debt, to simplify the transition of family firms. Real estate was targeted at 75 percent of its market value. A stock traded on the Stockholm stock
exchange main list was taxed at 75 percent of its market value, whereas Swedish stocks traded on other lists were taxed at only 30 percent of their market value. The estate was reported in tax values to the tax agency.

The tax on spousal bequests generated incomes of approximately SEK 430 million in 2003—an average tax payment of about SEK 15,000 per estate. Approximately 37 percent of the inheriting spouses paid taxes in that year.

2.3 The tax repeal

The tax on spousal bequests was repealed on January 1, 2004, implying that no spousal bequest, left by an individual who passed away on or after that date, was taxed. The repeal followed a relatively short political process. It was first suggested in an interim report presented by the Property Tax Committee in January 2003 (Egendomsskattekommittén 2004). It was then submitted to Parliament as part of the budget proposal, on October 22 (Regeringens proposition 2003/04:14), and voted into law on December 17.

Note that other heirs—for instance, children—had to pay taxes on inheritances until December 17, 2004, when inheritance taxes were abolished altogether. The second reform was the result of a decision separate from the repeal of the tax on spousal bequests. This second reform has been studied by, for instance, Erixson (2014) and Elinder, Erixson, and Waldenström (2016).

2.4 Planning to avoid the inheritance tax

The focus of this study is on estate size under-reporting. However, there were several ways for both the decedent and the surviving spouse to minimize tax payments. This section discusses the strategies tested for in Section 5, in order to support that the observed tax planning is indeed estate size under-reporting. The discussed strategies do not include all available planning responses to the tax; people could, for instance, also reduce their savings or transfer wealth through a series of small inter-vivos gifts over several years. However, they are the strategies most likely to be carried out in proximity to death.

Adding more heirs to the estate. Tax payments could be reduced through the addition of more heirs to the estate. Adding more heirs reduced the tax payments could be reduced through the addition of more heirs to the estate. Adding more heirs reduced the tax

6. Table A1 of Appendix A gives more details on valuation principles.
7. The numbers on total revenue and average tax payments, as well as the share of inheriting spouses paying taxes, are based on my own calculations.
8. People were allowed to receive gifts from one person of up to SEK 10,000 per year, tax exempted.
payments by making each inheritance small enough to be exempted from taxation (e.g., by making it smaller than SEK 280,000 for the surviving spouse), or by making the inheritances subject to a lower tax rate. This strategy required a will, written by the decedent, stating the additional heirs.

Marital agreements. Tax payments could be reduced by the use of marital agreements, which in turn reduced the estate size. As described, a division of a couple’s joint property is carried out upon the death of the first spouse, and a marital agreement can ensure that as much property as possible is owned separately by the surviving spouse, instead of being owned jointly. Separate property of the surviving spouse was not part of the decedent’s wealth and was not, during the tax regime, liable to inheritance taxation. A marital agreement must be written before the decedent’s demise, but not necessarily before entering a marriage.

Asset shifting. The asset valuation principles of the inheritance tax created an opportunity to reduce the estate’s tax value and thereby the tax payments through asset shifting. Assets with high tax values could be shifted into assets with low tax values; for instance, by selling stocks on the Stockholm main list (taxed at 75 percent of their market value) to instead buy Swedish stocks not traded on that list (taxed at 30 percent of their market value). This strategy had to be carried out by the decedent.

Under-reporting. The estate’s reported tax value and thereby the tax payments could be reduced through estate size under-reporting. There were many ways to illegally under-report the value of assets or to completely withhold assets from the estate report, as all assets of the estate were self-reported. However, even though all assets were self-reported in the estate report, the tax agency did have third-party information on some of the decedent’s wealth. Banks, other financial institutes and, for some assets, the national land agency, reported wealth holdings in shares, bonds, bank accounts and real estate to the tax agency for collection of wealth and property taxes. Thus, the tax agency had more possibilities to detect under-reporting of such assets. Under-reporting of other assets, however, was not as easy to detect. For instance, cash holdings, inventories (e.g., art, jewelry, etc.) and consumer durables (e.g., cars) were self-reported also with regard to the wealth tax, and therefore more difficult to observe. Note that this is the only strategy that may be carried out after the demise of the donor. Note also that the heir is the only one who can carry out under-reporting; the donor cannot under-report, as under-reporting is by definition carried out in relation to the estate value at the time of the donor’s demise. The donor could, however, to some extent help the surviving spouse’s under-reporting, by keeping wealth in assets that were easy to

This section describes the data used in the empirical analyses. It presents and discusses the key variables used for measuring tax planning. It also defines the relevant study population and describes the decedents of this population.

### 3.1 The Belinda database

The analyses use information from the Belinda database. The Belinda database is a population-wide, individual level register, covering the universe of estates in Sweden over the period from 2002 to 2004.\footnote{The database has been used before by, for instance, Erixson (2014), Erixson and Ohlsson (2014) and Elinder, Erixson, and Waldenström (2016). For a more detailed description of the data, see Elinder, Erixson, Escobar, et al. (2014).} It was collected by the tax agency and contains information from the estate reports. In particular, it lists the value of the estate, which is necessary for estimating the amount of under-reporting. It also lists all heirs to the estate, reports the value of separate and joint property, as well as indicates the presence of wills and marital agreements. The decedent and heirs are identified by a public identification number, which makes it possible to obtain information on the individual from other databases.

Data quality deteriorates after December 17, 2004. The inheritance tax was repealed for all heirs on that date, meaning that the tax agency no longer had any incentives for collecting data.

### 3.2 Definition of key variables

There are three main outcome variables of this study: Estate size, Taxable estate and Tax payments. They require some further explanation.

The outcome Estate size is obtained from the estate report and captures the decedent’s net worth at death. In the Belinda database, estate size is reported in tax value, which is also the relevant valuation principle for the outcome, as the tax value is what matters for inheritance taxation. Estate size is able to capture all forms of planning that reduce the estate’s taxable value. In particular, it captures estate size under-reporting.\footnote{What planning the outcome actually captures depends on the empirical strategy. It is thus able to capture all planning responses that reduced the tax value, but, as mentioned, it most likely only captures estate size under-reporting.} It is transformed using the inverse hyperbolic sine function.
transformation, which approximates the logarithm, but accounts for zeros.

The outcome *Taxable estate* is an indicator variable that takes the value one if the reported estate size is larger than SEK 280,000. This means that the variable indicates whether or not the estate is large enough to be taxed if transferred in its entirety to the surviving spouse, under the tax regime. It captures the extent to which heirs are able, or unable, to evade inheritance taxation completely.

The outcome *Tax payments* is defined as the sum of the tax payments made by all heirs to the estate. After the tax repeal, *Tax payments* is hypothetical and calculated in the same manner as if the tax on spousal bequests would still have been in place. In other words, it is calculated based on the inheritance received by each heir, using the tax schedule applicable before the tax repeal. The outcome captures the effect of tax planning on the heirs’ tax payments, and as taxes paid mirror the taxes collected, the variable also captures the effect of the tax planning on government revenues. Just as *Estate size*, the outcome *Tax payments* is transformed using the inverse hyperbolic sine transformation.

To conclude the variable description, note that the main outcome of this study is *Estate size*, as it captures the extent of estate size under-reporting. *Tax payments* complements *Estate size* by providing an estimate of how much tax revenues that are forgone as a result of planning, and *Taxable estate* complements *Estate size*, as it shows the extent to which individuals are able to fully evade the tax.

### 3.3 Study population

I restrict the Belinda database to the relevant population of study: the population affected by the tax repeal. It is thus restricted to married individuals who passed away before and after the repeal (in 2003 or 2004), a total of 61,201 observations. The population is further restricted to individuals whose surviving spouses’ had incentives to under-report (i.e., individuals with wealth exceeding the tax threshold), when they passed away. To construct an exogenous measure of this, I use the decedent’s wealth in 2002, information on which is obtained from the Swedish registry of wealth (*Förmögenhetsregistret*). An estate is considered taxable (there were incentives for tax planning) if a decedent had net wealth in 2002 that would

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12. The full population of married individuals passing away in 2003 and 2004 was 62,296 individuals. However, for 976 observations, the date of death is missing, and for another 119 observations, information on the cause of death is missing. Information on the cause of death is required for some of the tests to determine how people planned.

13. Note that wealth in the wealth register is measured at December 31, each year. Thus, wealth in 2002 is measured approximately one year before death, both for individuals passing away just before and just after the tax repeal.
have implied inheritance tax payments for the spouse had the decedent died in that year. In other words, it is considered taxable if the decedent’s wealth in 2002 was larger than SEK 280,000. This restriction to individuals with incentives to under-report leaves 29,923 observations.

3.4 Descriptive statistics

This section presents descriptive statistics for decedents passing away before and after the tax repeal. The purpose is to provide a picture of the studied individuals. In particular, I want to show how the decedents’ wealth while alive relates to their reported estate size, and also to provide points of reference for the discussion of the results. Note that the economic variables—income and wealth—are measured on December 31, 2002 (i.e., one year before the demise of individuals passing away close to January 1, 2004).

The descriptive statistics are presented in Table 1. The average estate size among the studied decedents is about SEK 570,000 before the reform, and about SEK 700,000 after the reform. That it is lower before the tax repeal is probably to some extent due to under-reporting. The average wealth is about SEK 1,100,000. It is important to note that the estate sizes are given in tax value, whereas the wealth is given in market value. This is likely to explain some of the difference between estate size and wealth. Using the wealth portfolio of the decedent to translate the estate’s assets into their tax values results in the decedent’s wealth instead being about SEK 900,000. This is still larger than the estate size, which is probably due to some, or all, of the wealth captured by the variable being marital property and that the wealth variable does not take into account the division of joint property, carried out upon death of a married individual.

14. Note that as opposed to the variable estate size, which is given in tax value, wealth in 2002 represents the market value of all the decedent’s assets. Furthermore, the wealth in 2002 reflects the individual’s wealth before the division of the estate that is carried out at the passing away of the first spouse. For these reasons, the wealth in 2002, as captured by the wealth variable, may deviate from what the estate size of that individual would have been had he or she actually passed away in 2002. Thus, it is not a perfect measure of individuals’ tax incentives and wealth before death, but it provides an approximation of these factors. For more discussion on this, see next subsection.

15. I use the 10,486 individuals fulfilling these criteria, who passed away in 2002 as well as a sample of non-married decedents for placebo analyses. More details on these populations are provided in Appendix B.

16. It is impossible to perfectly translate wealth into what the estate size would have been had the individual passed away in 2002, as the tax value of some assets in the estate report (small business and co-operative building society housing) is not related to their market value, making it impossible to say what their tax value would have been. In addition, wealth is measured before
Table 1: Descriptive statistics of the decedents

<table>
<thead>
<tr>
<th></th>
<th>Before repeal</th>
<th>After repeal</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>76.69</td>
<td>75.67</td>
</tr>
<tr>
<td>University education</td>
<td>15.55</td>
<td>16.64</td>
</tr>
<tr>
<td>Female</td>
<td>23.76</td>
<td>24.13</td>
</tr>
<tr>
<td>Wealth</td>
<td>1,108,607</td>
<td>1,144,610</td>
</tr>
<tr>
<td>Income</td>
<td>160,694</td>
<td>166,594</td>
</tr>
<tr>
<td>Estate size</td>
<td>571,617</td>
<td>702,718</td>
</tr>
<tr>
<td>Observations</td>
<td>14,902</td>
<td>15,021</td>
</tr>
</tbody>
</table>

*Note:* Female and university education are in percent. Wealth is in market value. Wealth and income are in SEK, nominal values, and measured in 2002. Income includes retirement income. Estate size is in taxable value and measured in SEK, nominal values. Age is measured in 2002. The first column, Before repeal, includes decedents in 2003, and the second column, After repeal, includes decedents in 2004.

Some additional statistics are presented in Table 1. The decedents passing away in 2003 are, on average, 77 years old, whereas those passing away in 2004 are, on average, 76 years old. The difference follows from age being measured in the same year for both groups, 2002, which means that an individual, on average, is younger at that time, the later he or she passes away. The share of females is about 25 percent, which also follows from the restriction to married decedents. Wives tend to outlive husbands and there are therefore fewer women than men in this population. Income (including retirement income) is SEK 160,000–170,000 for decedents passing away before and after the repeal. The average income is low compared to the national average, as many decedents are retired. The share with university education is about the same in both years, approximately 16 percent.

17. University education is an indicator variable taking the value one if an individual has any university education.
4 Identification strategy

This section describes the identification strategy. First, it describes the RD design, used for quantifying the extent of under-reporting. Then it describes its implementation. It also discusses the identifying assumptions, presents potential threats to them and shows that these are unlikely to be present.

4.1 How tax planning is measured: The RD design

The main issue in studies on tax planning in general, and under-reporting in particular, is observing it. Under-reporting is illegal, which makes people reluctant to reveal it to the tax agency or report it to surveys. Instead, to measure it, I compare the estates of individuals passing away before the inheritance tax repeal of January 1, 2004, to estates of individuals passing away on or after that date. That is, I compare estates for which there are incentives to under-report with estates for which there are no such incentives, and infer the extent of under-reporting from how they differ from each other. The change, at the date of the repeal, in the outcome $\text{Estate size}$, and in the outcome $\text{Taxable estate}$, captures under-reporting, while the change in $\text{Tax payments}$ captures the effect of under-reporting on tax payments and government revenues.$^{18}$

To identify the extent of under-reporting, and the extent of under-reporting only, requires that the compared individuals are similar, so that the estate size of those passing away before and after the repeal would have been the same had the estates before the repeal not been under-reported. To ensure this, the extent of under-reporting is estimated using a RD design. The RD design accounts for possible differences between the studied estates by comparing individuals passing away close to the repeal. It accounts for such differences, because if people are unable to perfectly control the date on which they die, then it is essentially random whether an individual passing away close to the reform dies before or after it, and this randomness ensures that the differences in estate size and probability of paying taxes are due to under-reporting only.

Simply described, estimating the extent of under-reporting using a RD design implies that the relationship between estate size (or the other main outcomes) and date of death is estimated separately for individuals passing away before and after the tax repeal. Both of these estimations provide predictions with regard to the estate size of individuals passing away on the date of the repeal, and the difference between the two predictions is the estimated extent of tax planning.

18. Remember that $\text{Taxable estate}$ and $\text{Tax payments}$ were hypothetical for those passing away after the repeal, see Section 3 for details.
Going into the specifics of the RD design, there are two methods of estimation to choose from: the parametric and the non-parametric approaches. The parametric approach imposes a parametric form on the relationship between the outcome of interest and date of death, and estimates the regression model using all available observations. The non-parametric approach, on the other hand, applies local linear regression and estimates the relationship using observations closer to the reform. Using observations closer to the reform—or in RD terminology: narrowing the bandwidth—results in the relationship becoming more likely to be approximately linear. The main advantage of the parametric approach is that it uses all available data, which gives more statistical power. But as shown by Gelman and Imbens (2014), its use of higher order polynomials makes the estimation sensitive to bias. Thus, given that the number of observations is sufficient for maintaining precision with less data, the non-parametric approach is preferable, and that is the one I use for my estimations.

The non-parametric approach is implemented using local linear regression, which means that it is implemented by running a linear regression that is weighted—in this case, using a triangular kernel. The triangular kernel assigns a weight to each observation that is linearly decreasing with the distance between the demise and the reform date. This implies that the estimation gives less importance to an individual the farther away from the reform he or she passes away. The triangular kernel has been proved optimal for local regression at boundary points (see, for instance, Lee and Lemieux (2010) for a discussion on this).

In tables showing the results, the presented estimates are obtained using bandwidths selected according to the criteria of Calonico et al. (2014b) and the software RD-Robust (Calonico et al. 2014a). For completeness, I also show estimates obtained using other bandwidths and polynomial degrees, primarily, in Appendix D and E. The presented results are estimated without control covariates.

4.2 Threats to identification

As mentioned above, the identification strategy requires that estate sizes of individuals passing away before and after the repeal would have been the same had the estates of those passing away before the repeal not been under-reported. This section discusses one of the main threats to that assumption, namely sorting, or people’s ability to control their date of death. Presented below is a summary of a more extensive discussion on this issue, reported in Appendix C. The appendix also presents detailed results of the tests.

Sorting, in this setting, is when individuals adapt their behavior in anticipation of the reform and manage to control the date on which they pass away. Consider,
for instance, individuals who are wealthy and mortally ill close to January 1, 2004. They have strong incentives to survive until after the repeal and thereby avoid taxation. If this happens, people passing away just after the reform will be wealthier than those passing away just before it, and their estate sizes will differ for reasons other than under-reporting. The problem may seem unlikely, but it has been established in several studies (Kopczuk and Slemrod 2003; Gans and Leigh 2006). Eliason and Ohlsson (2008, 2013) even find evidence of sorting for the reform at hand. However, the sorting does not necessarily invalidate the identification strategy. Lee and Card (2008) show that a RD design can produce unbiased estimates in the presence of sorting, as long as individuals are unable to perfectly control their date of death (i.e., as long as the sorting is partial). I therefore study the extent of sorting to see whether or not it should be deemed partial, and thus whether or not it is likely to bias the estimates on tax planning, using two strategies. The first strategy analyzes the number of people passing away on each day close to the tax repeal. The relative number of people passing away just after rather than just before the repeal gives an indication of how easy it was to sort. The second strategy estimates differences in predetermined characteristics between the decedents passing away just before and just after the repeal. If it is essentially random whether people pass away before or after the repeal, we should expect no differences in these characteristics.

There are two formal ways of evaluating how many people that pass away just after rather than just before the repeal: the tests of McCrary (2008) and the test of Cattaneo et al. (2015). In essence, both tests apply an RD design on the date of the reform, using the number of deaths per day as outcome. Reassuringly, none of the tests is able to estimate a statistically significant difference in the number of individuals passing away before and after the reform. This does not contradict the finding of Eliason and Ohlsson (2008, 2013), who showed that there was sorting. It only means that the sorting was not extensive enough to be captured by the tests, which suggests that relatively few individuals were able to postpone their death and that the sorting is partial.

The second strategy acknowledges that the individuals who, after all, postpone death may differ from those who do not postpone their death. In particular, they are likely to be wealthier, as being wealthier gives them stronger incentives to survive the tax repeal. If they are wealthier, their wealth may translate into higher estate values and bias the estimates on under-reporting. It is thus useful to test for differences in predetermined characteristics directly, and to do this, I apply the

19. This could be achieved not only through actual prolonged survival, but also through falsely stating the timing of death in the death certificate.
RD design on the population of study, but with predetermined characteristics as outcomes. The predetermined characteristics include wealth, income, education, sex and age. Reassuringly, the test is unable to estimate any effects of economic or statistical significance. Again, this does not mean that there is no sorting, but it suggests that the sorting is partial and unlikely to bias the estimates on under-reporting to any larger extent.

To summarize, I am unable to show that individuals perfectly control whether they pass away before or after the tax repeal. This suggests that the estimates I present on the extent of under-reporting are causally identified. For now, I settle with these tests. However, to ensure that the estimates indeed are causal, I complement the main results in the next section with additional specification tests.

5 Results

This section presents the estimates of estate size under-reporting. It then presents the results from a number of robustness and specification tests, which show that the main findings are relatively robust to the choice of bandwidth and local polynomial degree, and also that possible sorters are unlikely to invalidate the identification strategy. Then follow a number of tests to ensure that the lower reported estate values are due to under-reporting, and not to other inheritance tax planning strategies. To understand the implications of the inheritance tax and inheritance tax planning, estimates are also presented on how the extent of planning is related to the estate size, and to the characteristics of the under-reporting heir.

5.1 Main results: the extent of under-reporting

The extent of under-reporting is captured by the change in reported estate size due to the tax repeal. We see the relationship between estate size and the date of the donor’s demise in Figure 1a. The figure suggests that reported estate sizes were substantially lower before than after the repeal. The estimate of this difference, obtained using the RD design, is presented in the first column of Table 2. The estimate is -0.17, implying that estate sizes were under-reported with about 17 percent, on average, as a result of the inheritance tax.

The extent to which people were able to evade the inheritance tax completely is captured by how much the share of estates reported to a taxable value (SEK 280,000) is reduced as a result of the tax. We see the relationship between the probability of reporting an estate to a taxable value and the date of the donor’s
demise in Figure 1b. There appears to be a shift in the incidence of estates in excess of SEK 280,000 on the reform date. The estimate reported in the second column of Table 2 confirms this and suggests that people dying before the reform, rather than after it were 6 percentage points less likely to leave behind an estate of taxable value. The result implies that the share of estates completely evading tax payments increased with 26 percent.

The effect of under-reporting on tax payments and government revenues is captured by the change in the outcome *Tax payments* on the date of the tax repeal. Remember that tax payments are hypothetical after the tax repeal and calculated using the tax schedule applicable before it. We see the relationship between this outcome and the date of the donor’s demise in Figure 1c. The outcome appears to be considerably smaller before than after the reform. In the third column of Table 2, we see that the estimated effect of the reform is -0.78. This suggests that the tax payments were 55 percent lower, as a result of the under-reporting. The tax payments being 55 percent lower, on average, also implies that the revenues from the tax were 55 percent lower.

Table 2: Estimates on the extent of tax planning: main outcomes

<table>
<thead>
<tr>
<th>Treatment effect</th>
<th>Tax payment</th>
<th>Taxable estate</th>
<th>Estate size</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-0.172**</td>
<td>-0.0611***</td>
<td>-0.781***</td>
</tr>
<tr>
<td></td>
<td>(0.0670)</td>
<td>(0.0190)</td>
<td>(0.192)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>105</td>
<td>113</td>
<td>147</td>
</tr>
<tr>
<td>Observations</td>
<td>8,908</td>
<td>9,536</td>
<td>12,168</td>
</tr>
</tbody>
</table>

Note: Standard errors in parenthesis. Significance stars *, **, *** denote p < 0.10, p < 0.05 and p < 0.01, respectively. Outcome variables are transformed using the inverse hyperbolic sine transformation, except taxable estate which is binary. The estimates are obtained using local linear regression. Bandwidth chosen following Calonico et al. (2014b). For robustness to the choice of bandwidth and polynomial degree, see Figure 2 as well as E1 to E3 of Appendix E.

5.2 Robustness and placebo tests

This section presents the results of a number of robustness checks, as well as three additional tests of the identifying assumptions. The robustness checks are carried out to make sure that the main estimates are not sensitive to the choice of estimation bandwidth and local polynomial degree. The purpose of the three addi-
Figure 1: Relationship between main outcomes and date of death around the tax repeal. Outcomes are averaged at week level. Date is relative to January 1, 2004 (Date=0). For graphs averaging the outcomes by day (lowest level of aggregation), see Figure D1 of Appendix D. Subfigure a shows relationship between date of death and (inverse sine) estate size. Subfigure b shows relationship between date of death and probability of reporting a taxable estate. Subfigure c shows relationship between date of death and (inverse sine) tax payment.
tional tests of the identifying assumptions is to further evaluate the identification strategy.

Robustness to bandwidth and polynomial choice  The effect of the reform is estimated using a wide set of bandwidths, as well as a second and a third order local polynomial, to ensure that the main estimates are reasonably robust to the choice of bandwidth and polynomial degree. Estimates on under-reporting, obtained using local linear regression for a wide range of bandwidths, are shown in Figure 2. Detailed results of the robustness checks can be found in Appendix E.1. The tests show that the estimated effects are negative for all outcomes and almost all bandwidths, with the few positive point estimates found all being statistically insignificant. In addition, the point estimates of the RD design on the outcomes Estate size and Taxable estate vary only slightly with the polynomial and bandwidth choice, see Figure E1 and E2 of Appendix E.1. The estimates of the RD design on the outcome Tax payments, on the other hand, vary more, but are qualitatively robust, see Figure E3 of Appendix E.1. Thus, the extent of under-reporting appears to be precisely estimated, whereas the effect of under-reporting on tax payments appears to be less so.

Evaluating the possible impact of sorting.  As mentioned in Section 4, previous research has reported evidence of sorting around the reform date (Eliason and Ohlsson 2008, 2013). To test for whether sorters may bias the main estimates, I remove individuals who passed away in the days close to the reform. This approach is commonly referred to as a RD-donut design. The logic here is that sorters are likely to pass away close to the reform, and removing all individuals passing away in days close to the reform removes the possible sorters from the estimations. Guided by Eliason and Ohlsson (2008, 2013), I remove individuals passing away within two weeks before the repeal and one week after. Removing these observations has little effect on the estimates, suggesting that possible sorters at most contribute with a small bias to the results. Detailed results of the test can be found in Appendix E.2.

Testing for effects in previous years.  There is a risk of estimating an effect of the tax repeal when there is no effect if wealthy individuals tend to pass away

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20. Almost all estimates for estate size are in the range -0.12 to -0.22. Almost all estimates on the share of taxable estate are in the range -0.05 to -0.08.
21. The estimates range from -0.25 to -0.6.
22. The same approach have been used by, for instance, Dahl et al. (2013) for avoiding bias from strategic timing of births.
Figure 2: Estimates on the extent of tax planning by bandwidth. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Bandwidth ranges from 15 to 350 days. Estimates are obtained using local linear regression.
early in the year and less wealthy individuals tend to pass away late in the year. To ensure that this is not the case, I implement a placebo reform pretending that there was a tax repeal on January 1, 2003. Finding an effect of this placebo reform would suggest that there are mortality patterns—such as the one described above—introducing bias to the results. However, the tests show no difference in tax payments, the share of taxable estates or in estate size, which suggests that there is no general tendency of wealthy individuals passing away early in the year. Detailed results of the tests can be found in Appendix E.3.

**Testing for other events possibly affecting the outcomes.** There is also a risk of estimating an effect of the tax repeal when there is no effect, if the identification strategy captures changes, close to January 1, 2004, other than the one studied, which also affect the estate size, such as legislation or tax reforms. I look for such changes by estimating the effect of the tax repeal on two populations of decedents not affected by the tax repeal. The first population consists of non-married decedents and the second consists of decedents whose wealth in 2002 was lower than SEK 280,000. The first population is unaffected by the reform, as its decedents have no surviving spouses to be concerned by the repeal. The second population is unaffected by the reform as its decedents had wealth lower than the tax threshold and their heirs thus lacked incentives to under-report. Finding an effect in these populations would indicate that the identification strategy captures more responses than tax planning, and that the estimates are biased. The tests on the population of non-married decedents show no such effects and thus speak in favor of the validity of the research design. The test on the population with non-taxable wealth shows no effect on estate size, a finding supporting the research design. There are some effects on tax payments and the share of taxable estates. However, these seem to depend on the definition of non-taxable wealth being inexact and that many individuals in the non-taxable population in fact had tax liable estates and incentives to reduce their tax payments. Detailed results and additional discussion concerning the tests can be found in Appendix E.4.

To conclude this subsection, the estimates on estate size and share of taxable estates are robust to the choice of local polynomial degree and bandwidth. The estimates on tax payments are more sensitive and, though the sign of the estimates are consistently negative, claims about their exact size require caution. The tests of the identifying assumptions generally speak in favor of the empirical design. The overall impression, therefore, is that the identification strategy is valid, especially if we consider the specification tests in Section 4, which showed little, if any, evidence of more than partial sorting and no signs of differences in predetermined
characteristics between those passing away before and after the reform, not even in such an essential characteristic as wealth.

5.3 Testing for other tax planning strategies

The main estimates show that, as a consequence of the inheritance tax, people under-reported estate sizes with, on average, 17 percent, and their probability of reporting an estate size of taxable value was reduced by 6 percentage points. In addition, this under-reporting translated into a reduction in tax payments and government revenue from the tax of up to 55 percent. This section continues by testing for other margins of tax planning to show that the captured response indeed constitutes under-reporting. A number of tests are carried out: one trying to find evidence of real responses (e.g., consumption), one trying to find responses in will-writing, marital agreements and the number of heirs to the estate, and one trying to find responses in asset shifting. None of the tests show evidence of other tax planning strategies being captured by the RD design.

5.3.1 Reducing wealth

The first test looks for responses with an impact on the value of the decedent’s wealth at death. As opposed to responses reducing the reported estate size, these responses reduced the actual wealth transferred to the surviving spouse. This could be real responses, because even though changes in wealth accumulation are unlikely to be captured by the research design, people could react to imminent death through consumption, as consumption becomes less costly relative to leaving bequests when there is an inheritance tax.

To test for such responses, I estimate the RD design, using the wealth of the surviving spouse in the years following the inheritance as outcomes. We see the results of this in Table 3. The point estimates are negative and about -0.05, suggesting that the surviving spouses of those passing away before the repeal had five percent lower wealth than the surviving spouses of those passing away after the repeal. However, the estimates of the responses are not statistically significant in any of the years. Thus, I cannot establish any important responses of this kind.

23. This also suggests that the tax had small effects on the surviving spouses’ reported wealth in years after the inheritance.
Table 3: Effect of the under-reporting on heirs’ wealth in years following the inheritance receipt (2005–2007)

<table>
<thead>
<tr>
<th>Year</th>
<th>Treatment effect</th>
<th>Bandwidth</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>2005</td>
<td>-0.0541 (0.0415)</td>
<td>136.6</td>
<td>11,048</td>
</tr>
<tr>
<td>2006</td>
<td>-0.0538 (0.0379)</td>
<td>155.6</td>
<td>12,440</td>
</tr>
<tr>
<td>2007</td>
<td>-0.0590 (0.0387)</td>
<td>147.9</td>
<td>11,814</td>
</tr>
</tbody>
</table>

Note: Standard errors in parenthesis. Significance stars ***, *** denote $p < 0.10$, $p < 0.05$ and $p < 0.01$, respectively. Heir’s wealth is transformed using the inverse hyperbolic sine transformation. Estimates obtained using local linear regression. Bandwidth chosen following Calonico et al. (2014b). For robustness to bandwidth selection, see Figure D2 of Appendix D.

5.3.2 Planning through marital agreements and inclusion of more heirs to the estate

The second test looks for tax planning through marital agreements and inclusion of more heirs to the estate. A marital agreement could lower the tax payments, as it converts assets belonging to both the decedent and the surviving spouse into assets belonging only to the surviving spouse. In other words, marital property is turned into the separate property of the surviving spouse, which is not part of the estate and therefore not taxed. Adding more heirs to the estate could make the tax payments lower, as this makes each inheritance lot smaller. It results in them being taxed at a lower tax rate, or not at all, if their value is lower than the exemption level. This strategy required a written will.

I test for these strategies by implementing the RD design on three outcomes: an indicator for marital agreements, an indicator for the presence of a written will and a variable on the number of heirs to the estate. The outcomes are obtained from the estate reports.

Figure 3 shows the relationship between date of death and the frequency of marital agreements, wills and the number of heirs to each estate. The results of the estimations are shown in Table 4. The RD design estimations do not show any effects along these margins, which suggests that the lower tax payments and estate sizes found were not due to these strategies. However, it is interesting to note the downward trend in the number of heirs after the reform, visible in Figure 3b.
trend suggests that the number of heirs was higher because of the inheritance tax and that this was in fact a tax planning strategy. However, as there is no estimated effect, and as there is no apparent level shift in the outcome on January 1, 2004, it is not part of the response captured by the RD design. This is probably explained by the fact that it takes time to adapt these strategies to the new tax regime.

Figure 3: Relationship between means of tax planning and date of death around the tax repeal. Outcomes are averaged at week level. Date is relative to January 1, 2004 (Date=0). Subfigure a shows the relationship between date of death and the probability of the decedent having a will. Subfigure b shows the relationship between date of death and the number of heirs related to the estate. Subfigure c shows the relationship between date of death and the probability of the decedent having a marital agreement.
Table 4: Effect of the tax repeal on means of tax planning

<table>
<thead>
<tr>
<th></th>
<th>No. of heirs</th>
<th>Will</th>
<th>Marital agreement</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment effect</td>
<td>0.0321</td>
<td>0.000444</td>
<td>-0.00816</td>
</tr>
<tr>
<td></td>
<td>(0.0595)</td>
<td>(0.0189)</td>
<td>(0.0136)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>107</td>
<td>126</td>
<td>106</td>
</tr>
<tr>
<td>Observations</td>
<td>8,307</td>
<td>10,560</td>
<td>8,970</td>
</tr>
</tbody>
</table>

Note: Standard errors in parenthesis. Significance stars *, **, *** denote $p < 0.10$, $p < 0.05$ and $p < 0.01$, respectively. Bandwidth chosen following Calonico et al. (2014b). Estimates obtained using local linear regression. For robustness to bandwidth selection, see Figure D3 of Appendix D.

5.3.3 Planning through asset shifting

The third test looks for planning through asset shifting. Asset shifting was legal and could be carried out by the donor. It lowered the taxable value of the estate, as the tax value of some assets in the estate was lower than their market value. Ideally, a test for this strategy would use the asset composition of the estate. Unfortunately, such data do not exist with respect to these estates.

The test instead acknowledges that asset shifting had to be carried out before the donor passed away. Thus, with little or no time to plan, asset shifting was unlikely. Also, as pointed out by Kopczuk (2007), there is a large body of literature showing that people tend to postpone decisions concerning their death for as long as possible. It is thus less likely that people have carried out this strategy when their death is not imminent. The test splits the population into two groups, based on whether or not the decedent passed away suddenly, and estimates the RD design separately on each group. Deaths are considered sudden if the decedent was not diagnosed with the same illness he or she died from, before death. The relevant outcome is the estate size, as it captures asset shifting. The test assumes that individuals who passed away suddenly did not have time shift assets. Or rather, that individuals passing away suddenly had less time to respond to the tax repeal, and that the extent of asset shifting therefore is more likely to be accounted for by the RD design. If the estimated effect on estate values is lower among individuals passing away suddenly, then some of the effect is likely to come

24. In the second chapter of this thesis, it is shown that little planning is carried out by the decedent.
25. For details on the definition, see Appendix F.
from asset shifting rather than from planning carried out after the donor’s demise.

We see in Table 5 that the point estimates in the population of sudden deaths generally are larger, but less precise, than the effects in the non-sudden population. This is the opposite of what we would expect if asset shifting explained the main results.

Table 5: Estimates on the extent of under-reporting with respect to estates of individuals passing away suddenly and non-suddenly

<table>
<thead>
<tr>
<th></th>
<th>Non-sudden</th>
<th>Sudden</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment effect</td>
<td>-0.160*</td>
<td>-0.345*</td>
</tr>
<tr>
<td></td>
<td>(0.0823)</td>
<td>(0.187)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>80</td>
<td>86</td>
</tr>
<tr>
<td>Observations</td>
<td>5,803</td>
<td>1,167</td>
</tr>
</tbody>
</table>

Note: Standard errors in parenthesis. Significance stars *, **, *** denote \( p < 0.10 \), \( p < 0.05 \) and \( p < 0.01 \), respectively. The outcome is (inverse sine) reported estate size. Estimates obtained using local linear regression. Bandwidth chosen following Calonico et al. (2014b). For robustness to bandwidth selection, see Figure D4 of Appendix D.

5.4 Distribution of under-reporting in the population

This section presents estimates of heterogeneity in estate size under-reporting along a number of dimensions. The dimensions are based on the estate size, as well as characteristics of the under-reporting surviving spouse.

To determine whether under-reporting was widespread or confined to a small group of individuals receiving large inheritances, I investigate the relationship between the estate size and the extent of under-reporting. The Swedish inheritance tax covered a comparatively large share of the population, which allows me to estimate the extent of tax planning among people with relatively low wealth. This is a contribution to previous studies, which have only analyzed tax planning related to large estates. This means that I am able to contribute with knowledge on the preferences, incentives and possibilities for planning in a broad segment of the population.

To investigate this, I estimate the effect of the tax on the estate size for subgroups of the population, defined by the estate size before under-reporting. As the size of the estate before under-reporting is unknown, the donor’s wealth in 2002
is used as an approximation for this. Each estate is then assigned to a subgroup based on the tax bracket it would have belonged to had the decedent passed away in 2002 and had the estate been passed in its entirety to the surviving spouse.

We see the effect of taxes on (inverse sine) reported estate size by bracket in Table 6. The results are inconclusive. The estimates on (inverse sine) estate size are about -0.14 and statistically insignificant for the first and third bracket. For the second bracket the estimate is -0.34 and statistically significant ($p < 0.01$). The estimates being similar in the first and third bracket suggests that under-reporting in relative terms does not increase with the tax rate. However, this appears to be the case with under-reporting in absolute terms. For estates in the first tax bracket, the under-reported amount is approximately SEK 45,000. For estates in the second tax bracket, the under-reported amount is about SEK 126,000. Both these estimates are statistically significant ($p < 0.01$). For estates in the third bracket, the amount under-reported is approximately SEK 115,000. However, the estimate for the third bracket is imprecise.

Combined, the results suggest that preferences for under-reporting are present among all individuals with incentives to evade the tax. However, those receiving larger estates under-report more in absolute amounts. This could be explained by the fact that they contain more assets to under-report, but also by the gains from under-reporting increasing in relation to the tax rate. This is further discussed in Section 6.

A number of surviving spouse characteristics may affect the extent of estate size under-reporting. Table 7 presents estimates on how under-reporting relates to the surviving spouse’s wealth, income and education, as well as to whether or not he or she has children. Regarding university education, we see that the point estimate for those with such education is more negative than the point estimate for those without. However, the estimate concerning those with university education is not statistically significant. In addition, estimates on under-reporting in the two groups obtained using other bandwidths are more similar, as can be seen in Figure D7 of Appendix D.

Regarding income, we see that the point estimate with respect to the under-reporting of surviving spouses with high incomes is more negative than with respect to under-reporting of those with low income. But similar to what we saw concerning education, this relationship is not robust to the bandwidth choice, as can be seen in Figure D8 of Appendix D. There is thus little evidence to suggest that the extent of under-reporting differs with respect to the surviving spouse’s education.

26. Division choices may be endogenous to the tax system.
Table 6: Extent of under-reporting by tax bracket

<table>
<thead>
<tr>
<th></th>
<th>1st</th>
<th>2nd</th>
<th>3rd</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Inverse sine</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment effect</td>
<td>-0.142</td>
<td>-0.340***</td>
<td>-0.146</td>
</tr>
<tr>
<td></td>
<td>(0.141)</td>
<td>(0.0926)</td>
<td>(0.0907)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>89</td>
<td>59</td>
<td>80</td>
</tr>
<tr>
<td>Observations</td>
<td>2,886</td>
<td>1,087</td>
<td>2,831</td>
</tr>
<tr>
<td><strong>Level</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment effect</td>
<td>-45037.0***</td>
<td>-126954.4***</td>
<td>-115149.3</td>
</tr>
<tr>
<td></td>
<td>(13712.8)</td>
<td>(31144.9)</td>
<td>(134222.5)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>131</td>
<td>74</td>
<td>155</td>
</tr>
<tr>
<td>Observations</td>
<td>4,102</td>
<td>1,320</td>
<td>5,191</td>
</tr>
</tbody>
</table>

*Note:* Standard errors in parenthesis. Significance stars *, **, *** denote $p < 0.10$, $p < 0.05$ and $p < 0.01$, respectively. The outcome is estate size (inverse sine or level, as indicated) in all estimations. Estimates obtained using local linear regression. Bandwidth chosen following Calonico et al. (2014b). For robustness to bandwidth selection, see Figure D5 and D6 of Appendix D.
and income.

There appears to be some heterogeneity in the extent of under-reporting based on whether or not the surviving spouse has children. The estimates of Table 7 suggest that surviving spouses with children under-report, but that those without children do not. As seen in Figure D9 of Appendix D, this finding appears to be robust to the choice of estimation bandwidth. The estimates could be the result of a bequest motive; that the surviving spouse cares about the welfare of his or her children and therefore wants to withhold as much wealth as possible for taxation, because the more that is withheld from taxation, the more can be transferred to the children when the surviving spouse eventually passes away. This interpretation is in line with the reasoning of, for instance, Hurd (1987), who argues that having children is an indication of bequest motives. Another explanation for the result may be that the child of the surviving spouse is involved in the under-reporting. The surviving spouse and the child have aligned motives for reducing the inheritance tax payment, and there is no conflict between how much the surviving spouse receives and how much the child will receive when the wealth is transferred to him or her upon the surviving spouse’s demise. The combined effort of the surviving spouse and the children may lead to greater under-reporting.

The table also suggests that surviving spouses with high wealth under-report more than others. This finding is also robust to the choice of estimation bandwidth, as can be seen in Figure D10. A possible explanation for the difference is the Swedish wealth tax, in place at the time, and that wealthier individuals have stronger incentives to under-report the estate due to that tax. There are clear incentives to conceal received wealth that would imply liability for the wealth tax, from the tax agency. In addition, Seim (2017) shows that there was under-reporting with respect to the wealth tax in Sweden, and it is possible that knowledge about how to under-report assets is more widespread among individuals who have under-reported to evade the wealth tax.

27. Kopczuk and Lupton (2007), however, show that bequest motives are common among individuals without children as well, thus making having children a weaker indicator for their presence.
Table 7: Heterogeneity in under-reporting of estate size

<table>
<thead>
<tr>
<th></th>
<th>Wealth</th>
<th></th>
<th>Income</th>
<th></th>
<th>University education</th>
<th></th>
<th>Child</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>High</td>
<td>Low</td>
<td>High</td>
<td>Low</td>
<td>With</td>
<td>With</td>
<td>With</td>
<td>Without</td>
</tr>
<tr>
<td>Treatment effect</td>
<td>-0.292***</td>
<td>-0.0311</td>
<td>-0.210**</td>
<td>-0.149</td>
<td>-0.300</td>
<td>-0.163**</td>
<td>-0.253***</td>
<td>0.207</td>
</tr>
<tr>
<td></td>
<td>(0.108)</td>
<td>(0.0940)</td>
<td>(0.0929)</td>
<td>(0.112)</td>
<td>(0.208)</td>
<td>(0.0806)</td>
<td>(0.0829)</td>
<td>(0.245)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>90</td>
<td>76</td>
<td>76</td>
<td>90</td>
<td>80</td>
<td>80</td>
<td>80</td>
<td>82</td>
</tr>
<tr>
<td>Observations</td>
<td>3,675</td>
<td>3,407</td>
<td>3,229</td>
<td>3,912</td>
<td>1,046</td>
<td>5,834</td>
<td>4,373</td>
<td>2,569</td>
</tr>
</tbody>
</table>

Note: Standard errors in parenthesis. Significance stars *, **, *** denote $p < 0.10$, $p < 0.05$ and $p < 0.01$, respectively. The outcome is (inverse sine) reported estate size. High (low) income and wealth refers to income and wealth greater (lower) than median income and wealth in the population of study. Estimates obtained using local linear regression. Bandwidth chosen following Calonico et al. (2014b). For robustness to bandwidth selection, see Figure D7 to D10 of Appendix D.
6 Discussion

This section discusses some implications and interpretations of the results. First, it discusses the sensitivity of the tax base and what implications this has for the possibility to tax inheritances. Then, it discusses how under-reporting is related to the tax rate, and how the results can be generalized.

6.1 How sensitive is the estate size to taxation?

An important question in studies on taxation is on how much the tax base changes because of the tax rate. In this case, the question would be: how sensitive is the reported estate size to taxation?\(^{28}\) The question matters, as it helps in determining how much revenues the tax can raise and how large the distortions of the tax might be.

To obtain a measure of this sensitivity, I calculate the elasticity of estate size with respect to the net-of-tax rate.\(^{29}\) Formally, the elasticity is \(\epsilon = \frac{\Delta \ln E}{\Delta \ln(1 - \tau)}\), where \(E\) is the reported estate size, \(\tau\) is the average tax rate and \(\Delta \ln(1 - \tau) = \ln(1 - \tau_2) - \ln(1 - \tau_1)\). After the reform, the average tax rate, \(\tau_2\), was zero, as the tax had been repealed. Before the reform, the average tax rate, \(\tau_1\), was 0.12 in the studied population.\(^{30}\) The main results told us that \(\Delta \ln E\) was 0.17. Thus, \(\epsilon = \frac{0.17}{\ln(1 - 0.12)} = 1.33\). It is also useful to complement the obtained estimates with a lower bound on the elasticity. Let us therefore assume that all individuals paid the highest tax rate before the reform; in other words, \(\tau_1 = 0.3\). The lower bound of the elasticity is then \(\epsilon = \frac{0.17}{\ln(1 - 0.3)} = 0.48\).

These elasticities are large compared to those found in studies from the U.S., which are generally between 0.1 and 0.2 (see Kopczuk (2013) for an overview). An interpretation of the elasticities I obtain being larger is that heirs in Sweden are more sensitive to taxation than heirs in the U.S. However, such conclusions are probably premature, because the earlier studies captured the effect on actual estate size and real responses, such as wealth accumulation, whereas this study captures the effect on reported estate size and under-reporting responses, and the response in wealth accumulation to the tax may differ from the response in under-reporting. In addition, the earlier studies used variation in tax rates, whereas this study uses variation from a tax repeal. The change in incentives from a tax repeal tends to...

\(^{28}\) Technically, the tax base in the Swedish setting is the inheritance size and not the estate size. However, the inheritance size depends directly on the estate size, and in the case of spousal bequests, they are the same by the default rules.

\(^{29}\) This exercise relies on the assumption that the elasticity is the same for all individuals.

\(^{30}\) This is the average tax rate weighted by the tax base.
be more salient, which in turn could explain the higher elasticity estimates.\footnote{In an influential study, (Chetty et al. 2009) showed the importance of salience for people's response to taxes.}

So, what do the large elasticities tell us about the effect of the tax on the decedent's savings and labor supply? The answer to that question is not obvious. As mentioned, the literature on optimal inheritance taxation generally considers the elasticity of bequest (or estate) size a measure of the sensitivity of the decedents' savings and labor supply.\footnote{Examples of trade offs of this kind are found in, for instance, Piketty and Saez (2013).} Such an interpretation suggests that the higher the estimated elasticity, the larger the distortions on the donor's savings and labor supply. However, that is not necessarily true for the elasticity found in this study, because this elasticity does not reflect real responses, only tax planning. Instead, the large elasticity suggests that the tax is easy to evade, or at least that people are able to evade large amounts. This, in turn, may imply that the tax has less distortions on wealth accumulation, as the effective price of transferring wealth is lower than the stated tax rate when the tax is easy to evade, which in turn makes wealth accumulation closer to what it would have been in absence of the tax.

That the obtained elasticity reflects tax evasion is also important for its implications on the dead weight loss of the tax. In two influential articles, Feldstein shows that the elasticity of the tax base is a sufficient statistic for capturing the excess burden of taxation, under the assumption that the social and private cost of tax planning are the same (Feldstein 1995, 1999). Given this result, the large elasticity would suggest that the dead weight loss of the tax is large. However, as argued by Chetty (2009), the private and social costs of tax evasion are probably not the same, as several evasion strategies are associated with externalities. For instance, the fines that have to be paid if under-reporting is detected are transfers to the government. The private cost of the fine is thus an income of the government, and assuming that the evasion implies no extra cost for the tax agency, there is no social cost of tax evasion, despite there being a private one.\footnote{This argument requires the under-reporting heir being risk neutral. If he or she is risk averse, then tax evasion implies a dead weight loss, but lower than it would have been had the fine only been a cost and not also government revenues.} The social cost of under-reporting being lower than the private cost would suggest that the dead weight loss of the studied tax evasion is lower than first suggested by the large elasticity.

Of course, the tax having less distortions does not mean that it is without problems. First, because of extensive planning, it raises less revenues; the estimates of this study suggest that the revenue could be reduced by as much as 55 percent. This casts doubts on the justification of the tax. Second, even though the tax...
has little effect on the amount of wealth accumulated in a direct sense, it may still distort wealth holdings, as decedents may have enabled under-reporting by holding assets that are easy to under-report. Wealth would then be held in self-reported assets, which does not include stocks or bonds. In other words, wealth would be more likely to be held in low yield assets, such as cash or inventories.

### 6.2 Under-reporting and the tax rate

The theoretical tax evasion literature provides predictions on how the amount under-reported relates to the tax rate. We saw in Section 5 that, in absolute terms, estates liable to higher tax rates were under-reported more, in absolute value, than estates liable to lower tax rates. This is consistent with the setting of tax evasion models, in which the penalty for evasion is defined as a fixed share of the amount under-reported and the probability of detection depends on how much the estate is under-reported rather than the value of taxes evaded.

To see how the finding is consistent with such a setting, consider the choice facing the surviving spouse when he or she decides on how much assets to under-report. In the model of Yitzhaki (1987) the individual maximizes expected utility of evasion by choosing the amount to under-report given the probability and penalty of being detected. Because of the setting, a higher tax rate implies that the gains from under-reporting increase (more taxes are evaded) for a given value of under-reported assets (fixed probability for detection). Thus, we expect larger amounts to be under-reported for higher tax rates, which is in line with what we observe.

### 6.3 How general are the results?

The estimates on estate size under-reporting presented in this study concern spousal bequests and are estimated using the estates of individuals passing away close to January 1, 2004. Studying a specific group of individuals and a specific inheritance tax raises the question of how general the results are. For instance, are the estimates also telling for under-reporting of estates left by individuals passing away on other dates? And, are the estimates telling not only for under-reporting

34. His framework explains income under-reporting, but is applicable to estate size under-reporting as well.

35. Formally, the surviving spouse optimizes $\max_g [1 - P(g)] U(c + g) + P(g)U(c - \pi g)$, where $g$ is the under-reported amount, $P(\cdot)$ is the probability of audit, $U$ is utility, $c$ is consumption, which is exogenously given, and $\pi$ is the penalty rate.
with respect to spousal bequests, but also for under-reporting of intergenerational transfers?

Estimates of RD designs are often interpreted as local average treatment effects, meaning that the estimates are specific for the individuals close to the treatment threshold, and that caution is needed when generalizing the results to individuals farther away from the threshold. But, as pointed out by Lee and Lemieux (2010), the localness of the RD design may be somewhat exaggerated, because the extent to which its estimates may be generalized depends on how the treatment effect relates to the running variable—in this case, date of death. They propose that, under heterogeneous treatment effects, RD design estimates should be seen as weighted average treatment effects, with weights proportional to an individual’s probability of being close to the treatment threshold. The more similar individuals’ probability to pass away close to the treatment threshold, the closer the RD design comes to estimating an average treatment effect. The problem with this interpretation is that it is impossible to know ex ante how likely an individual is to pass away close to January 1, 2004. We only know when he or she actually passed away. However, in this setting, it is not unreasonable to assume that the weights should be relatively similar for all studied individuals, and that the estimates thus are closer to represent an average treatment effect.

There are important differences between spousal bequests and intergenerational transfers that affect the generalizability of the results. For instance, the wealth—or at least some of it—of a married decedent has been controlled by not only the decedent, but also by the surviving spouse, who has thus already had the opportunity to engage in tax planning. However, this is likely to influence tax planning strategies carried out before the decedent’s demise and not planning strategies carried out after, meaning that it is less important for the generalizability of results on under-reporting. A second, and perhaps more important difference, is that there are seldom any conflicts of interest when the estate of a married decedent is transferred and under-reported; conflicts of interest that are present in under-reporting with respect to many intergenerational bequests. With regard to intergenerational bequests, there are often several heirs, who all have an incentive to under-report as much as possible, while simultaneously wanting to ensure that all wealth they receive is listed in the estate report, so that they can claim their right and avoid future disputes with the other heirs. When a married person passes away, on the other hand, the default rule stipulates that the spouse is supposed to inherit the full estate. There is thus only one heir, and no extra restriction on the amount that can be under-reported, coming from the need to have the received wealth listed in the estate report to defend one’s right to the inheritance. This difference between spousal bequests and intergenerational bequests suggests
that the estimates obtained in this study are less representative for settings in which there are several heirs. Thus, they are probably less translatable to under-reporting of estates to decedents with several children, than they are translatable to under-reporting of estates to decedents with only one child.

7 Summary

This paper studies the extent of planning to evade inheritance taxes, in Sweden. It shows that estate size under-reporting was widespread and that people were able to reduce their tax payments to a considerable extent. Both large and relatively small estates were under-reported, but the value of larger estates, facing higher tax rates, was reduced more.

The paper provides several contributions to the literature on inheritance taxes and tax planning. It provides evidence on the extent of estate size under-reporting, using a quasi-experimental research design, and shows that under-reporting is an important inheritance tax planning strategy. It also shows that the preferences and means for under-reporting applied to a large segment of the population.

To conclude, the paper shows that the inheritance tax was evaded to a great extent. This weakens the arguments for the tax, such as its ability to raise revenue and promote equality of opportunity. However, the tax being easy to evade also implies that it has less distortive effects on labor supply and savings. Thus, inheritance tax planning—and in particular, estate size under-reporting—is important and its consequences have to be acknowledged when weighing the benefits and drawbacks of the inheritance tax against each other.

References


### A Asset valuation

#### Table A1: Asset values

<table>
<thead>
<tr>
<th>Asset type</th>
<th>Comment</th>
<th>% of market value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Real estate</td>
<td>Includes all real estate, but not co-operative building society dwellings</td>
<td>75</td>
</tr>
<tr>
<td></td>
<td>This refers to co-operative building society</td>
<td></td>
</tr>
<tr>
<td>Apartments</td>
<td>housing, the most common form of apartments in Sweden</td>
<td>n.a.(^1)</td>
</tr>
<tr>
<td>Cash etc.</td>
<td>Includes bank holdings, cash and bonds at the National Debt Office</td>
<td>100</td>
</tr>
<tr>
<td>Assets taxed at 75%</td>
<td>Stocks at Stockholm stock exchange’s main list, stocks in foreign stock exchanges, bonds, etc.</td>
<td>75</td>
</tr>
<tr>
<td>Assets taxed at 30%</td>
<td>Stocks at other Swedish lists than the Stockholm stock exchange’s main list, stocks at NASDAQ</td>
<td>30</td>
</tr>
<tr>
<td>Small businesses</td>
<td>Family firms, closely held corporations, partnerships, sole proprietorship etc.</td>
<td>n.a.(^2)</td>
</tr>
<tr>
<td>Inventories</td>
<td>Inventories, consumer durables, etc.</td>
<td>100</td>
</tr>
<tr>
<td>Debt</td>
<td>All debt</td>
<td>100</td>
</tr>
</tbody>
</table>

Note: \(^1\) The tax value of an apartment was not based on its market value, but on the net wealth of the housing society, which was only weakly, if at all, related to the market value of the apartment. \(^2\) Firms were valued at 30 percent of their book value.

### B Placebo populations

#### B.1 Placebo population: decedents in 2002 and 2003

The first placebo test in Section 5 uses the population of married decedents in 2002 and 2003. All selection criteria that apply to this population are the same as for the main population of study, except for the years studied now being 2002 and 2003, and the exogenous measure for tax liability now being based on wealth.

**B.2 Placebo population: exit households 2003 and 2004**

The second placebo test in Section 5 studies non-married decedents in 2003 and 2004. A set of restrictions apply to this population. First, it is restricted to non-married decedents with children. This restriction makes it possible to calculate tax liability and tax payments. Second, to make the population comparable to the main population of study, it is restricted to decedents having wealth one year before death (2002) that would have implied tax payments had the decedent passed away in that year. This leaves 35,607 individuals, 17,542 of whom passed away in 2003 and 18,065 of whom passed away in 2004.

**B.3 Placebo population: non-taxable estates in 2003 and 2004**

The third placebo test in Section 5 studies married decedents who had no incentives to tax plan. To define the population, the same sample criteria as for the main population are used (described in Section 3), but instead of being restricted to individuals who in 2002 had more wealth than SEK 280,000, the population is restricted to those who in that year had less than SEK 280,000. Thus, it is restricted to individuals who had wealth of a value that made under-reporting to evade taxes unnecessary. The restriction leaves 31,272 individuals, 15,989 of whom passed away in 2003 and 15,283 of whom passed away in 2004.

**C Test of the identifying assumptions**

This appendix presents the tests of the identifying assumptions, discussed in Section 4.

**C.1 Number of decedents by day**

I study the distribution of deaths on dates close to January 1, 2004. The presence of sorting is more likely if we see that people tend to pass away after rather than before the tax was repealed. In addition, the more people passing away after the repeal rather than before it (i.e., the larger the number of people postponing their deaths), the easier sorting is likely to be. Figure C1a shows the number of deaths
Figure C1: Number of decedents by day. Subfigure a is binned at a daily level. Subfigure b is binned at a weekly level. Date is relative to January 1, 2004 (Date=0)

by day for dates around January 1, 2004. We see that there is a spike in the distribution at the date of the reform, in line with the sorting found by Eliason and Ohlsson (2008, 2013). The spike seems to be concentrated to January 1, and there is little visible over- and under-density in the days after and before the reform. If we look at the full year distribution in Figure C1b, we see that there is great variation in the number of deaths. Thus, even if there appears to be some sorting, it is hard to tell whether the sorting is extensive or not.

McCrary (2008) provides a formal way to assess the difference in density before and after the repeal. The test creates a histogram and estimates the difference in log density for histogram bins around the threshold using a RD design. Reassuringly, the test fails to estimate a significant effect; the estimated log difference in density is -0.017 (0.038). However, the approach of McCrary provides no guidance on how the data should be aggregated for the histogram. In addition, it provides no guidance in selecting the appropriate estimation bandwidth. I therefore perform the test using several bandwidths and binwidths to show that the results are robust to these choices. The results of these tests are shown in Figure C2. In addition, I implement the test suggested by Cattaneo et al. (2015). Similar to the approach of McCrary, it tests for a difference in density at the threshold. However, it does not pre-bin the data and it provides data-driven approaches for bandwidth selection. Reassuringly, this test is also unable to find any statistically significant differences in density. It estimates the log difference to -0.15, the p-value of the test being 0.87.

36. Result not reported in table.
37. Results not reported in table.
Figure C2: Test of McCrary (2008) by bandwidth and bin size. Estimate and 95 percent confidence intervals on y-axis. Bandwidth in days on x-axis.
C.2 Test for differences in predetermined characteristics

It is not only the number of sorters that matter for causal identification, but also who the sorters are. If people with strong incentives to avoid taxes (e.g., the wealthy) are able to perfectly control the date on which they pass away, bias may be large even if there are few sorters. To test for whether or not this is the case, individuals passing away before and after the reform are compared by estimating the RD design on predetermined characteristics. If treatment is as good as random, we should see no estimated differences in these characteristics. Figure C3 shows the relationship between the predetermined outcomes and date of death. There is no apparent jump in the level of the outcomes at the treatment threshold. Regression results are presented in Table C1. We see that the estimates for wealth, income, age, sex and university education all are imprecise and close to zero. The zero effects found are robust to the choice of bandwidth, as seen in Figure C4. Overall, the estimates on predetermined characteristics show no support of systematic differences or sorting.

Table C1: Effect of the tax repeal on predetermined characteristics

<table>
<thead>
<tr>
<th></th>
<th>Wealth</th>
<th>Age</th>
<th>Female</th>
<th>University education</th>
<th>Income</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment effect</td>
<td>-0.0294</td>
<td>0.245</td>
<td>0.0218</td>
<td>-0.0138</td>
<td>-0.116</td>
</tr>
<tr>
<td>(0.0301)</td>
<td>(0.422)</td>
<td>(0.0199)</td>
<td>(0.0148)</td>
<td>(0.108)</td>
<td></td>
</tr>
<tr>
<td>Bandwidth</td>
<td>120</td>
<td>132</td>
<td>99</td>
<td>139</td>
<td>81</td>
</tr>
<tr>
<td>Observations</td>
<td>10,082</td>
<td>11,002</td>
<td>8,437</td>
<td>11,215</td>
<td>6971</td>
</tr>
</tbody>
</table>

Note: Standard errors in parenthesis. Significance stars *, **, *** denote $p < 0.10$, $p < 0.05$ and $p < 0.01$, respectively. Wealth and income are transformed using the inverse hyperbolic sine transformation. Age is in years. Female and university education are in shares. Estimates obtained using local linear regression. Bandwidth chosen following Calonico et al. (2014). For robustness to bandwidth choice, see Figures C4.

38. Note that age is defined as age in 2002. Thus, there is a negative trend in age in subfigure e, as the later an individual pass away, the younger is he or she in 2002.
Figure C3: Relationship between predetermined characteristics and date of death around the tax repeal. The outcomes are aggregated at weekly level. Date is relative to January 1, 2004 (Date=0). Subfigure a shows the relationship between date of death and the decedent’s (inverse sine) wealth 2002. Subfigure b shows the relationship between date of death and the decedent’s (inverse sine) income in 2002 (including pension payments). Subfigure c shows the relationship between date of death and the probability of the decedent having a university education. Subfigure d shows the relationship between date of death and the probability of the decedent being female. Subfigure e shows the relationship between date of death and the decedent’s age.
Figure C4: Predetermined characteristics—Robustness to bandwidth choice. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days. Income and wealth are transformed using inverse hyperbolic sine transformation.
D Additional results

This appendix contains figures showing the robustness of the estimates, presented in Section 5 to the choice of bandwidth and polynomial degree.

Figure D1: Relationship between main outcomes and date of death around the tax repeal. Outcomes are averaged at daily level. Dates relative January 1, 2004 (Date=0). Subfigure a shows the relationship between date of death and (inverse sine) estate size. Subfigure b shows the relationship between date of death and the share of taxable estates. Subfigure c shows the relationship between date of death and (inverse sine) tax payment.
Figure D2: Heir’s wealth—Robustness to bandwidth choice. The outcome is the heir’s (inverse sine) wealth in years following the inheritance. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days.
Figure D3: Margins of tax planning—Robustness to bandwidth choice. Outcomes indicated in subfigure title. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days.
Figure D4: Sudden and non-sudden deaths—Robustness to bandwidth choice. The outcome is (inverse sine) reported estate size. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days.
Figure D5: Extent of under-reporting by tax bracket—Robustness to bandwidth choice. The outcome is (inverse sine) reported estate size. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days.
Figure D6: Extent of under-reporting by tax bracket—Robustness to bandwidth choice. The outcome is reported estate size. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days.
Figure D7: Heterogeneity with respect to whether or not the surviving spouse has university education—Robustness to bandwidth choice. The outcome is (inverse sine) reported estate size. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days.

Figure D8: Heterogeneity with respect to income of the surviving spouse—Robustness to bandwidth choice. The outcome is (inverse sine) reported estate size. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days.
Figure D9: Heterogeneity with respect to whether or not the surviving spouse has children—Robustness to bandwidth choice. The outcome is (inverse sine) reported estate size. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days.

Figure D10: Heterogeneity with respect to wealth of the surviving spouse—Robustness to bandwidth choice. The outcome is (inverse sine) reported estate size. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Estimates obtained using local linear regression. Bandwidth ranges between 15 and 350 days.
E Robustness and placebo tests

This appendix presents detailed results of the robustness checks and placebo tests discussed in Section 5.

E.1 Robustness to the choice of bandwidth and polynomial degree

The results presented in Table 2 were obtained using optimal bandwidths, as given by the approach of Calonico et al. (2014), and local linear regression. To ensure that the estimates are robust to these choices, the extent of tax planning is estimated using a wide set of bandwidths as well as a second and third order polynomial. The robustness to bandwidth choice of the estimates obtained using local linear regression estimations are presented in Figure 2 of Section 5.

E.1.1 Robustness of estimates regarding estate size

Figure E1 plots the estimates regarding estate size by bandwidth and local polynomial degree. The estimates of the linear specification, presented in Figure 2 of Section 5, vary slightly between -0.12 and -0.22. They tend to be more negative for smaller bandwidths and become less negative with bandwidth size. The effect is significant at the 5 percent level for all estimation bandwidths larger than 45 days. The estimates obtained using a second order and a third order polynomial show similar patterns, but they tend to be less precise.

E.1.2 Robustness of estimates regarding the share of taxable estates

Figure E2 plots the estimates regarding the share of taxable estates by bandwidth and local polynomial degree. The estimates of the linear specification, presented in Figure 2 of Section 5, are all between -0.05 and -0.08, except those using bandwidths of 15 and 20 days, which are slightly smaller. The effect is significant at the 5 percent level for all bandwidths between 35 and 350 days. The estimates obtained using a second order and a third order polynomial are similar, but those obtained using small bandwidths are a bit less negative or slightly positive: estimates obtained using a second order polynomial are positive for bandwidths between 15 and 25 days, and estimates obtained using a third order polynomial are positive for bandwidths between 15 to 45 days.
Figure E1: Estimates with respect to (inverse sine) estate size—Robustness to bandwidth and polynomial choice. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Bandwidth ranges between 15 and 350 days.

Figure E2: Estimates with respect to the share of taxable estates—Robustness to bandwidth and polynomial choice. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Bandwidth ranges between 15 and 350 days.
Figure E3: Estimates with respect to tax payments—Robustness to bandwidth and polynomial choice. Point estimates and 95 percent confidence intervals on y-axis and bandwidth in days on x-axis. Bandwidth ranges between 15 and 350 days.

E.1.3 Robustness of estimates regarding tax payments

Figure E3 plots the estimates on tax payments by bandwidth and local polynomial degree. The estimates follow a pattern similar to those of the estimates regarding estate size and share of taxable estates. However, they are generally a bit more imprecise. With the linear specification, presented in Figure 2 of Section 5, they vary between -0.5 and -1.5.

E.2 Accounting for possible sorters—RD donut design

For robustness, the extent of tax planning is estimated using a RD donut design. To implement the donut design, a RD design is estimated on the study population, excluding observations close to the cut-off. The purpose of this is to exclude individuals that may have been able to postpone their death until after the tax repeal.

Given the findings of Eliason and Ohlsson (2013), it is reasonable to assume that individuals are able to postpone their death for a shorter period, to avoid inheritance taxation, but not for very long. Eliason and Ohlsson (2013) show that the window possibly affected by such sorters extends up until six days after the reform, but that it is longer before it, possibly up to two weeks. Two weeks before the reform was also the date parliament decided on the repeal, and thus the first day at which individuals could react to the reform being certain that it would be implemented. I therefore exclude estates of all individuals passing away during
these days.

Figure E4a compares the estimates regarding estate size, for the main estimations and the donut design. The donut design estimates tend to be larger for small bandwidths but become more similar to those of the main estimations for bandwidths close to 90 days—the bandwidth used for the estimation of the effect with respect to estate size presented in Table 2.

Figure E4b compares the estimates regarding the share of taxable estates, for the main estimations and the donut design. The pattern is similar to that of the estimates on the estate size. The estimated effects of the donut design tend to be more negative for smaller bandwidths, but are close, if not identical, to those of the main estimation for bandwidths of 110 days—the bandwidth used for the estimation of the effect with respect to the share of taxable estates in Table 2.

Figure E4c compares the estimates regarding tax payments for the main estimations and the donut design. For all bandwidths, the estimates of the donut design are more negative than the main estimates, and for almost all bandwidths they stay negative and significant.

To summarize, we see that the estimates on estate size and on the probability of passing away with a taxable estate are robust to exclusion of observations close to the repeal. If anything, the donut design’s point estimates for small bandwidths are more negative than the regular RD design’s, although less precise. The donut estimates being more negative is the opposite of what we would expect if wealthy individuals manipulate their date of death. This result thus speaks in favor of the validity of the empirical strategy.
Figure E4: RD donut design estimates—Robustness to bandwidth choice. Displaying both main and donut estimates. Bandwidth ranges from 40 to 350 days. Point estimates and 95 percent confidence intervals. Estate size and tax payments are transformed using inverse hyperbolic sine transformation.
E.3 Placebo test I: Testing for effects in previous years

To test for effects in previous years, a placebo reform is implemented on January 1, 2003. In other words, I use the exact same sample criteria and identification strategy as for the estimation of the main effect, but pretend that the tax was repealed one year earlier than it actually was\textsuperscript{39}. We see the relationship between the outcomes of interest and date of death around the date of the placebo reform in Figure E5. There are no visible evidence of any effect. Table E1 shows the estimates of the placebo reform. The effects of the placebo reform on estate size and the share of taxable estates are small and insignificant. The point estimate on tax payment is 0.19 and insignificant. The size of the estimate is not negligible, but only a third to a fourth the size of the main estimate, and the standard errors are relatively big. Taken together the estimates do not give reason to believe that there is an effect of the placebo reform, and they thereby support the identification strategy.

Table E1: Effect of the placebo tax repeal on main outcomes (1 January, 2003)

<table>
<thead>
<tr>
<th></th>
<th>Estate size</th>
<th>Taxable estate</th>
<th>Tax payment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment effect</td>
<td>-0.0307</td>
<td>-0.0272</td>
<td>-0.194</td>
</tr>
<tr>
<td></td>
<td>(0.0797)</td>
<td>(0.0204)</td>
<td>(0.220)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>106</td>
<td>103</td>
<td>110</td>
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<tr>
<td>Observations</td>
<td>9,687</td>
<td>9,429</td>
<td>9,935</td>
</tr>
</tbody>
</table>

Note: Standard errors in parenthesis. Significance stars *, **, *** denote $p < 0.10$, $p < 0.05$ and $p < 0.01$, respectively. Outcomes transformed using the inverse hyperbolic sine transformation, except taxable estate, which is binary. Estimates obtained using local linear regression. Bandwidth chosen following Calonico et al. (2014).

\textsuperscript{39} For more information on this population, see Appendix B.
Figure E5: Relationship between main outcomes and date of death around the placebo tax repeal (1 January, 2003). Outcomes aggregated by week. Date relative January 1, 2003 (Date=0). Subfigure a shows the relationship between date of death and (inverse sine) estate size. Subfigure b shows the relationship between date of death and the share of taxable estates. Subfigure c shows the relationship between date of death and (inverse sine) tax payments.

E.4 Placebo test II & III: Testing for other effects possibly affecting the outcomes

Two additional placebo tests are carried out to ensure that no other tax reform or legislation affect the estate size and tax payments of individuals passing away close to January 1, 2004. The first test studies the estates of non-married decedents, and the second studies the estates of decedents who had wealth in 2002 of a value lower than SEK 280,000—decedents with wealth low enough for the surviving spouse not having incentives to under-report. These groups should be unaffected
by the studied tax reform and we should thus expect no estimated effects if the identification strategy is valid.

Figure E6 shows the relationship between the date of demise and tax payments, estate size and the share of taxable estates, among non-married decedents. There is no clear increase in any of the outcomes on the date of the reform, which is confirmed by the estimates presented in Table E2. The estimates with respect to tax payments and the probability of reporting an estate to a taxable value are, if anything, positive, and the estimates on estate size are negative, but close to zero and imprecise.

Figure E7 shows the relationship between the date of death and tax payments, estate size as well as the share of taxable estates among decedents with wealth in 2002 lower than SEK 280,000. There is no increase in estate size on the date of the reform, but there appears to be level shifts in tax payments and taxable estate. This is also confirmed by the estimates presented in Table E3. The estimate with respect to estate size is insignificant and considerably smaller than the main effect, whereas the estimates regarding tax payments and the probability of passing away with a taxable estate are negative and statistically significant. The results on taxable estate and tax payments indicate tax planning, but they do not necessarily invalidate the identification strategy. As discussed in Section 3, wealth in 2002 is not a perfect measure of the tax incentives, because the variable is measured in market value and observed before the division of marital property. We see in Figure E7 that about 17 percent of all estates were of a taxable value after January 1, 2004 even though the decedent’s wealth in 2002 was below SEK 280,000. This may suggest that 17 percent of the individuals passing away before the tax repeal also had incentives to tax plan. It is most likely the planning among these individuals that the estimates capture. Note also that even if the point estimate is of about the same size in this placebo population as in the main population, the relative increase in the share of individuals managing to report an estate of non-taxable values is much lower: it is only 7 percent, compared to 26 percent in the main population. That 17 percent of the individuals are likely to have tax planning incentives may also explain the significant estimates with respect to tax payments. These estimates capture the effect of under-reporting on tax payments by estimating the relative difference between what the tax payments are and what they would have been had there not been any under-reporting. Most individuals in this group—all but the 17 percent whose wealth actually was higher than the tax threshold—make no tax payments, regardless of whether or not they under-report,

40. An additional assumption is that the placebo populations are correctly defined. The assumption is not innocuous, which will be apparent in the discussion on the placebo test using estates of lower than taxable value.
Table E2: Effect of the tax repeal on main outcomes in the placebo population of non-married decedents

<table>
<thead>
<tr>
<th>Estate size</th>
<th>Taxable estate</th>
<th>Tax payment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment effect</td>
<td>-0.00973</td>
<td>0.00263</td>
</tr>
<tr>
<td>(0.0392)</td>
<td>(0.0184)</td>
<td>(0.0702)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>131</td>
<td>123</td>
</tr>
<tr>
<td>Observations</td>
<td>12,753</td>
<td>12,339</td>
</tr>
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</table>

Note: Standard errors in parenthesis. Significance stars *,**,*** denote $p < 0.10$, $p < 0.05$ and $p < 0.01$, respectively. Outcomes transformed using the inverse hyperbolic sine transformation, except taxable estate, which is binary. Estimates obtained using local linear regression. Bandwidth chosen following Calonico et al. (2014).

because their wealth is too low. These individuals do not affect the estimate on the relative difference, as their tax payments are always zero. This means that the estimate is only based on the individuals actually having incentives to under-report. Thus, the results do not invalidate the identification strategy as much as they show that wealth in 2002 is an imperfect measure of tax incentives.

Table E3: Effect of the tax repeal on main outcomes in the placebo population of decedents with non-taxable estates

<table>
<thead>
<tr>
<th>Tax payment</th>
<th>Taxable estate</th>
<th>Estate size</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment effect</td>
<td>-0.0812</td>
<td>-0.0587***</td>
</tr>
<tr>
<td>(0.196)</td>
<td>(0.0137)</td>
<td>(0.133)</td>
</tr>
<tr>
<td>Bandwidth</td>
<td>129</td>
<td>134</td>
</tr>
<tr>
<td>Observations</td>
<td>11,242</td>
<td>11,657</td>
</tr>
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Note: Standard errors in parenthesis. Significance stars *,**,*** denote $p < 0.10$, $p < 0.05$ and $p < 0.01$, respectively. Outcomes transformed using the inverse hyperbolic sine transformation, except taxable estate, which is binary. Estimates obtained using local linear regression. Bandwidth chosen following Calonico et al. (2014).
Figure E6: Relationship between main outcomes and date of death in placebo population (non-married decedents) around the tax repeal. Weakened averages. Date is relative to January 1, 2004 (Date=0). Subfigure a shows the relationship between date of death and (inverse sine) estate size. Subfigure b shows the relationship between date of death and the share of taxable estates. Subfigure c shows the relationship between date of death and (inverse sine) tax payment.
Figure E7: Relationship between main outcomes and date of death in placebo population (non-taxable estates) around the tax repeal. Weakly averages. Date is relative to January 1, 2004 (Date=0). Subfigure a shows the relationship between date of death and (inverse sine) estate size. Subfigure b shows the relationship between date of death and the share of taxable estates. Subfigure c shows the relationship between date of death and (inverse sine) tax payment.
F Definition of sudden death

Sudden death is defined using the cause of death and medical history of the decedent. Information on cause of death is obtained from the Swedish Cause of Death Register (Dödsorsaksregistret), and information on medical history is obtained from the National Patient Register (Patientregistret). Causes of deaths and earlier diagnosed conditions are identified using WHO’s International Statistical Classification of Diseases and Related Health Problems (ICD).

Deaths are defined as sudden if an individual had not been diagnosed with the illness he or she passed away from, in hospital, before the demise. The process to determine this is as follows: First, the main cause of death is identified in the cause of death register. Second, the patient register is searched through to see if there are any hospital admissions at which the decedent was diagnosed with the same condition. A death is considered non-sudden if there is such an admission and sudden otherwise. For some individuals, the Cause of Death Register lists contributory causes of death. In such cases, the same process as for the main cause of death is repeated.

A few caveats of the approach should be mentioned. First, the diagnose classification system changed in 1997, from ICD-9 to ICD-10, and it is not possible to perfectly translate all diagnoses between the two systems. However, the classification system contains letter categories capturing broader sets of illnesses such as cancer, circulatory conditions, infections, as shown in Table F1. These 21 categories are translatable between the systems, and I use them to match the cause of death with diagnoses from previous hospitalizations. This means that, technically, a death is considered as non-sudden if a person was diagnosed with a condition in the same category as his or her cause of death, but not necessarily the exact same condition. Second, as there are no data on hospitalizations before 1993, it is possible that a person was diagnosed with a terminal illness before that year, making his or her death falsely appear as sudden. However, that would require that the person did not seek hospital care for the same illness, or other illnesses in that category, during the 10 years in between initial diagnosis and demise.

References


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