DYING TO SAVE TAXES:
EVIDENCE FROM ESTATE TAX RETURNS ON THE DEATH ELASTICITY

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ABSTRACT

This paper examines data from U.S. federal tax returns to shed light on whether the timing of death is responsive to its tax consequences. We investigate the temporal pattern of deaths around the time of changes in the estate tax system – periods when living longer, or dying sooner, could significantly affect estate tax liability.

We find some evidence that there is a small death elasticity, although we cannot rule out that what we have uncovered is *ex post* doctoring of the reported date of death. However, the fact that we find that postponement, rather than acceleration, of death is more likely to occur suggests that this phenomenon is at last partly a real (albeit timing) response to taxation.

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1 Introduction

On January 15, 2000, *The New York Times* reported that in the first week of the new millennium local hospitals had recorded an astonishing 50.8% more deaths than in the last week of 1999.[1] The *Times* suggested that this phenomenon was due to infirm people willing themselves to stay alive long enough to witness the dawning of the new age. Apparently, the anticipation of momentous events can motivate people to live longer.

This evidence raises the intriguing question of whether the timing of death responds to economic factors. Could the timing of death be, to some extent, a *rational* decision? Economists presume that the timing of other important events, such as childbearing or marriage, may be so affected--why not dying itself?

In this paper we examine data from U.S. federal estate tax returns to shed light on this question. We investigate the temporal pattern of deaths around the time of changes in the estate tax system--periods when living longer (or, dying sooner) could significantly affect estate tax liability. These periods provide ideal natural experiments enabling us to test for the presence and strength of this particular kind of behavioral response to taxes.

1.1 Evidence on the effect of taxation on the timing of economic decisions

There is a vast literature, briefly summarized in Auerbach and Slemrod (1996), concerning the impact of taxation on economic decisions ranging from labor supply to

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[1] Hershey (2000).

business organization to exercise of stock options. Slemrod (1990) characterized the magnitude of behavioral response as fitting a hierarchy, at the top of which, with the largest degree of responsiveness, lies the timing of transactions with respect to anticipated changes in the tax structure.[2] The classic example, detailed in Burman, Clausing, and O'Hare (1994), is the increase in capital gains realizations in 1986 in anticipation of increased taxation beginning the next year. Realizations increased from $167 billion in 1985 to $322 billion in 1986, only to fall back to $137 billion in 1987. Long-term capital gains realizations of corporate stock in December of 1986 were nearly seven times their level in the same month of 1985. Other examples of large timing responses include exercise of stock options (Goolsbee, 2000), charitable contributions (Burman and Randolph, 1994), and firms' shifting of taxable income through deferred income recognition and accelerated expense recognition (Scholes, Wilson, and Wolfson, 1992).

1.2 Evidence on the effect of taxation on the timing of "non-economic" decisions

There is also evidence that financial considerations affect the timing of decisions that are not generally thought of as being "economic." For example, Sjoquist and Walker (1995) conclude from an analysis of Census data that the marriage penalty embedded in

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[2] The second rung of the hierarchy includes accounting and renaming responses, such as the shift from Subchapter C to Subchapter S corporations after the Tax Reform Act of 1986 inverted the long-standing relationship of the top corporate and top individual tax rates. The third rung, the least responsive, comprises "real" decisions such as labor supply or saving.

the U.S. income tax has a significant negative effect on the timing of marriages: as the penalty increases, fewer couples marry in the months of November and December relative to the number of marriages during the first few months of spring in the new year. Alm and Whittington (1995), using data from the Panel Study on Income Dynamics, also find that taxes have a significant effect on the probability of a couple delaying marriage from the last quarter of one year to the first quarter of the next year; they find no evidence, though, of taxes having an effect on speeding divorce to the current year to avoid a year's marriage penalty. Gelardi (1996) reports a significant drop in the percentage of marriages occurring in the last months of the tax year following tax law amendments in Canada, as well as in England and Wales, designed to eliminate the tax benefit of marrying just prior to the tax year-end.

Dickert-Conlin and Chandra (1999) find that the timing of births is sensitive to tax incentives. Under the U.S. tax system, the tax benefits of having a child are (fully) realized only if the birth takes place before midnight, January 1. Using a sample of children from the National Longitudinal Survey of Youth, Dickert-Conlin and Chandra find that the probability that a child is born in the last week of December, rather than the first week of January, is positively correlated with tax benefits from so doing; they estimate that increasing the tax benefit by $500 raises the probability of having a child in the last week of December by 26.9 percent.

1.3 The timing of death

If birth, why not death? Of course, barring a future technological advance, any effect of tax policy on death could *only* be a timing response. There is certainly a large literature on how average longevity responds to changes in, for example, health care, but

we know of no evidence about its high-frequency response to pecuniary incentives. In the introduction we referred to the millennium-end evidence that the timing of death is responsive to non-pecuniary incentives. There is a substantial body of evidence corroborating this phenomenon in other contexts. Phillips and King (1988) report that, among Jews, the number of deaths was lower than expected in the week before Passover and higher than expected in the week after; the pattern was most pronounced in years when the holiday fell on a weekend, when it is most likely to be celebrated by the largest number of people. Phillips and Smith (1990) find that mortality among Chinese dips by 35.1% in the week before the Harvest Moon festival and peaks by the same amount in the week after. Anson and Anson (1997) find a similar effect related to the timing of Ramadan for Moslems living in Israel, and note that the effect was larger for women than for men, reflecting their different roles in the celebration of the holy day rites. Phillips and Feldman (1973) claim that the same phenomenon occurs around birthdays and presidential elections, although Schulz and Bazerman (1980) argue that this analysis does not withstand close scrutiny. The consensus of this literature is that death can be briefly postponed until after the occurrence of a significant occasion.[3]

1.4 Implications for bequest motives

A non-zero death elasticity is consistent with the notion of a bequest motive. Altruistic individuals should consider adjusting the timing of their death if by so doing it will benefit their heirs. There is, however, another possibility. Decisions about

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[3] There is also a literature on the pattern of suicides around major public holidays, but the contentious findings on this topic are related to the theory of the "broken promise effect."

prolonging the life of a critically ill person (e.g., regarding whether to continue with life support) are often made not by the dying person but by others, including the potential heirs themselves. For this reason, observing a non-zero death elasticity would not definitively establish the presence of an altruistic bequest motive on the part of the decedent. Note, however, that a parent anticipating the possibility of self-serving behavior by potential heirs might at an earlier time alter his or her behavior, perhaps by writing a will whose terms are contingent on heirs' behavior. Even under this scenario, observing a death elasticity may be considered as evidence against the pure life-cycle model, and in favor of a model with a strategic bequest motive.

2 Institutional Background

2.1 Estate tax

The modern U.S. estate tax was introduced in 1916. Initially, the highest marginal tax rate was just 10%, on estates above $50,000,000. The tax rates were increased twice during 1917, and then they were reduced in 1919 and 1926. Starting in 1932, a series of 5 consecutive tax reforms increased the top marginal tax rates to 77%. At that level the tax rates stayed until 1976. Between 1977 and 1987 the exemption level changed every year. The Tax Reform Act of 1976 and the Economic Recovery Act of 1981 also modified the rate structure (the latter was phased in over three years).

2.2 Estate tax changes

We examine the timing of deaths resulting in taxable estates in the period surrounding 13 major changes in the estate tax.[4] Eight of them (3/3/1917, 10/4/1917, 6/2/1924, 6/6/1932, 5/10/1934, 8/30/1935, 6/26/1940, 9/20/1941) were tax increases, and five of them were tax decreases (2/24/1919, 2/26/1926, 10/21/1942, 1/1/1983, 1/1/1984). These tax reforms involved changes in the tax structure, and occasionally also in the exemption level (but always with the same direction of changes in tax liability for all estates).

The chronology of events leading to the tax changes--and therefore the degree to which the effective date might have been anticipated--varied somewhat. The pre-war reforms took effect on the day they were signed by the President. Our reading of newspaper accounts of the time suggest that once the House and Senate conference agreed on the tax bill, that the President would sign it was a foregone conclusion. As the chronology in Table A-1 shows, the elapsed time between the two dates ranged from two to eighteen days, and averaged 6.6 days. The time between the passage of a tax bill in the Senate and presidential signature ranged from five to sixty-three days, and averaged 19.1

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[4] These are the reforms that involved significant changes in the tax rate structure, as identified by McCubbin (1990) and Luckey (1995). Because of the incomplete coverage of our data detailed in section 3, we cannot analyze a few tax reforms such as the changes in the rate structure and exemption level between 1977 and 1982. Because of data deficiencies related to inconsistent sampling rules across years, we also do not examine changes in the exemption level between 1985 and 1987 and a small modification of the tax rate structure introduced by the Omnibus Budget Reconciliation Act of 1987 that applied only to very large estates (above $10,000,000).

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days. The generally swift procedure strongly suggests that the effective date of the pre-war tax bills could to some varying extent be anticipated. The timing and content of reforms of the 1980s were known in advance, because they were part of legislation designed to gradually increase the tax credit and phase out high marginal tax rates.

The changes in tax law usually involved more than just a change in the tax rate structure. On a few occasions the definition of taxable estate changed, and we account for that in the analysis below. Occasionally, the definition of gross and net estates changed as well. This is relevant because the filing requirement is expressed in those terms, so that some size class of estates would appear in the data before but not after a tax reform, or vice versa. As mentioned below, we deal with this issue by considering only those returns with a reported net worth that is above the larger of the pre- and post-reform thresholds.

3 Data

Our analysis makes use of an extraordinary sample of estate tax returns filed from the inception of the modern U.S. tax until very recently. This database contains most of the information from estate tax returns filed in every year between 1916 and 1945, as well as returns filed in 1962, 1965, 1969, 1972, 1976 and all years between 1982 and 1996. For the years 1916 to 1945, *all* the returns actually filed in those years (provided they were not missing at the time the data was entered in the database) are included. For the post-war data, the data are a stratified sample of all returns filed, with sampling probabilities available to enable us to statistically represent the unsampled returns, as well. McCubbin (1990) describes the origin and structure of the data in detail.

The information requested on the tax return changed over time, as did the items from the returns that were placed into the data set. In one key respect, the data for the pre-1945 period is better, because information on the day of death is available for every individual, so that it is possible to identify a complete population of individuals dying close to any tax reform. The post-war tax reforms coincide with the beginning of the year, and thus in every case also correspond to differences in the sampling procedure.[5] [6]

4 Aggregate Analysis

We begin with the simplest possible approach for detecting the effect of estate tax changes on the reported dates of deaths: observing the number of returns filed that report dates of death within a period of days before and after the tax reform. Because for later years the sampling procedure changed exactly at the time when tax reforms were implemented, we analyze in this way only the pre-1945 reforms.

In order to make this comparison meaningful, tax returns before and after a tax reform need to be drawn from the same population. For that reason we investigate only those returns with a reported net worth above the larger of the pre- and post-reform filing thresholds. Implicitly, this amounts to treating net worth around the tax reform as

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[5]As noted earlier, there may also be non-tax behavioral responses with regard to end-of-year deaths. We return this issue later.

[6] The sampling procedure used in choosing estates filed in 1985 differed from those for other years. In particular, no individuals who were older than 45 and had estates below $5 million were sampled. Most tax returns are filed within 3 years of decedent's death (more than 99%, according to Johnson, 1994) so that it is unclear if data between 1982 and 1985 are representative. For this reason, we don't analyze small reforms in this period, and we do not include this data in the pooled specification.

exogenous. This assumption would be unacceptable for analysis over a longer period of the time, but it seems to be reasonable to make it for an analysis of the decisions regarding a short of period of time as we do here.[7]

Some of the pre-1945 tax reforms did not take effect at midnight. Instead, the law specified a precise time of day other than midnight when the new law started to apply. As a result, returns filed for decedents who passed away on the day of the reform may be subject to either of the two statutes.[8] For this reason, for these reforms[9] we exclude from the analysis returns that report the reform date as the date of death.

Table 1 summarizes this data. As a basis of comparison, it first gives the number of tax returns filed reporting the date of death to be the day of the tax reform. Then it reports the average number of returns with date of death within 1, 3, 7, and 14 days of the tax reform. The averages do not include the day of the reform. The table contains t-statistics and p-values for the mean of the number of returns filed within a given number of days from the tax reform in the lower tax regime to be greater than of those filed in the high tax regime.[10] Note that reforms of 1919, 1926 and 1942 are tax decreases (denoted by D in the third column of the table) and the others are tax increases (denoted I).

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[7] In other work (Kopczuk and Slemrod, 2001), we examine the effect of the estate tax on the value of reported estates.

[8] It was not possible to precisely ascertain which law was applicable based on the magnitude of the estate and the reported tax liability, because the tax liability variable available in the dataset was reduced by certain credits whose magnitude is not known.

[9] These are the reforms of 1919, 1924, 1926, 1932, 1934, 1935, 1940, 1941 and 1942.

[10] This is a standard one-sided t-test for equality of the means of two populations (i.e., average numbers of deaths before and after the tax reform), with standard deviations calculated based on a 30-day window.

There is some evidence for the presence of a death elasticity for the 14-day window. The tests for differences in the means are significant at the 10% level, and in the expected direction, for the reforms of 3/3/1917, 1926, 1934 and 1942. This includes two out of the three tax decreases for this period. This pattern is particularly interesting because one might expect that the behavioral response to tax decreases is stronger, as it is more plausible that people live longer to lower estate taxes rather than die sooner to save taxes. The evidence for shorter windows is less clear, which is not surprising in light of the small number of observations.

The foregoing analysis of dates of death makes no use of the fact that the financial incentive to postpone or accelerate death or its reported date varies greatly across the reforms, and across individuals for a given reform. To investigate this issue we calculate, for every individual with a reported date of death close to a tax reform, the tax liability under the old and new tax systems; one of these figures is a counterfactual, or hypothetical, tax liability. (Details of the calculation are presented in the appendix.) The difference between these two tax liabilities is our measure of the tax incentive regarding the time of death.

Table 2 presents some summary information about the average and median tax incentive, or potential savings, for people who died before and after a given tax reform. Only individuals who could have saved a positive amount are included. It is clear that the potential tax saving is much higher for some of the tax changes, in particular 1932, 1934, 1935 and 1942. In some cases (1935, 1941, 1942), mean tax savings before and after the tax change are significantly statistically different. In 1935 and 1942 the mean tax saving is higher in the low-tax regime, but in 1942 it is higher in the low-tax regime. Overall,

means and medians of tax savings do not suggest that the behavioral response, if any, should be concentrated among the richer part of the sample.

A comparison of means and medians in Table 2 suggests that the distribution of potential tax saving is highly skewed. This is especially obvious for the reform of 1924, when the medians are very close, but mean tax savings are very different. This suggests that one has to be careful in order to make sure that outliers do not unduly affect the conclusions.

Table 3 contains more information about the potential tax saving of different tax reforms. Mean savings in constant 1945 dollars (multiplying by 9.05 yields 1998 dollars) are shown, as well as the average value of ratio of the potential tax saving to the individual's net worth. The reforms vary significantly in the magnitudes of potential savings, ranging from $2,577 in 1940 to $47,765 in 1924. The medians also vary quite a bit, from $308 in 1917 to $7,629 in 1932. In most cases, the medians are significantly smaller than the means. The mean ratio of saving to net worth also varies significantly, from 0.4% to 3.7%. The medians range from 0.2% to 3.6%.

5 Micro Regression Analysis

If the (reported) date of death responds to tax changes, one should observe that the probability of dying in the low-tax regime is a function of the tax saving from so doing. To test this hypothesis, we run a series of probit regression analyses spanning a short window around estate tax changes, with the sole right-hand side variable being a measure of the potential tax saving from dying in the low-tax regime, which may be

before or after the tax law change. [11] Thus, we are examining whether the probability of dying in the low-tax regime, whether that is before or after a tax change, depends on the tax saving from dying then compared to the high-tax regime.

We compute and examine three different measures of tax saving: (1) the absolute value of the potential tax saving (in 1945 dollars); (2) the tax saving expressed as a fraction of the decedent's net worth, and (3) the logarithm of the absolute tax saving (in 1945 dollars) plus $100.[12] We pursue the last two measures in order to reduce the influence of large outlying values, and because we suspect that the incentive of any dollar amount of tax saving may be smaller the larger is the overall size of the estate.

We first pool all of the data, and then separately analyze the data for each tax change.[13] Table 4 reports the results for deaths that occurred within 14 days of the tax reform. The third column of this table shows the number of individuals who died in the high and low tax regimes for each reform, respectively. The pooled regressions suggest

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[11]For each reform, taxes for all individuals change in the same direction. Therefore, for each reform it is unambiguous to refer to one of the tax regimes as the high-tax one and to the other as the low-tax one. For individuals whose tax liability is the same under either regime, we set the low-tax dummy according to the time of their death. In a sense, people with zero tax saving constitute a natural control group.

[12] More precisely, this variable is defined as $\log(100+\text{saving})-\log(100)$, so that it takes value of zero when there is no potential tax saving.

[13] We use sampling weights in the regressions. This matters only for the reforms of 1983 and 1984. The results when weights are not used share the qualitative features of the ones presented here. We report the robust standard errors. In the regressions the tax savings are expressed in real (1945) dollars. The pooled regression does not include the reforms of the 1980s. The results when they are included show stronger behavioral response than what is reported in the pooled results.

that there may be a significant "death elasticity." When the tax saving is measured either in log terms or as a fraction of net worth, there is a statistically significant relationship between the probability of dying in the low-tax period and the tax saving from so doing. The probit coefficients imply that a 1% increase in the ratio of tax saving to wealth would increase the probability of dying in the low-tax period by 1.6%. In the logarithmic specification, a $1000 tax saving is associated with an increase of 0.6% in the probability of dying in the low-tax period. We find no significant relationship for the absolute tax saving specification, which we suspect is due to the noise introduced by large outlying values of the potential tax saving.

The results for individual tax reforms are not nearly as strong, although most of the estimated coefficients have the "right," that is, positive sign. Of the thirteen tax reforms we study, the coefficients are of expected sign in ten cases for each of the specifications. If the coefficient was truly zero, estimated coefficients should be positive and negative with an equal probability. The probability of observing at least that many positive coefficients is 4.6%. Treating these thirteen reforms as draws from the same distributions, for each of the three measures of potential tax saving the hypothesis of no effect may be rejected at a 5% level of significance, although for many reforms the magnitude of the effect is not statistically different from zero.

The measured behavioral response is most pronounced for the two reforms of the 1980s, when the sampling procedure was not consistent in adjacent years. The returns were sampled every year, but the procedure was designed to achieve a representative sample of decedents dying in 1982 and 1986, so that returns filed in these years were oversampled. Although the population weights for other years were constructed to

replicate the actual distribution of net worth of decedents, the precise sampling properties for those years are not known. In particular we are not sure if the sample is representative of the individuals dying during the short period we consider. This problem is not present for the pre-1945 reforms.

If the tax reform occurs on a day of the year without any special significance, one should expect that, in the absence of any behavioral response to tax changes, the probability of dying within a given time interval before the tax reform and after it should be equal. This need not be the case when the reform occurs on a "special" date such as, e.g., January 1st, because as discussed earlier, the timing of death in such circumstances may be influenced by the desire to see the new year in. This reasoning suggests that the constant terms in our regressions should be equal to zero for all of the pre-1945 reforms.[14] The results in Table 4 generally support this assumption: the estimated constant is statistically different from zero in only three cases, and in each of these cases it is so for just one of the specifications. This is no longer true for the 1980s, when the January 1 timing of the tax change and changes in the sampling procedure may play a role.

By imposing a zero restriction on the constant term, we can presumably sharpen our estimates. The results of imposing this constraint are shown in Table 5. We present

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[14] A zero restriction on the constant corresponds to assuming that an individual with no tax saving is equally likely to die before and after the tax reform, because in this case the dependent variable of the probit equation has the value of zero. This is also the case for the logarithmic specification, because, as explained in footnote 12, the tax variable is defined to take value of zero when there is no potential tax saving.

the results for all reforms, although we are confident about the validity of the restriction only for the pre-1945 reforms. The coefficients for the pooled (up until 1942) specifications are significant at the 1% level, and close in magnitude to the ones estimated earlier. Comparing the results for individual reforms to those in Table 4, a few more coefficients are statistically significant. For all specifications, the coefficients for 1942 are significant at either a 1% or 5% level. There is also some evidence of an effect for the reforms of 1917 and 1934. The coefficients for the 1940 reform remain positive but they are no longer significant. Out of eleven pre-war reforms, all but two of the tax saving coefficients are positive for the logarithmic and absolute specifications, and all but one are positive for the relative specification. If the effect was not present, the probability of estimating at least that many positive coefficients is 3.3% and 0.6%, respectively.

In Table 6, we re-estimate the pooled specification (excluding the 1980s observations) allowing for differential response for tax decreases and tax increases. When the constants are not restricted to be zero, the estimated tax coefficients are not statistically significant. When the restriction is imposed, the estimated effect for tax decreases becomes significant at the 5% level for the relative specification, and at the 10% level for the logarithmic one. The effect for tax increases is of the expected direction, but it is insignificant. According to the results of the logarithmic specification, a $1000 tax saving increases the probability of dying just before a tax increase by 1%, while the same amount of saving increases the probability of dying just after a tax decrease by almost 2.5%. This result is plausible, because it seems more likely that people are able to live longer due to financial incentives rather than die sooner. It also suggests that the bulk of the response, at least for tax decreases, is real behavioral

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response and not *ex post* manipulation about the reported date of death: arguably, for the latter type of response the direction of a tax change should not matter.

Finally, in the results reported in Table 7, we allow for a different response by demographic groups. Because of the data limitations (see the appendix), the sample used in these analyses includes only the reforms that occurred between 1926 and 1942. There is no evidence for the presence of a differential response by age, gender, or marital status. Although the coefficient on the interaction of tax saving with being a male is significant at the 10% level for the logarithmic specification (and suggests a weaker response),[15] it is insignificant for the other two specifications. Similarly, the coefficient on the interaction with age is significant only for the absolute specification (at a 1% level, however). No additional coefficients are significant when we restrict the constant to zero, and when we include each interaction term separately. (These results are not reported.)

6 Conclusion

There is abundant evidence that some people will themselves to survive in order to live through a momentous event. Evidence from estate tax returns suggests that some people will themselves to survive a bit longer if it will enrich their heirs. That there is any effect at all adds to the large body of evidence that taxes affect behavior, and particularly the timing if behavior, including activities such as marriage and childbearing which are not generally thought to respond to financial incentives.

We cannot rule out that what we have uncovered is not a "real" death elasticity, but instead *ex post* doctoring of the reported date of death to save on taxes. However, the

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[15] This coefficient is no longer significant when the constants are restricted to zero (not reported).

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fact that we find that postponement, rather than acceleration, of death occurs suggests that this phenomenon is at least partly a real (albeit, timing) behavioral response to taxation.

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Appendix

For the pre-1945 period, the value of the taxable estate is not present in the dataset. We define it as being equal to the value of net worth (gross estate minus debts and mortgages) before 1918, and as the difference between the value of net worth and charitable bequests for 1918 through 1945 (the deduction for charitable bequests was introduced as of January 1, 1918). This definition ignores credits against the tax, funeral and administrative expenses, payments for the support of the decedent's dependents required by local law, uninsured losses during administration, and deductions for the property previously taxed, because our data does not contain information on them. For the post-1945 period, we use the actual adjusted taxable estate from the tax returns. We apply to the taxable estate the actual tax schedule before and after the tax reform, and define tax saving as the difference between them. The information on the estate tax structure is obtained from the Internal Revenue Code.

The decedent's marital status is not present in our dataset before the 1924 reform. Age is missing for most observations in 1917 and for some other observations. For the sample of observations with known age, we regress it on the time trend and the constant. For this purpose, all 677,329 observations (i.e., not just those close to a tax reform) are used. We impute age for the remaining observations using predicted values from this regression. Age is imputed for less than 10% of observations, and the results are robust to restricting the sample to individuals with known age.

The Tax Reform Act of 1924 was repealed in 1926, but we investigate this reform nevertheless, because repeal was unlikely to be anticipated when it was enacted.

Table 1: Number of decedents by reported date of death, relative to date of estate tax reforms

Reform date		Day of reform		Within 1 day	Within 3 days	Within 7 days	Within 14 days	Standard deviation
03/03/1917	I	28	Before	27	24.67	23.86	24.93	5.61
			After	23	23.00	20.57	22.21	4.55
			t-stat	0.55	0.40	1.20	1.41	
			p-value	0.29	0.35	0.12	0.08	
10/04/1917	I	12	Before	23	19.33	21.14	20.00	5.06
			After	20	15.67	19.43	18.86	4.79
			t-stat	0.43	0.91	0.65	0.61	
			p-value	0.33	0.18	0.26	0.27	
02/24/1919	D	21	Before	36	29.67	27.14	28.21	7.53
			After	35	33.00	31.00	29.93	5.33
			t-stat	0.11	0.63	1.11	0.70	
			p-value	0.46	0.27	0.14	0.24	
06/02/1924	I	36	Before	35	31.67	33.29	30.64	5.02
			After	29	31.00	33.57	31.64	5.60
			t-stat	0.80	0.15	0.10	0.50	
			p-value	0.21	0.44	0.46	0.31	
02/26/1926	D	16	Before	23	20.33	18.57	18.43	4.08
			After	17	19.00	19.71	20.86	4.65
			t-stat	0.97	0.37	0.49	1.47	
			p-value	0.17	0.36	0.31	0.07	
06/06/1932	I	10	Before	9	7.67	9.14	10.14	3.46
			After	15	10.33	9.57	9.50	3.42
			t-stat	-1.23	-0.95	-0.23	0.49	
			p-value	0.89	0.83	0.59	0.31	
05/10/1934	I	26	Before	20	24.67	26.71	28.00	4.94
			After	24	23.67	24.86	24.29	5.02
			t-stat	0.57	0.25	0.70	1.97	
			p-value	0.29	0.40	0.24	0.03	
08/30/1935	I	28	Before	26	28.00	26.29	25.29	4.07
			After	21	24.00	25.29	26.64	5.39
			t-stat	0.74	1.03	0.39	-0.75	
			p-value	0.23	0.15	0.35	0.77	
06/25/1940	I	40	Before	36	35.67	35.00	35.00	5.36
			After	36	35.00	36.57	35.07	6.90
			t-stat	0.00	0.13	-0.48	-0.03	
			p-value	0.50	0.45	0.68	0.51	
09/20/1941	I	31	Before	30	34.67	35.86	37.21	7.40
			After	35	40.67	37.57	38.86	6.33
			t-stat	-0.51	-1.07	-0.47	-0.63	
			p-value	0.70	0.84	0.68	0.73	
10/21/1942	D	32	Before	33	25.33	29.14	28.50	5.09
			After	43	38.67	34.43	34.79	6.07
			t-stat	1.26	2.92	1.77	2.97	
			p-value	0.11	0.00	0.04	0.00	

Note: I or D in the second column refer to whether the tax return was an increase or decrease in tax liability.

Table 2: Descriptive statistics for the estate tax reforms.

Reform date			Number of individuals		Tax saving (current dollars)		
			Weighted	Unweighted	Median	Mean	Std. dev.
3/3/1917	I	Before	349	349	285.8	4,456	1,114
		After	318	318	225.9	4,471	1,608
10/4/1917	I	Before	280	280	256.8	4,142	783
		After	260	260	186.1	3,271	1,406
02/24/1919 [1]	D	Before	392	392	417.5	3,560	419
		After	416	416	632.6	3,609	391
02/06/1924 [1]	I	Before	132	132	1,071.3	77,122	62,153
		After	126	126	1,102.4	12,110	7,319
02/26/1926 [1]	D	Before	257	257	1,389.2	18,674	7,352
		After	288	288	1,313.9	21,761	7,183
06/06/1932 [1]	I	Before	141	141	5,788.7	14,056	2,332
		After	132	132	5,851.7	15,895	2,392
05/10/1934 [1]	I	Before	156	156	3,247.1	35,360	11,869
		After	124	124	4,235.3	21,059	8,366
08/30/1935 [1]	I	Before	348	348	2,732.0	8,032	829
		After	366	366	2,037.5	6,345	618
06/25/1940 [1]	I	Before	483	483	140.4	1,599	324
		After	483	483	192.4	2,417	1,066
09/20/1941 [1]	I	Before	508	508	1,929.6	10,017	965
		After	535	535	1,563.6	8,235	1,120
10/21/1942 [1]	D	Before	390	390	5,211.4	4,630	95
		After	482	482	4,688.1	4,404	79
Reforms of the 1980's							
01/01/1983	D	Before	1512.3	557	16,500.0	16,146	810
		After	1672.9	53	16,500.0	17,895	2,949
01/01/1984	D	Before	1345.4	40	17,000.0	17,070	6,489
		After	1870.8	49	17,000.0	23,131	23,786

[1] Day of the reform is excluded.

Note: The pre-1945 data is based on a 100% sample of estate tax returns, while the data from the 1980's are based on stratified random samples, for which the sampling probabilities change from year to year.

Table 3: Mean and median potential tax saving, overall and by tax reform.

Reform date		Mean tax saving		Median tax saving	
		Value (1945$)	Share in net worth	Value (1945$)	Share in net worth
All (except 1980's)		10,772	1.8%	1,532	0.9%
By reform					
3/3/1917	I	6,265	0.5%	358	0.3%
10/4/1917	I	5,226	0.5%	308	0.2%
2/24/1919	D	3,731	0.8%	505	0.5%
6/2/1924	I	47,765	0.8%	1,129	0.4%
2/26/1926	D	20,650	1.2%	1,378	0.7%
6/6/1932	I	19,696	3.7%	7,629	3.4%
5/10/1934	I	39,015	2.0%	4,701	1.4%
8/30/1935	I	9,399	2.5%	3,133	2.3%
6/25/1940	I	2,577	0.4%	211	0.2%
9/20/1941	I	11,126	3.5%	2,106	2.4%
10/21/1942	D	4,976	3.4%	5,302	3.6%
Reforms of the 1980's					
1/1/1983	D	3,130	2.9%	3,078	1.6%
1/1/1984	D	3,583	3.1%	3,073	2.5%

Table 4: Probability of dying in the low-tax regime as a function of potential tax saving.

Reform date		Deaths in high and low-tax regimes	Independent Variable					
			Log of absolute saving (1945$)	Constant	Relative tax saving	Constant	Absolute tax saving (1945$)	Constant
All (except 1980s)		3954	0.0173**	-0.0109	1.3857**	0.0098	0.0004	0.0280**
		4155	(0.0074)	(0.0228)	(0.6976)	(0.0176)	(0.0003)	(0.0141)
Reform-specific regressions								
03/03/1917	I	318	0.0281	0.0018	6.9134	0.0246	0.0000	0.0584
		349	(0.0310)	(0.0790)	(8.3583)	(0.0633)	(0.0014)	(0.0493)
10/04/1917	I	260	0.0587*	-0.0687	16.4911*	-0.0320	0.0011	0.0407
		280	(0.0342)	(0.0861)	(9.5632)	(0.0704)	(0.0025)	0.05533
02/24/1919	D	395	0.0436	-0.0597	5.6060	-0.0092	0.0005	0.0353
		419	(0.0286)	(0.0770)	(5.5281)	(0.0631)	(0.0052)	(0.0480)
06/02/1924	I	443	0.0244	-0.0407	5.4666	-0.0333	0.0003	-0.0231
		429	(0.0265)	(0.0480)	(5.0264)	(0.0442)	(0.0005)	(0.0425)
02/26/1926	D	258	-0.0344	0.1847	-0.9970	0.0891	0.0001	0.0750
		292	(0.0377)	(0.1289)	(3.5177)	(0.0671)	(0.0005)	(0.0543)
06/06/1932	I	133	-0.0345	0.1952	-3.3180	0.1617	-0.0011	0.0633
		142	(0.0607)	(0.2820)	(4.1634)	(0.1695)	(0.0022)	(0.0865)
05/10/1934	I	340	0.0295	0.0459	4.3427	0.0566	0.0005	0.0815*
		392	(0.0210)	(0.0557)	(2.8996)	(0.0512)	(0.0005)	(0.0468)
08/30/1935	I	373	0.0503	-0.2092*	4.6057	-0.1437	0.0044	-0.0729
		354	(0.0321)	(0.1218)	(3.3223)	(0.0925)	(0.0027)	(0.0527)
06/25/1940	I	491	-0.0506*	0.0772	-13.9118*	0.0567	-0.0016	0.0027
		490	(0.0276)	(0.0586)	(7.9534)	(0.0518)	(0.0014)	(0.0402)
09/20/1941	I	544	0.0291	-0.1179	1.7881	-0.0890	0.0015	-0.0432
		521	(0.0204)	(0.0745)	(1.2409)	(0.0577)	(0.0015)	(0.0416)
10/21/1942	D	399	0.0122	0.0786	6.0601*	-0.0782	-0.0266	0.2554**
		487	(0.0648)	(0.2480)	(3.6403)	(0.1288)	(0.0205)	(0.1093)
Reforms of the 1980's								
01/01/1983	D	2457.0	0.2212***	-0.6699***	8.3163***	-0.3008***	0.0529	-0.2424**
		2013.7	(0.0268)	(0.0787)	(2.1572)	(0.0596)	(0.0536)	(0.1197)
01/01/1984	D	2697.9	0.1412**	-0.2160	17.2686***	-0.2337	0.0096	0.0261
		2891.5	(0.0640)	(0.1582)	(6.3250)	(0.1473)	(0.0367)	(0.1208)

Note: ***, ** and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 5: Probability of dying in the low-tax regime as a function of potential tax saving: regressions with a constant restricted to zero.

Reform date		Deaths in high and low-tax regimes	Independent Variable		
			Log of absolute saving (1945$)	Relative tax saving	Absolute tax saving (1945$)
All (except 1980's)		3954	0.0144***	1.6224***	0.0004
		4155	(0.0045)	(0.5531)	(0.0003)
Reform specific regressions					
03/03/1917	I	318	0.0286	8.9773	0.0003
		349	(0.0191)	(6.4434)	(0.0014)
10/04/1917	I	260	0.0374*	13.7679*	0.0014
		280	(0.0215)	(7.2879)	(0.0026)
02/24/1919	D	395	0.0255	5.0312	0.0020
		419	(0.0163)	(3.8465)	(0.0048)
06/02/1924	I	443	0.0138	4.3062	0.0003
		429	(0.0235)	(4.7735)	(0.0004)
02/26/1926	D	258	0.0147	1.8256	0.0002
		292	(0.0157)	(2.8125)	(0.0004)
06/06/1932	I	133	0.0060	0.2441	-0.0004
		142	(0.0163)	(1.8605)	(0.0019)
05/10/1934	I	340	0.0391**	5.6836**	0.0007
		392	(0.0175)	(2.6763)	(0.0005)
08/30/1935	I	373	-0.0006	0.1476	0.0027
		354	(0.0122)	(1.6694)	(0.0023)
06/25/1940	I	491	-0.0242	-8.4523	-0.0016
		490	(0.0188)	(6.0950)	(0.0014)
09/20/1941	I	544	0.0014	0.3598	0.0010
		521	(0.0105)	(0.8267)	(0.0014)
10/21/1942	D	399	0.0324***	3.9761***	0.0175**
		487	(0.0110)	(1.1933)	(0.0080)
Reforms of the 1980's					
01/01/1983	D	2457.0	0.0258*	0.1723	0.0149
		2013.7	(0.0137)	(1.4153)	(0.0138)
01/01/1984	D	2697.9	0.0773*	10.3397**	0.0114
		2891.5	(0.0430)	(4.5571)	(0.0385)

Note: ***, ** and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 6: Probability of dying in the low-tax regimes as a function of potential tax saving: allowing for differential response for tax increases and decreases.

	Log saving	Relative saving	Absolute saving
Saving	0.0125	0.8000	0.0005
	(0.0083)	(0.7791)	(0.0004)
Decrease*Saving	0.0119	2.2386	-0.0004
	(0.0158)	(1.7947)	(0.0006)
Decrease	0.0227	0.0232	0.0731**
	(0.0677)	(0.0450)	(0.0315)
Constant	-0.0156	0.0006	0.0073
	(0.0245)	(0.0197)	(0.0166)
	Marginal effects at zero		
Tax increase	0.0119	0.0032	0.0002
Tax decrease	0.0231	0.0121	0.0000

With zero constraint on constants imposed

	Log saving	Relative saving	Absolute saving
Saving	0.0085	0.8126	0.0005
	(0.0056)	(0.6491)	(0.0004)
Decrease*Saving	0.0178*	2.9429**	-0.0003
	(0.0096)	(1.2393)	(0.0006)
	Marginal effects at zero		
Tax increase	0.0082	0.0032	0.0002
Tax decrease	0.0249	0.0150	0.0001

Note: ***, ** and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 7: Probability of dying in the low-tax regime as a fraction of potential tax saving: regressions using additional indicators.

	Log saving	Relative saving	Absolute saving
Saving	-0.0304	-1.7838	-0.0190**
	(0.0869)	(7.2830)	(0.0081)
Age*Saving	-0.0003	-0.0528	0.00016***
	(0.0009)	(0.0737)	(0.00006)
Male*Saving	-0.0418*	-1.6300	0.0001
	(0.0217)	(1.8317)	(0.0011)
Married*Saving	0.0988	8.0363	0.0093
	(0.0645)	(5.4886)	(0.0072)
Single*Saving	0.0900	8.1263	0.0095
	(0.0680)	(5.8291)	(0.0072)
Widow*Saving	0.0794	7.8955	0.0059
	(0.0655)	(5.5882)	(0.0072)
Age	0.0010	0.0012	-0.0012
	(0.0030)	(0.0023)	(0.0017)
Male	0.0734	0.0456	0.0098
	(0.0726)	(0.0553)	(0.0414)
Married	-0.3411	-0.2357	-0.1362
	(0.2195)	(0.1753)	(0.1377)
Single	-0.3739	-0.2949	-0.1936
	(0.2289)	(0.1830)	(0.1436)
Widow	-0.2805	-0.2261	-0.1036
	(0.2217)	(0.1772)	(0.1389)
Constant	0.1715	0.1272	0.2382
	(0.2931)	(0.2290)	(0.1756)

Note: ***, ** and * denote significance at the 1%, 5%, and 10% level, respectively.

Table A-1: Chronology of pre-1945 Estate Tax Bills

House Bill Passed	Senate Bill Passed	Conference Agreement	Conference Bill Passed by House	Conference Bill Passed by Senate	Bill Signed by President and Effective Date
2/1/17	2/28/17	n.a.	n.a.	n.a.	3/31/17
5/23/17	9/10/17	10/1/17	10/1/17	10/2/17	10/4/17
9/20/18	12/23/18	2/6/19	2/8/19	2/13/19	2/24/19
2/29/24	5/12/24	5/24/24	5/26/24	5/24/24	6/2/24
12/28/25	2/13/26	2/22/26	2/22/26	2/25/26	2/26/26
4/1/32	6/1/32	6/3/32	6/4/32	6/6/32	6/6/32
2/2/34	4/17/34	4/30/34	5/1/34	5/3/34	5/10/34
8/5/35	8/16/35	8/22/35	8/23/35	8/23/35	8/30/35
6/11/40	6/20/40	6/21/40	6/22/40	6/22/40	6/25/40
8/4/41	9/8/41	9/15/41	9/16/41	9/18/41	9/20/41
7/20/42	10/10/42	10/19/42	10/20/42	10/20/42	10/21/42

Note: n.a. means that the conference committee procedure was bypassed.