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# Behavioral Responses to Estate Taxation: Evidence from Taiwan*

Hsien-Ming Lien Linda Wu Tzu-Ting Yang

**Abstract**

We quantify behavioral responses to estate taxation by exploiting two large and opposing reforms in Taiwan. A fundamental challenge is that estates are observed only at death, complicating treatment assignment. To address this, we link administrative estate and wealth records to implement a prediction-based difference-in-difference design. We estimate elasticities of taxable estates of 2.8 for the tax increase and 0.5 for the tax cut. Several patterns indicate wealth-shifting avoidance rather than real wealth changes: liquid assets reported at death diverge from pre-death holdings, closely held firm owners inflate liabilities to reduce book values, and heirs' labor supply remains stable despite sizable inheritance shocks. We interpret the asymmetry through a model of avoidance with sunk costs, suggesting that the tax-increase elasticity better reflects the long-run response. This would imply a revenue-maximizing rate of 21%, close to the OECD-average top statutory rate today.

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# 1 Introduction

Although extensive research indicates rising wealth concentration (Piketty and Zucman, 2014; Saez and Zucman, 2016; Alvaredo et al., 2017), estate tax policies vary widely worldwide. Ten OECD countries have abolished such taxes, while those that retain them have raised exemption thresholds substantially (OECD, 2021). For example, the U.S. exemption is now twelve times its 2001 level.[1] The policy divergence hinges on an unsettled debate over behavioral responses to estate taxes. Proponents argue these taxes promote equality, while critics emphasize distortions and the scope for avoidance. Quantifying these responses is therefore crucial to evaluating their redistributive effect.

Despite advances in optimal estate-tax theory (Farhi and Werning, 2013; Piketty and Saez, 2013), credible empirical estimates of estate-tax elasticities remain scarce. Earlier aggregate studies and recent work using bunching find elasticities of taxable estates around 0.1 (Kopczuk and Slemrod, 2001; Joulfaian, 2006; Glogowsky, 2021). These estimates would imply revenue-maximizing rates of 90%, far higher than rates in any country today. The disconnect underscores the need for more evidence, yet progress has been limited for three reasons. First, detailed microdata on estates are scarce and often truncated at exemption thresholds. Second, exogenous tax variation among top estate taxpayers is rare. Third, identification is difficult because each estate is observed only *once*, at death. For those who died before a tax change, we cannot observe their post-reform estates, and for those who died after, we lack their counterfactual estates.[2]

In this paper, we address these challenges by leveraging detailed administrative data and rich quasi-experimental variation in Taiwan. We link estate records to full-population wealth and income data. Crucially, the estate data include decedents below the exemption threshold, avoiding the truncation present in prior studies. We exploit two large reforms: a tax cut in 2009 that replaced progressive rates of 2% to 50% with a flat 10%, and a tax increase in 2017 that restored progressivity. These changes provide sharp variation in exemption thresholds and tax rates for studying behavioral responses.

We employ a difference-in-difference design comparing decedents likely to be affected by the tax changes (treated) to those likely to be unaffected (control). The key challenge

[1]The nominal exemption threshold for estate taxes in the U.S. was 675 thousand USD in 2001 and 13.61 million USD in 2024, which is around 12 times higher in real terms (Internal Revenue Service, 2024).

[2]This identification challenge also arises in other fields, such as studies on the impact of economic shocks on mortality (Sullivan and von Wachter, 2009; Finkelstein et al., 2024).

is that for decedents who died after the reform, we cannot observe what their estates would have been under the pre-reform tax regime. To address this, we predict treatment status based on their pre-reform characteristics. We train a machine learning algorithm on pre-reform decedents to predict whether their estates would have fallen above the reform cutoff. The algorithm's predictors include total wealth, detailed wealth components measured at least four years before death, and demographic characteristics. Among these, recent pre-death wealth is the strongest predictor. This approach allows us to predict treatment status for all decedents using predetermined pre-reform variables. For transparency and robustness, we also assign treatment based solely on pre-death wealth without machine learning and find similar results.

Our design relies on two assumptions. First, the parallel trends assumption requires that treated and control estates would have evolved similarly absent the reform. Second, the relationship between predictors and treatment status would have remained stable absent the reform. We provide evidence for both. Event study estimates show no pretrends between the treated and control groups. Algorithm performance and top influential predictors remain stable across pre-reform years, suggesting no structural changes in the relationship between lagged wealth components and estates at death.

Leveraging the two reforms, our analysis yields three findings. First, we document asymmetric behavioral responses to estate tax changes. Taxable estates respond quickly and persistently to both reforms, but more strongly to the tax increase than to the cut. The 2017 tax increase led to a sharp and persistent decline in taxable estates, whereas the 2009 tax cut produced a persistent increase that accumulated more gradually over time. Among the top 0.5% decedents, the group affected in both reforms, the elasticity of taxable estates with respect to the net-of-tax rate is 2.76 (s.e. 0.39) for the tax increase but only 0.47 (s.e. 0.13) for the tax cut. Decomposing estates shows that the asymmetry arises from liquid items such as financial assets, deposit savings, and charitable contributions and allowances, which respond more to the tax increase than to the cut. During the tax increase, 64% of the elasticity of estates is attributed to assets and 36% to deductions. In contrast, during the tax cut, almost all responses are from deductions.

Second, we examine whether these responses reflect wealth-shifting avoidance or real wealth changes by analyzing how both decedents and heirs adjust. On the decedent side, we first compare asset values reported at death with those held one year earlier. Treated decedents exhibit significantly larger discrepancies in stocks and deposit savings, but not in housing, which is illiquid and third-party reported. We then examine closely held firms, where owners' discretion over valuations can affect the private stock values assessed at death. In the years preceding death, closely held firms whose owners were subject to the tax increase reduced book values by inflating liabilities, even though revenues remained unchanged. The absence of changes in revenues is consistent with the shift in book values reflecting accounting adjustments rather than real operational activity.

On the heir side, we test whether reform-induced inheritance shocks affected their labor supply. Prior studies show that inheritance windfalls reduce labor supply (Holtz-Eakin et al., 1993; Elinder et al., 2012; Bø et al., 2019; Nekoei and Seim, 2023). If the estate responses represent genuine wealth changes, heirs would adjust their behavior accordingly. If instead they reflect avoidance that left actual transfers unaffected, heirs' behavior would not change. We test this in two steps. First, using heirs before the 2017 tax increase, we estimate a baseline windfall effect of a 2.4 (s.e. 0.6) percentage-point decrease in employment following inheritance. Second, we apply this baseline to the reform-induced changes. Based on how much the tax increase reduced reported inheritance, the baseline predicts an employment decline of only 0.2 percentage points following inheritance. However, post-reform heirs reduce employment by 2.3 (s.e. 0.6) percentage points, statistically indistinguishable from the baseline and inconsistent with the prediction. We find similar patterns for the 2009 tax cut. These findings suggest heirs did not respond as they would if the inheritance changes were real.

Finally, we develop an illustrative two-period model with sunk costs to rationalize the decedent and heir evidence that responses reflect wealth-shifting avoidance rather than real behavior, yet exhibit asymmetry. The framework formalizes the mechanism that accessing avoidance technologies requires irreversible fixed setup costs. As a result, when taxes fall, individuals who have already paid the fixed costs do not unwind avoidance because costs are sunk. By contrast, when taxes rise, individuals with existing structures expand avoidance, and for sufficiently large shocks, new entrants are incentivized to pay the fixed cost, yielding a larger response.

A key implication of the framework is that the tax-increase elasticity of 2.76 better reflects the long-run response unconstrained by sunk costs. Applying the sufficient statistics formula (Piketty and Saez, 2013) with this elasticity and our estimated Pareto distribution parameter of the taxable estate distribution yields a revenue-maximizing top tax rate of $1/(1+1.34\times 2.76) = 21\%$, which is close to the average top rate of 23% in OECD countries with bequest taxes (OECD, 2021). This finding contrasts with prior elasticity estimates of around 0.1, often derived from local variations or tax cuts, which would imply very high revenue-maximizing rates near 90%. While Taiwan's institutional setting may be specific, the underlying mechanism is not. Avoidance vehicles with setup costs are central to tax planning among the wealthy globally, including trusts in the U.S. (Hines, 2023) and offshore arrangements documented in the U.S. (Johannesen et al., 2020; Guyton et al., 2021) and Europe (Alstadsæter et al., 2018; Leenders et al., 2023).

**Related Literature and Contributions.** Our paper contributes to the literature on behavioral responses to estate and inheritance taxation in several ways.

First, we provide, to our knowledge, the first well-identified elasticities of taxable estates that leverage quasi-experimental variation from both a tax increase and a tax cut within the same institutional setting. This advances earlier work using time-series or state-level variation and recent bunching designs, which find elasticities around 0.1 (Kopczuk and Slemrod, 2001; Joulfaian, 2006; Glogowsky, 2021). Unpublished reform-based studies focusing only on tax cuts estimate higher elasticities of 0.8 to 1.9 (Escobar, 2017; Mas-Montserrat, 2019), while cross-sectional bunching estimates vary from null to 1.5 (Escobar et al., 2019). These disparities likely reflect empirical features: single-reform designs capture only one policy direction, and bunching estimates are often attenuated by adjustment frictions that confine identification to local kinks (Kleven, 2016). Recent wealth tax evidence finds bunching is absent at top-rate kinks (Jakobsen et al., 2020; Garbinti et al., 2023), consistent with our finding that bunching appears only at exemption thresholds but not at top-rate kinks where rates move within the positive region.

We advance beyond these methodological constraints by tackling the fundamental identification challenge in estate tax research where individuals only die once, so their estates cannot be observed under alternative tax regimes. To address this, we develop a novel prediction-based difference-in-difference design. We use pre-death wealth and demographic characteristics to predict counterfactual estates with machine learning, enabling credible comparisons of estates before and after tax changes. This approach yields credible elasticity estimates that capture the broader behavioral effects, derived from both a tax increase and a tax cut.

Second, we provide the first evidence of asymmetric responses to estate tax changes and rationalize them through a simple framework. While asymmetries have been documented in other tax contexts, such as value-added taxes (Benzarti et al., 2020, 2024; Benzarti, 2024) that are remitted by firms but borne by the broader population, estate taxes differ fundamentally as they target wealthy individuals. Our model contributes to the literature by showing how irreversible fixed costs in avoidance generate an asymmetry between tax increases and tax cuts. The framework clarifies why elasticities inferred from opposing policy directions are not interchangeable and why the elasticity identified from a tax increase may be more informative about longer-run responsiveness relevant for policy evaluation in settings characterized by sunk costs.

Third, our granular administrative data allow us to move beyond aggregate estate responses to identify specific channels for both decedents and heirs. On the decedent side, we decompose estates into components, compare lifetime holdings with values reported at death, and trace effects through closely held businesses. On the heir side, we provide the first evidence on how estate tax changes affect heirs' labor supply. Building on prior work that documents labor supply responses to inheritance windfalls using variation in the size and timing of inheritance (Holtz-Eakin et al., 1993; Elinder et al., 2012; Bø et al., 2019; Nekoei and Seim, 2023), we exploit quasi-experimental tax variation to test whether the changes in taxable estates represent real wealth transfers. We find no labor supply effects among heirs despite substantial inheritance shocks, indicating that the observed estate responses have limited real effects.

The rest of this paper is organized as follows. Section 2 describes the institutional context and data. Section 3 presents the identification strategy and results. Section 4 investigates the mechanism. Section 5 presents the model. Section 6 concludes.

# 2 Institutional Context & Data

## 2.1 Institutional Context

Estate taxation in Taiwan applies only to the very top of the wealth distribution. Approximately 4% of annual deaths in Taiwan are subject to an estate tax. This number is similar to the U.K.'s 4% and higher than the U.S.'s 0.1%.

**Tax Base.** Estate taxation in Taiwan is levied on the estate of the decedent. This system, also used in the U.K., the U.S., and South Korea, contrasts with the inheritance tax system in most European countries, where the tax is levied on the amount received by an heir. Taxable estates are defined as assets minus deductions, which is the statutory tax base before applying the exemption threshold. Assets include housing (land and houses), financial assets (publicly listed and privately held stock, bonds, futures, trusts, insurance, and other financial instruments), and deposit savings. Deductions fall into two categories. The first consists of charitable contributions and allowances, which include donations to nonprofit organizations, the spousal allowances under the Civil Law, and credits for estate taxes paid within the previous five years. The second consists of other deductible items, such as debts, farm, funeral, and demographic deductions for dependents, parents, or disabilities.

**Assessment and Enforcement.** Housing values are determined by prices announced by the government, which are adjusted every two years and serve as the basis for property taxes. These prices are generally lower than market prices. Ownership of housing is third-party reported and can be verified through property tax records. Deposit savings are reported by the filer with supporting bank statements, but can also be cross-verified against third-party reported interest income.

Financial assets include publicly listed and privately held stock, and other self-reported assets. Taiwan maintains comprehensive shareholder registries for all firms, enabling third-party reporting of stock ownership even without dividend distributions. Publicly listed stock is valued at the market price on the date of death. Privately held stock is assessed at book value, defined as assets minus liabilities, on the date of death. Since most private firms do not keep daily balance sheets and are often closely held by the decedent, this creates opportunities for manipulation. Other self-reported assets include trusts, insurance, cash, jewelry, and offshore wealth, which the government has limited ability to detect. Appendix Table A.1 provides more details.

Penalties for underreporting depend on the amount owed and have changed over time. Before 2009, if the shortfall was less than 35,000 TWD (about 1,100 USD), the taxpayer must pay the missing tax plus interest but incurs no fine.[3] After 2009, this threshold increased to 60,000 TWD (about 1,900 USD). For larger shortfalls above these thresholds, fines range from one to two times the unpaid tax. In cases of fraud, penalties can include fines of one to three times the owed tax as well as criminal charges.

**Estate Tax Schedule.** Figure 1 illustrates the estate tax schedule in Taiwan between 2004 and 2019. Between 2004 and 2005, it was a progressive schedule where the marginal tax

[3] All TWD to USD conversions use the average 2016 exchange rate of 32 TWD per USD.

rates (MTRs) ranged between 2% and 50%. The exemption threshold was 7 million TWD (219,000 USD). In 2006, the exemption threshold was adjusted for inflation, increasing to 7.79 million TWD, while the MTRs remained the same.

In October 2008, the government announced significant changes to take effect on January 23, 2009. First, the exemption threshold was raised by approximately 50%, from 7.79 million TWD to 12 million TWD (243,000 USD to 375,000 USD). Second, the progressive tax rates between 11% and 50% were replaced with a flat rate of 10%. The announcement was unexpected, as the government had only taken office in May 2008 and the reform was not part of the electoral discourse.

The reform aimed to achieve several objectives. Policymakers sought to attract wealthy individuals to repatriate offshore investment, with an estimated 1.2 trillion TWD (37 billion USD) flowing overseas each year before 2008. The government believed that the reform could stop the funds from going overseas, especially during the unstable financial outlook of the global crisis. In addition, the lower tax rate was intended to discourage tax planning, and the higher exemption threshold was designed to facilitate succession planning for small and medium-sized family businesses.

In 2016, following a change in government, the new administration proposed an estate tax increase to help finance the Long-Term Care Services Act 2.0. The reform restored the progressivity of the schedule by introducing two new MTRs of 15% and 20% for the top 0.5%, effective May 17, 2017.

**Gift Tax Schedule.** Understanding Taiwan's gift tax is crucial for interpreting our estate tax results because gifts could otherwise serve as a substitute channel for tax avoidance. Taiwan explicitly coordinates its gift and estate tax systems to prevent such substitution. Gifts made within two years of death are added back to the estate tax base and revalued as of the date of death, with any gift tax already paid credited against the estate liability. This ensures that near-death transfers are effectively subject to estate taxation regardless of their timing. Similar provisions exist in other countries, such as the U.K. and the U.S.[4]

Alongside this two-year inclusion rule, the gift tax schedule has historically moved hand in hand with the estate tax schedule. Appendix Figure A.1 shows the gift tax schedule over time. Before 2006, the gift tax was progressive with a 1 million TWD

[4]In the U.K., gifts made within seven years of death are counted towards the estate tax base, although differential tax rates would be applied depending on the timing (I.H.T.A. 1984 §7(4)). In the U.S., the gross estate includes certain specific transfers made within three years of death (I.R.C. §2035(a)).

exemption (31,000 USD). In 2006, the exemption rose slightly to 1.11 million TWD. The 2009 reform doubled the exemption to 2.2 million TWD (69,000 USD) and replaced progressive rates with a flat 10%. In 2017, progressivity was restored with 15% and 20% brackets for top gifts. This institutional coordination matters for our analysis because it limits short-run substitution between gifts and estates, ensuring that the estate tax responses we estimate are not simply driven by timing shifts between the two tax bases.

## 2.2 Data and Sample

We use administrative data from the Fiscal Information Agency in Taiwan. While Taiwan does not have a wealth tax, wealth information can be derived from other tax sources.

**Estate Tax (2004-2019).** The estate data contains all estate tax records filed to the government. It contains the individual's ID, date of death, taxable estates, assets, and deductions. The data are organized into a master file that reports total estate values and component subfiles that break down assets by housing, financial assets, and deposit savings. Financial assets at death include both stocks (publicly listed and privately held) and other instruments such as bonds, futures, trusts, insurance, and other financial instruments.

**Property Registry (2004-2019).** The property registry files provide information on land and housing. The land data is collected for land taxes, and the housing data is collected for house taxes. Both datasets contain individual IDs, property IDs, property area, location, and government-assessed values that are updated every two years.

**Shareholder Registry (2004-2019).** These data originate from corporate income tax filings under Taiwan's imputed dividend tax credit system, which linked corporate and personal income taxation. Firms report all shareholders and their equity stakes so that tax credits can be assigned proportionally when corporate income taxes are paid. The registry therefore covers ownership and quantities of both publicly listed and privately held shares, even if dividends are not distributed. It includes individual IDs, firm IDs, and the number of shares held.

**Income Registry (2004-2021).** The income registry files include information on various income types, such as interest, labor, dividend, rental, and other income. It contains individual IDs, the type of income, values, and the firm from which the income was earned, where applicable.

**Wealth Construction.** We define wealth as the sum of housing, stock, and deposit savings. When constructing these values, we follow the estate assessment rule. Housing values are based on government-assessed prices. Publicly listed stock is valued at its market price using data from the Taiwan Economic Journal (Lien et al., 2021). Privately held stock is valued at the book values reported in the corporate income tax data. For deposit savings, we capitalize interest income following Saez and Zucman (2016).

**Sample Construction.** To construct our decedent sample, we extract individuals who died above the age of 30 between 2004 and 2019. We merge their estate records with lifetime information from the property, shareholder, and income registries. We then apply two restrictions. First, we drop cases where the manually calculated estate from summing the component subfiles differs substantially from the total estate values in the master file to address likely data errors. This removes roughly 1% of the raw sample. Second, we trim extreme outliers by excluding the single largest taxable estate in each year, corresponding to the top 0.0005 percentile of the distribution.

## 2.3 Graphical Evidence on Bunching at Exemption Thresholds

We begin by showing that estate taxpayers are attentive to exemption thresholds. Figure 2 traces estate distributions in 200 thousand TWD bins around the thresholds as they changed over time. Panel A presents the distribution before 2006, where there was an excess mass at the old exemption threshold (7 million TWD), denoted by the gray dashed line. In 2006, the exemption threshold was adjusted by 10% to account for inflation, moving from the gray dashed line to the red dashed line (7.79 million TWD). As shown in Panel B, within a year the excess mass disappeared at the old kink and an excess mass appeared at the new exemption threshold. The response was persistent and strong through the end of 2008. A similar pattern happened when the 2009 reform increased the exemption threshold by 150% (to 12 million TWD). Panel C shows the immediate shift in bunching from the old to the new threshold, which persisted throughout subsequent years, as shown in Panel D. These patterns demonstrate that taxpayers were attentive to exemption changes and adjusted their planning accordingly.

To quantify these visual patterns, we apply the bunching method of Saez (2010) to estimate elasticities. We find significant bunching only at exemption thresholds, not at other kink points in the tax schedule. Appendix Figure A.2 shows clear excess mass at exemption thresholds. Panel A shows bunching at the 12 million TWD threshold where the marginal tax rate increases from 0% to 10% between 2009 and 2019, yielding an elasticity of 0.45 (s.e. 0.12). Panel B shows bunching at the 7.79 million TWD threshold where the marginal tax rate increases from 0% to 2% from 2006 to 2008, with an elasticity of 2.56 (s.e. 0.66). In contrast, Panels C to F show no statistically significant bunching at any within-positive kinks where marginal rates increase between positive levels.

This pattern highlights two points. First, the finding that a smaller rate jump (0% to 2%) produced a larger elasticity than a larger rate jump (0% to 10%) at exemption thresholds suggests responses are not proportional to tax rate changes, pointing to behavioral mechanisms beyond standard optimization. Second, it is consistent with recent wealth tax literature, which finds taxpayers respond to discrete discontinuities such as exemption thresholds or reporting requirements, rather than to smooth within-positive rate changes (Garbinti et al., 2023; Londoño-Vélez and Ávila Mahecha, 2024). The absence of bunching at within-positive kinks indicates that threshold responses may reflect non-classical behavior that standard bunching methods cannot fully characterize. This limitation motivates our main empirical strategy in the next section, where we turn to a difference-in-difference design to capture broader behavioral responses.

# 3 The Effect of Estate Tax Changes

## 3.1 Difference-in-difference Design

**Goal.** Our objective is to estimate the causal effect of estate tax reforms on taxable estates. We use a difference-in-difference design by comparing those whose pre-reform estates are above the reform cutoff (treated) and those below (control). Let $EstatePercentile_{t(i)}(\tau)$ be the percentile of the taxable estate of decedent $i$ who dies in year $t$ under schedule $\tau \in \{\tau_0, \tau_1\}$ where $\tau_0$ is the pre-reform tax schedule and $\tau_1$ the post-reform schedule. Treatment status is defined relative to the reform cutoff $C$:

$$T_i = \mathbb{1}\left[EstatePercentile_{t(i)}(\tau_0) \geq C\right]$$

For individuals who died *before* the reform, we directly observe $EstatePercentile_{t(i)}(\tau_0)$ and can assign treatment status straightforwardly. However, for individuals who died *after* the reform, we only observe their estates under the post-reform tax schedule $\tau_1$. Defining treatment status using $EstatePercentile_{t(i)}(\tau_1)$ would create endogeneity because post-reform estates may reflect behavioral adjustments in response to the reform.

**Prediction Idea.** To address the above challenge, we predict individuals' treatment status using our rich panel data on their pre-death wealth, income, and demographics. We use *pre-reform* decedents to model the relationship between treatment status and their pre-death characteristics:

$$T_i = \mathbb{1}\left[f(\mathbf{W}_{t(i)-k}, \mathbf{W}_{t(i)-k-1}, \ldots, \mathbf{X}_i) + \eta_i \geq a\right]$$

where $f(\cdot)$ maps pre-death covariates into a latent index. The vector $\mathbf{W_{t(i)-k}}$ is a vector of wealth and income variables measured $k$ years before $i$'s death year $t$. $k$ is large enough to ensure that for post-reform decedents, $\mathbf{W_{t(i)-k}}$ is predetermined and unaffected by the reform. $\mathbf{X_i}$ is a vector of demographic variables. $\eta_i$ is a noise term. $a$ is the latent threshold corresponding to the estate-percentile reform cutoff $C$.

For all decedents, regardless of dying before or after the reform, we observe their vectors $\mathbf{W_{t(i)-k}}, \mathbf{W_{t(i)-k-1}}, ..., \mathbf{X_i}$, which are all predetermined prior to the reform. We can therefore apply the estimated model to predict a treatment status for each individual, indicating whether their estate percentile under the pre-reform schedule would have been above the cutoff.

$$\widehat{T_i} = \mathbb{1}[\widehat{f}(\mathbf{W}_{t(i)-k}, \mathbf{W}_{t(i)-k-1}, \ldots, \mathbf{X}_i) \geq a]$$

This predicted treatment status can be expressed in terms of the true treatment status and a classification error term:

$$\widehat{T_i} = T_i \cdot (1 - e_i) + (1 - T_i) \cdot e_i$$

where $e_i \in \{0, 1\}$ is a classification error coming from the prediction model; $e_i = 0$ means the algorithm perfectly predicts the treatment status.

Once treatment status is predicted, we estimate a diff-in-diff regression:

$$Y_i = \alpha_0 + \alpha_1 \widehat{T_i} \times Post_i + \alpha_2 \widehat{T_i} + \alpha_3 Post_i + u_i \tag{1}$$

where $Y_i$ is the outcome of decedent $i$. $\widehat{T_i}$ is the predicted treatment status of $i$. $Post_i$ is 1 if $i$ dies after the reform, and $u_i$ is the error term. We use robust standard errors.

The predicted treatment status dummy introduces a misclassification error that biases the coefficient $\alpha_1$. In simple cases this bias is usually attenuating, but with machine-learning predictions the direction is not always clear (Battaglia et al., 2025). Our objective, however, is not to interpret $\alpha_1$ directly but to compute a Wald estimator of the elasticity of taxable estates with respect to the net-of-tax rate. Under the assumption that prediction errors are stable over time absent the reform, the same prediction error enters both the reduced form and the first stage, and thus cancels in the ratio. This ensures that the Wald estimator provides a consistent estimate of our parameter of interest (see Section 3.2).

**Prediction Implementation.** In the previous paragraphs, we described the prediction concept in a binary classification setting for simplicity. In practice, we classify decedents into multiclasses based on their estate percentiles. We illustrate the procedure using the 2017 reform as an example, though the same approach applies to the 2009 reform. Specifically, decedents are classified into four percentile groups: (i) below the 90th percentile, (ii) between the 90th and 96th percentiles, (iii) between the 96th and 99.5th percentiles, and (iv) above the 99.5th percentile.

We train a random forest algorithm on the pre-reform decedents who died between 2014 and 2016. In other words, the $f(\cdot)$ function described previously is a random forest algorithm here. This training step establishes the relationship between which one of the classes a decedent falls in and predictors, such as total wealth and different wealth and income component values, all measured four to ten years before death, and demographic variables.[5] Appendix Section B.1 provides details on the prediction procedures. Appendix Figure B.1 shows that the algorithm identifies recent years' total wealth values as the most influential predictors. Appendix Table B.1 presents the model performance.

Then, we apply the trained algorithm to all decedents who died before and after the reform, i.e., between 2014 and 2019. As we observe all decedents' predictors, which are all predetermined and unaffected by the reform, the algorithm assigns each decedent a predicted class. This predicted class indicates which class a decedent's estate percentile would be in if they died under the pre-reform tax system. Analogous prediction details for the 2009 reform are in Appendix Figure B.2 and Appendix Table B.2.

We define the treated group as those predicted to be above the 99.5th percentile for

[5] We use predictors from more than four years before death to ensure they are predetermined and unaffected by the reform. For example, for individuals who died in 2019 (the last year in our study), predictors are measured between 2006 and 2015.

the 2017 reform and above the 96th percentile for the 2009 reform, respectively. In both cases, we use those predicted to be between the 90th and 96th percentiles as controls to ensure consistent comparability across reforms. Appendix Table B.3 presents summary statistics for pre-reform decedents in these predicted groups. The groups are balanced on demographics but differ in taxable estates, which is the treatment dimension of interest.

**Event Study Specification.** We group decedents by their death dates into half-year calendar periods (e.g., 2017Q1, 2017Q3, . . . ) and estimate:

$$Y_i = \alpha_0 + \sum_{k \neq Base} \alpha_{1k} \widehat{T_i} \times \mathbb{1}[DeathPeriod_i = k] + \alpha_2 \widehat{T_i} + \sum_{k \neq Base} \alpha_{3k} \mathbb{1}[DeathPeriod_i = k] + \epsilon_i \quad (2)$$

where $Y_i$ is the outcome of decedent $i$. Treated decedents are defined as those predicted to be above the 99.5th percentile for the 2017 reform and above the 96th percentile for the 2009 reform, respectively. In both cases, the control groups are those predicted between the 90th and 96th percentiles. $\mathbb{1}[DeathPeriod_i = k]$ equals 1 if decedent $i$ dies in half-year calendar period $k$. The omitted base period, denoted $Base$, is the half-year before each reform, which is 2016Q3 for the 2017 tax increase and 2008Q3 for the 2009 tax cut.

**Identification Assumptions.** The identification relies on two assumptions. First, the classic parallel trends assumption requires that treated and control groups would have evolved similarly absent the reform. Second, the relationship between our predictors and treatment status would remain stable over time absent the reform. As shown in the results below, event studies reveal that treated and control groups exhibit similar trends prior to the reforms. We validate both assumptions through multiple robustness checks in Section 3.4, including tests of algorithm stability, simple wealth-based treatment assignment without machine learning, alternative control groups, anticipation effects, placebo reforms, and other sensitivity analyses.

## 3.2 Elasticity Identification

The key parameter of interest for policy purposes is the elasticity of taxable estates with respect to the net-of-tax rate, i.e., when there is a 1% change in the net-of-tax rate, how much the taxable estate would change in %.

$$\varepsilon = \frac{\text{\% Change in Reported Estate}}{\text{\% Change in Net-of-tax Rate}}$$
$$= \frac{\mathbb{E}[\Delta \log Estate|T = 1] - \mathbb{E}[\Delta \log \text{Estate}|T = 0]}{\mathbb{E}[\Delta \log(1-\tau)|T = 1] - \mathbb{E}[\Delta \log(1-\tau)|T = 0]}$$

The elasticity is the effect of the reform on taxable estate (reduced-form) scaled by the effect of the reform on tax rate change (first-stage). In practice, we proxy the treated dummy with the predicted one and estimate the following:

$$\text{First stage: } \log\left(1-\tau_i^{Post}\right) - \log\left(1-\tau_i^{Pre}\right) = \beta_0 + \beta_1 \widehat{T}_i + v_i \tag{3}$$

$$\text{Reduced form: } \log Estate_i = \alpha_0 + \alpha_1 \widehat{T}_i \times Post_i + \alpha_2 \widehat{T}_i + \alpha_3 Post_i + u_i \tag{4}$$

For the first stage, we use pre-reform decedents and construct mechanical marginal tax rate changes on the left-hand side by applying both the old and new marginal tax rates to their realized taxable estates. The coefficient $\beta_1$ captures exposure to tax changes absent the behavioral effect. The reduced form uses all decedents to estimate the treatment effect on taxable estates. Taking the ratio of the two stages yields the implied elasticity. Since predicted treatment status introduces misclassification, both $\widehat{\beta}_1$ and $\widehat{\alpha}_1$ are biased. However, under the assumption that prediction errors are stable pre- and post-reform absent the reform, the biases are identical in both equations and cancel in the Wald ratio, yielding a consistent elasticity estimate. See Appendix Section B.4 for details.

$$\hat{\varepsilon} = \frac{\widehat{\alpha}_1}{\widehat{\beta}_1} \xrightarrow{p} \frac{\alpha_1}{\beta_1}$$

## 3.3 Results

**2017 Tax Increase.** We first examine the event study results for the 2017 estate tax reform, which increased the MTRs for the top 0.5% (see Figure 1). Panel A of Figure 3 presents event study coefficients from Equation (2). The reform was implemented in May 2017. The event study reveals no statistically significant differences in the pre-trends of the treated and control groups, suggesting our estimated effects are not driven by pre-existing differential trends. After the tax increase, taxable estates declined quickly. They dropped by 18% by the end of 2017 and oscillated around 20% until 2019, showing an immediate jump followed by a relatively flat path.

Table 1 summarizes the diff-in-diff and elasticity estimates. Column (1) presents the results of the first-stage, reduced-form, and implied elasticities for the 2017 tax increase. The first row of the first stage shows that the treated group experienced a 7.4% decrease in their net-of-tax rates compared to the control. The reduced form estimates indicate that they decreased their taxable estates by 20.4% after the reform. The implied elasticity is 2.76 (s.e. 0.39), which means that with a 1% decrease in the net-of-tax rate, taxable estates decreased by 2.76%.

**2009 Tax Cut.** We apply the same empirical strategy to the 2009 tax cut. The reform impacted the top 4%. We define the treated group as decedents with estates above this threshold and use the same control group definition (90th-96th percentiles) as used in the 2017 reform to ensure comparability across the two reforms. Panel B of Figure 3 presents the event study of the 2009 tax cut, estimated using the same Specification (2). The reform was implemented in January 2009. Again, we find no statistically significant differences in pre-reform trends between treated and control groups, indicating that our results are not confounded by pre-existing trend differences. Following the reform, the treated group's taxable estates rose by about 8% by the end of 2009 and continued to grow to nearly 20% by 2011, reflecting a more compounded adjustment over time.

Column (2) of Table 1 reports the implied elasticity. On average, the treated faced a 9.9% increase in their net-of-tax rates and responded with a 13.1% increase in their taxable estates. This yields an elasticity of 1.32 (s.e. 0.16). In other words, when the net-of-tax rate increased by 1%, taxable estates increased by 1.32%.

**Heterogeneity within the 2009 Tax Cut.** We further explore heterogeneity in responses by splitting the treated group into two subgroups: (i) top 4%-0.5% and (ii) top 0.5%. The latter serves as a benchmark for comparison with the treated group in the 2017 tax increase. Panel C of Figure 3 illustrates the evolution of the taxable estates of the subgroups, using the 90th to 96th percentiles as controls. The estimates of the top 4%-0.5% in blue and those of the top 0.5% are in orange. Again, we find no statistical difference in pre-trends. In terms of responsiveness, the top 4%-0.5% show rapid increases in their taxable estates, with approximately a 10% increase within the first period after the reform. The responses persisted and grew over the years. As for the top 0.5%, the estimates are noisier due to a smaller sample. While there is an upward trend in their taxable estates in the first two years after the reform, the estimates are not statistically significant until 2011, where their responses show a salient increase and reach around a 30% increase by the end of 2011.

When we consider the magnitude of the net-of-tax rate changes they faced (the first stage), the discrepancy between them becomes salient. Column (3) and (4) of Table 1 present the results. Despite facing different tax changes (7.2% vs. 33.1% in net-of-tax rates), their estate responses are similar (13.8% vs. 15.4%). As a result, their elasticities differ starkly: 1.92 (s.e. 0.21) for the former and 0.47 (s.e. 0.13) for the latter.

**Interpretation.** Two patterns from Table 1 stand out. First, within the 2009 tax cut, the top 4%-0.5% show larger elasticities than the top 0.5%, suggesting heterogeneous responses across estate levels. A likely explanation is that the very wealthiest had already incurred fixed avoidance costs, such as setting up trusts or paying legal fees, before the reform. This limited their ability to adjust further, while those slightly lower in the distribution had more room to respond.

Second, across reforms, elasticities are asymmetric for the top 0.5%. The elasticity resulting from the tax increase is almost six times larger than that of the tax cut. We show in Section 3.4 that these patterns are not driven by macroeconomic shocks or near-death gifting, and in Section 4 we examine the mechanisms behind the asymmetry. While asymmetric tax responses have been documented for other taxes (Benzarti et al., 2020, 2024), our study provides the first evidence of such asymmetry in estate taxation.

**Elasticity Comparison with Prior Literature.** Table 2 summarizes existing estimates of the elasticity of taxable estate or inheritance with respect to the net-of-tax rate. The literature falls into two waves. The earlier wave uses aggregate time-series variation and reports elasticities around 0.1 or imprecise estimates due to large standard errors. A more recent wave exploits micro-level variation using difference-in-difference, bunching, or regression discontinuity designs, though few of them have been published and their estimates vary across methods and contexts.

Panel A presents estimates using time-series variation and U.S. data with an OLS estimation. The results range from imprecise estimates spanning -0.11 to 0.09 in Kopczuk and Slemrod (2001) to around 0.14 (s.e. 0.05) in Joulfaian (2006).

Panel B presents estimates using a diff-in-diff method. Prior work has focused exclusively on tax cuts. For example, Mas-Montserrat (2019) exploits a Catalan inheritance tax cut on close heirs and reports an elasticity of 1.88 (s.e. 0.23) among the top 5%-1%. Our

2009 tax cut estimates fall in a comparable range, with 1.92 (s.e. 0.21) for taxpayers in the top 4%–0.5% range, though lower at 0.47 (s.e. 0.13) for the top 0.5%. Our 2017 estimate provides the first evidence from a tax increase, yielding an elasticity of 2.76 (s.e. 0.39). These findings show that behavioral responses depend not only on taxpayer position in the distribution but also on the direction of tax change.

Panel C presents the estimates using bunching at exemption thresholds where marginal tax rates move from zero to a positive rate. The only existing study in this area is Escobar et al. (2019), who examine the Swedish inheritance tax repeal and estimates bunching at one exemption kink and two within-positive kinks. They report an elasticity of 1.53 (s.e. 0.10) for the exemption kink where the marginal tax rate increases from 0% to 10%. Our bunching estimates from an exemption kink vary between 0.45 (s.e. 0.12), associated with a 0% to 10% increase, and 2.56 (s.e. 0.66), associated with a 0% to 2% increase. Although their estimate lies within the range of ours, the non-monotonic relationship suggests that individuals do not respond proportionally to the magnitude of the tax change at exemption thresholds, indicating that factors beyond rate changes influence behavior.

Panel D displays the bunching estimates from within-positive kinks where marginal tax rates increase within the positive range. The results are mixed. While Glogowsky (2021) and one of the estimates of Escobar et al. (2019) are statistically significant, the other estimate of Escobar et al. (2019) and all our estimates are statistically insignificant. Despite the mixed findings, the results are consistent on one point: estimates from within-positive kinks are much smaller than those from exemption kinks and also smaller than those derived from the diff-in-diff estimation. This indicates that taxpayers show little adjustment to incremental rate changes once they are above the exemption threshold, reinforcing the view that bunching methods capture local responses, which may be attenuated when frictions or lumpy adjustments constrain behavior. Panel E includes one estimate using an RDD from Escobar (2017) at 0.76 (s.e. 0.31).

Our estimates contribute to the literature in several ways. First, we provide the first well-identified estate elasticities exploiting quasi-experimental variation from both a tax increase and a tax cut within a single institutional setting. Second, we help bridge the gap between the estate and wealth tax literature. While earlier estate tax studies find elasticities around 0.1, recent diff-in-diff based wealth tax papers obtain much larger elasticities of taxable wealth w.r.t. net-of-tax rate (around 5 in Denmark Jakobsen et al. (2020) and around 0.8 to 4 in Colombia (Londoño-Vélez and Ávila Mahecha, 2024)). Third, our bunching results also mirror recent wealth tax evidence showing that bunching does not always appear at top-rate kinks (Jakobsen et al., 2020; Garbinti et al., 2023). Finally, our dual-reform setting allows us to distinguish responses to tax increases from those to tax cuts, revealing asymmetric behavior that has not been documented in prior work on either estate or wealth taxation. We explore the mechanism behind this asymmetry in Section 4.

### 3.4 Robustness Checks

**Stability of Algorithm.** We assess the stability of our prediction algorithm using two exercises. First, for each pre-reform year, we randomly split the data into a training and hold-out sample to test whether the model overfits year-specific noise. For the 2017 reform, Appendix Figure B.3 shows that the top predictors are stable across years. While their rankings vary slightly, the same variables consistently dominate. Appendix Table B.4 further shows similar prediction performance across years. Comparable patterns hold for the 2009 reform in Appendix Figure B.4 and Table B.5. Second, we train the model on one pre-reform year and test it on another. This mimics our baseline identification, which applies models trained on pre-reform data to post-reform periods. Appendix Table B.6 shows that the prediction performance of the 2017 reform is consistent across adjacent and non-adjacent years. We find no differences across the pre-reform years, supporting the stability of the model. Appendix Table B.7 shows similar patterns for the 2009 reform.

**Wealth-based Classification without Machine Learning.** While the algorithm appears stable across years, one may still be concerned that our findings hinge on the use of machine learning. We implement a transparent wealth-based rule that classifies treated and control groups directly based on pre-reform total wealth. This approach is motivated by the fact that the most recent total wealth variables have consistently been the key drivers of our prediction ML algorithm's classifications. For the 2017 tax increase, we define the treated group as those whose wealth four and five years before death were in the top 0.5%, and the control group as those between the 90th and 96th percentiles. Similarly, for the 2009 tax cut, we use wealth from three years prior to death to define these groups. Appendix Table B.8 presents the results, showing that the estimated elasticities lie within the 95% confidence intervals of our baseline estimation in Table 1.

**Alternative Gradient Boosting Algorithm.** We test the sensitivity to the choice of machine learning method by replacing the baseline random forest with a gradient boosting model. For the 2017 reform, Appendix Figure B.5 shows that the top predictors identified by gradient boosting mirror those from the baseline. Prediction performance across groups is likewise similar, as reported in Appendix Table B.9. For the 2009 reform, we find the same patterns in Appendix Figure B.6 and Table B.10.

**Placebo Reform Years.** To show that the treatment effects we captured in the results are not driven by algorithmic bias, mean reversion, or aspects unrelated to the reforms, we randomly assigned years as placebo reform years. For a given placebo reform year, we implement the prediction algorithm and diff-in-diff estimation as described in Section 3.1. Appendix Section B.8 describes the procedures and Appendix Table B.11 presents the results. We find no treatment effect in any of the placebo years.

**Alternative Control Group.** In our baseline, we use the P90-P96 group as the control for both reforms to ensure comparability. The choice of the control group involves a tradeoff: selecting a control group whose estates evolves similarly to the treated group (closer to the top percentiles) while avoiding contamination from the reform (lower down in the percentiles). Appendix Table B.12 shows that for the 2017 reform, elasticity estimates are similar when using P90-P93, P93-P96, and P96-P98 as the control group. The estimate becomes insignificant when using the immediately lower group (P98-P99.5), potentially because they behaved in a forward-looking manner and chose not to cross the threshold, consistent with Garbinti et al. (2023). For the 2009 reform, the estimates are similarly consistent with the P85–P90 control group, except again for the immediately below group.

**Anticipation.** If taxpayers anticipated the reforms, they might have adjusted behavior beforehand, biasing estimates downward. We test this by assuming the reform occurred one year earlier (2016 for the 2017 increase; 2008 for the 2009 cut). Appendix Table B.13 reports the elasticity estimates, showing that the results are similar to the baseline.

**Macroeconomic Conditions.** A concern is that the stronger response to the tax increase than to the tax cut may reflect different macroeconomic conditions, as the 2009 cut coincided with the financial crisis while the 2017 increase occurred in a stable economy. We address this concern in three ways. First, as shown in Section 4.1, even highly liquid assets such as deposit savings display asymmetric responses. Second, Appendix Figure B.7 shows Taiwan's stock market trading volume recovered to pre-crisis levels by early 2009. Third, Appendix Table B.14 shows no differential responses between decedents with high vs. low pre-reform stock exposure, proxied by their exposure to the financial crisis, suggesting that the economic downturn in 2009 does not explain the asymmetry. See Appendix Section B.11 for details.

**Gifting.** One potential concern is that the behavioral response we capture reflects gifting during lifetime; however, strategic gifting cannot explain our findings. As described in Section 2.1, Taiwan's tax system restricts this channel: gifts made within two years of death are included in the estate tax base, and gift and estate tax rates move in tandem. Appendix Table B.15 shows that decedents actually reduced gifts within two years before death after the 2017 tax increase, while the 2009 tax cut produced no significant change. These patterns are consistent with the relative costs of gifting compared to other avoidance strategies and with individuals' expectations about survival within the two-year window, as illustrated in Appendix Tables B.16 and B.17. See details in Appendix Section B.12.

# 4 Mechanism and Discussion

The previous section documented that taxable estates respond quickly and persistently to both reforms, with stronger reactions to the tax increase than to the cut. This section examines the sources of these responses and their asymmetry. Section 4.1 decomposes estates by item, estimates the elasticity of each component with respect to the net-of-tax rate, and quantifies their contributions to overall estate responses. Section 4.2 then examines whether these responses reflect tax avoidance or real behavioral changes.

## 4.1 The Anatomy of Estate Items

To understand the drivers of estate responses and the asymmetry between the tax increase and tax cut, we decompose estates into components and estimate item-specific elasticities, focusing on the top 0.5% for consistent comparison across reforms.

$$\text{Estate} = \underbrace{(\text{Housing} + \text{Financial} + \text{Deposits})}_{\text{Assets}} - \underbrace{(\text{Other Deductions} + \text{Charity \& Allowances})}_{\text{Deductions}}$$

**Elasticities of Each Item.** We estimate elasticities for each estate component using the procedure in Section 3.2, replacing the dependent variable in Equation (4) with the levels of each item to handle zeros. We then scale coefficients by the treated group's pre-reform mean. Figure 4 illustrates the results. For ease of comparison with asset categories, we plot and discuss deduction elasticities with signs flipped, multiplying the estimates for other deductions and charitable contributions and allowances by −1.[6] The 2017 tax increase, shown in blue, generated strong responses in liquid items. Financial assets, deposit savings, and charitable contributions and allowances all show significant changes, with elasticities of 2.79 (s.e. 0.35), 1.87 (s.e. 0.43), and 2.39 (s.e. 0.67), respectively. By contrast, housing and other deductions show no significant response. The 2009 tax cut, shown in orange, exhibits a more modest pattern. Deposit savings and other deductions respond mildly, with elasticities of 0.38 (s.e. 0.17) and 0.18 (s.e. 0.09), respectively. Charitable contributions and allowances exhibit a stronger response, with an elasticity of 0.80 (s.e. 0.15). Across reforms, the key sources of asymmetry lie in financial assets, deposit savings, and charitable contributions and allowances, whose estimated elasticities differ significantly. These components explain the strong response to the 2017 tax increase and the more muted reaction to the 2009 tax cut.

**Decomposition of Contribution to Total Elasticity.** We next quantify each item's contribution to the overall elasticity of taxable estates by decomposing the overall elasticity into a weighted sum of item-specific elasticities, where each component's contribution is the product of its elasticity and its share in total estates:

$$\varepsilon^{Estate} = \sum_{Item} \varepsilon^{Item} \times \omega_{\frac{Item}{Estate}}$$

Table 3 reports the elasticity, weight, and contribution of each component. Panel A shows results for assets. Although liquid assets such as financial assets and deposit savings are more elastic, their smaller weight limits their overall impact. Panel B shows that among deduction items, charity and allowances exhibit the highest elasticity but contribute modestly because of their low weight. The composition of responses differs starkly between reforms. For the 2017 tax increase, assets drive 64% of the total elasticity versus 36% from deductions. For the 2009 tax cut, this pattern reverses dramatically: assets contribute only 0.3% while deductions account for 99.7%. This suggests that estate responses to the tax increase are primarily driven by asset adjustments, while responses to the tax cut are largely driven by deductions.

**Interpretation.** These results highlight two points. First, housing, an illiquid and third-party-reported item, is unresponsive in both reforms. This aligns with evidence linking

[6] Appendix Tables C.1 and C.2 report the underlying estimates.

tax avoidance opportunities to whether items are subject to third-party reporting (Kleven et al., 2011; Waseem, 2020; Londoño-Vélez and Ávila Mahecha, 2024). By contrast, financial assets, deposit savings, and charity and allowances, which are more liquid or easier to manipulate, are highly elastic. Second, the contribution analysis shows that the overall elasticity was much larger during the 2017 tax increase because adjustments in assets and deductions were quantitatively stronger than during the 2009 tax cut, where responses came mainly from deductions and were smaller in scale.

## 4.2 Distinguishing Tax Avoidance from Real Behavior

In this subsection, we examine whether the reported adjustments reflect tax avoidance or real behavioral changes, focusing on both decedents and heirs. On the decedent side, Section 4.2.1 compares asset values reported at death with those held one year prior, and Section 4.2.2 analyzes closely held firms to test whether owners strategically lowered book values before death. On the heir side, Section 4.2.3 investigates whether reform-induced inheritance shocks affected their labor supply.

### 4.2.1 Reported Asset Values at Death vs. Pre-death Holdings

To assess the role of reporting and avoidance, we estimate Equation (4) using the difference between asset values at death and those held one year earlier as the outcome, scaled by the decedent's total wealth three years prior to account for zeros. We focus on housing, financial assets, and deposit savings, which are observed in both the estate and wealth datasets, though not always in identical detail.[7]

Panel A of Table 4 shows the results for the 2017 tax increase. Treated decedents reported significantly lower values at death for liquid assets. Relative to the control group, the gap between financial assets reported at death and a year earlier, measured as a share of baseline wealth, fell by 10.8 percentage points (pp), while that for deposit savings declined by 1.5 pp. In contrast, housing does not show a statistically significant difference. These results are consistent with the patterns in Section 4.1, where treated decedents report lower amounts of financial assets at death following the reform. Panel B reports analogous results for the 2009 tax cut. The difference in deposit savings as a share of baseline wealth increased by around 10.2 pp, while financial assets and housing do not exhibit significant changes.

[7] Housing is observed in both datasets. For financial assets, estate records include the sum of stock, bond, and other instruments, whereas the lifetime measure is limited to stock. For deposit savings, estate records report actual balances, whereas lifetime savings are imputed by capitalizing interest income.

These results point towards strategic reporting as a driver of these discrepancies, as opposed to genuine changes in wealth. Within a one-year window, treated decedents show much larger discrepancies in liquid assets such as financial assets and deposits than the control group, while no such differences appear in illiquid, third-party-reported assets like housing. We next examine to what extent these discrepancies in stockholdings might reflect adjustments in the book values of closely held firms.

#### 4.2.2 Closely Held Firms' Reported Valuations

To further explore the discrepancy in stockholdings, we focus on closely held firms, where owners' discretion over valuations can directly affect their private stock values at death. We construct an event study of firm outcomes over the seven years preceding an owner's death. The treated group consists of firms whose owners were treated in the 2017 tax increase (predicted to be the top 0.5%) and who at any point in the seven years before death held more than one-third of the company's ownership.[8] For the control group, we use publicly listed firms, in which individual owners cannot influence valuations, and assign each firm a random placebo death year. We estimate the following regression, separately for firms whose owners died before and after the 2017 reform:

$$Y_{ft} = \sum_{k \neq -5} \beta_k \widehat{T}_f \times \mathbb{1}[t - DeathYr_f = k] + \sum_{k \neq -5} \mathbb{1}[t - DeathYr_f = k] + \alpha_f + \delta_t + u_{ft} \quad (5)$$

where $Y_{ft}$ is an outcome for firm $f$ in year $t$. Since book value could be zero or negative, we scale the outcome by the average of the firm's total assets between five and seven years before death. $\widehat{T}_f$ equals 1 for closely held firms and 0 for publicly listed firms. $DeathYr_f$ is the death year of the firm owner (or a placebo year for the control group). We omit $k = -5$ as the reference year.[9] $\alpha_f$ and $\delta_t$ are firm and year fixed effects. Standard errors are clustered at the firm level.

We first examine book value, defined as assets minus liabilities, which is the basis for assessing privately held stock in the estate tax base at death. We scale the outcome by the firm's average assets between five to seven years before death to account for non-positive values. Panel A of Figure 5 plots the event-study estimates. The coefficients represent the differential trajectories of closely held firms (treated) relative to publicly listed firms (control) as owners approach death. The blue estimates correspond to owners who died

[8] Appendix Figure C.1 shows robustness to alternative ownership thresholds 40% and 50%.

[9] Since the analysis covers seven years before death, this choice provides three pre-periods to test for pre-trends and four subsequent periods closer to death.

before the 2017 tax increase, and the orange ones are for those who died after. In the pre-reform period, both series track parallel paths up to roughly five years before death. After the reform, however, the orange series diverges downward about three years before death, while the blue series remains flat. By the final year of life, relative to the control group, closely held firms whose owners died after the reform report a 27 percentage points (pp) decline in the ratio of book value to baseline assets.

To understand what drives this decline, Panels B and C decompose book value into liabilities and assets, both scaled by baseline assets. Panel B shows that liabilities as a share of baseline assets of treated firms rose sharply after the reform, by about 56 pp relative to controls in the final year before death. Panel C shows that assets, which equal liabilities plus equity, also increased, but only by around 29 pp. Since book value is defined as assets minus liabilities, the fact that liabilities rose much more than assets drives a fall in book value. By contrast, the pre-reform blue series shows no such patterns, indicating these adjustments emerged only after the tax increase.

Are these changes genuine business activity or simply accounting adjustments? Panel D plots the logarithm of sales revenues. Both the blue and orange estimates remain flat before and after the reform, with no systematic differences relative to controls. The absence of revenue changes is consistent with the shift in book value being driven by accounting adjustments rather than by real operational activity.

#### 4.2.3 Inheritance Windfalls and Heirs' Labor Supply

We next examine heirs, whose labor supply responses provide a test of whether the estate responses reflect tax avoidance or real wealth transfers. Prior work shows that inheritances reduce heirs' labor supply (Holtz-Eakin et al., 1993; Elinder et al., 2012; Bø et al., 2019; Nekoei and Seim, 2023). If reform-induced changes in taxable estates are real wealth changes, heirs' labor supply would respond accordingly. If instead they arise through tax avoidance, labor supply would not move because the actual inheritance did not change. Our approach proceeds in two steps. First, we measure how inheritance changed as a result of the reforms. Second, we use pre-reform heirs to establish the baseline effect of inheritance windfalls on labor supply. We then apply this baseline to predict how heirs should have responded to the reform-induced inheritance shocks and compare the predictions with the observed post-reform outcomes.

We construct our heir sample by linking children to the decedents from the previous analysis and restricting to those aged 25–70 at the time of parental death who had positive labor income at least once before the parent died. This yields baseline employment of about 89%, comparable to 85% in Nekoei and Seim (2023) for Sweden and 82% in Holtz-Eakin et al. (1993) for the U.S., both of which examine heirs in broader populations rather than the top rich.

**Measuring Reform-Induced Changes in Inheritance.** We quantify how heirs' inheritances changed following the reforms by estimating the following specification:

$$Inheritance_{j(i)} = \alpha_0 + \alpha_1 \widehat{T}_{j(i)} \times Post_{j(i)} + \alpha_2 \widehat{T}_{j(i)} + \alpha_3 Post_{j(i)} + \epsilon_{j(i)} \tag{6}$$

where $j(i)$ indexes heir $j$ of decedent $i$. $Inheritance_{j(i)}$ is the average after-tax inheritance per heir, defined as the after-tax taxable estate of decedent $i$ divided by the number of heirs.[10] $\widehat{T}_{j(i)}$ is 1 if $j$'s parent $i$ is predicted to be the top 0.5% and 0 if between P90-P96. $Post_{j(i)}$ is 1 if $i$ died after the reform. The coefficient $\alpha_1$ identifies the change in average after-tax inheritance per heir induced by the reform.

Table 5 reports the results. On average, treated heirs received 44.1 million TWD (1.38 million USD) less after the 2017 tax increase and 8.7 million TWD (272,000 USD) more after the 2009 tax cut.[11] The difference between the two reforms reflects our earlier findings that the 2017 tax increase induced a larger effect than the 2009 tax cut. This substantial first stage sets the stage for testing whether these inheritance changes represent real wealth transfers or merely avoidance.

**Baseline Windfall Effects.** We begin by establishing the baseline effect of inheritance windfalls on labor supply using pre-reform heirs. We estimate an analogous event study as Equation (5) at the heir level, using those whose parents are predicted to be in the top 0.5% as the treated group and those between P90-P96 as controls. We estimate this regression separately for heirs whose parents died before and after the reform, with $k = -1$ being the reference period.

Figure 6 presents the results. Panel A shows the extensive margin, defined as employment probability measured by having positive labor income (employed earnings), around

[10] We construct inheritance per heir as the after-tax taxable estate divided by the number of heirs (sum of children and living spouse), assuming equal distribution since we don't observe actual individual bequests. We net out gifts made within two years before death, as these are included in the estate tax base.

[11] Values are expressed in 2016 TWD and USD. All TWD to USD conversions use the average 2016 exchange rate of 32 TWD per USD.

the 2017 reform. The blue series shows pre-reform heir cohorts and the orange series post-reform cohorts. Pre-reform treated heirs, who on average inherit 52 million TWD (1.63 million USD) more than controls according to our descriptive statistics, reduced their employment in the first year after parental death and continued in subsequent years. By the second year, employment fell by 2.40 (s.e. 0.62) percentage points (pp) from the baseline of 89%, persisting through the fourth year.[12] We use year two as the benchmark, by which point most heirs are likely to have received their inheritance. This yields a baseline windfall effect of −0.05 pp per million TWD, which is −0.16 pp per 100,000 USD.

Panel B shows the extensive margin estimates around the 2009 tax cut. As the heir sample is smaller in this period, the estimates are noisier with larger standard errors. Even so, pre-reform heirs' employment probability fell similarly by about 2.03 pp (s.e. 0.85) in the second year after parental death. On average, they received 12.1 million TWD (375,000 USD) more than controls, implying a baseline windfall effect of −0.17 pp per million TWD, which is −0.54 pp per 100,000 USD.

To put our baseline windfall estimates in context, we compare them with prior studies. Unlike our focus on heirs of the very wealthy, earlier work has generally examined much broader heir populations. On the extensive margin, Nekoei and Seim (2023) draw on population-wide Swedish data and find that a 9,350 USD inheritance gain lowered employment probability by 0.4 pp from a baseline employment of 85%, equivalent to about –4.3 pp per 100,000 USD.[13] Holtz-Eakin et al. (1993), using a 1% sample of U.S. estate tax returns, applied their logit estimates to simulate that a 781,000 USD inheritance would reduce employment by 11.9 pp from a baseline 89%, which is –1.5 pp per 100,000 USD.[14] By contrast, our estimates are much smaller than those in prior studies. The pre-2017 baseline estimate implies only –0.16 pp per 100,000 USD, while the pre-2009 baseline estimate implies –0.54 pp per 100,000 USD. Both are below the effects documented in broader heir populations.

**Predicted vs. Post-reform Observed Effects.** We apply the baseline windfall effects to the inheritance changes documented in Table 5 to predict heirs' responses. The 2017 tax increase reduced inheritances by 44 million TWD, while the 2009 tax cut increased them

[12] The lag in employment decline is consistent with settlement processes for large estates and the fact that our data do not capture the exact timing of distributions, as noted in Elinder et al. (2012).

[13] We convert the reported 58,000 SEK average inheritance to 2016 USD, assuming 2003 as the base year (midpoint of their 2001-2005 treatment effects).

[14] We convert the 350,000 USD simulation exercise reported on page 425 to 2016 USD assuming the number was based in 1985.

by 8.7 million TWD. For the 2017 tax increase, the baseline windfall effect predicts that a 44 million TWD reduction in inheritance would mitigate the negative impact on employment, changing the estimate from −2.4 pp to −0.2 pp (calculated as $-2.4+0.05\times44$) by the second year after parental death. However, Panel A of Figure 6 shows that the observed post-reform estimate in year two is −2.26 pp (s.e. 0.57) with a 95% confidence interval of [−3.38, −1.14], statistically indistinguishable from the pre-reform effect. Moreover, the predicted response of −0.2 pp falls outside this interval, suggesting that heirs' employment did not adjust as it would under a real-wealth windfall.

For the 2009 tax cut, the predicted effect suggests employment would decrease from −2.03 pp to −3.51 pp (calculated as $-2.03 - 0.17 \times 8.7$) by year two. Instead, Panel B of Figure 6 shows the observed post-reform estimate in year two is −1.18 pp (s.e. 0.71) with a 95% confidence interval of [−2.57, 0.21]. Again, the prediction of −3.51 pp falls outside this interval.

We next examine effects on the intensive margin, defined as log labor income. Panel C and D show that neither the pre- nor post-reform estimates show statistically significant effects, detecting no response on the intensive margin. Our null results are consistent with the mixed evidence in prior work. Nekoei and Seim (2023) report around a 1% drop in earnings, while Elinder et al. (2012) find a much smaller 0.02% decline. Holtz-Eakin et al. (1993) find no effect for single filers and only small negative effects for joint filers. Bø et al. (2019) find noisy estimates when examining effects on hours worked.

In summary, the heir results suggest that the observed estate tax responses reflect tax avoidance rather than real wealth change. If the reported inheritance shocks had represented true wealth transfer changes, heirs' labor supply would have responded in line with established windfall effects. Instead, we find no corresponding adjustments in labor supply. This implies that the heirs may be receiving just as much as before the reform, but through ways that bypass the tax authority.

**Summary and Interpretation.** The preceding analyses document four patterns in the administrative data pointing to wealth-shifting avoidance rather than real behavior: (i) estate responses are concentrated in liquid assets and manipulable deductions; (ii) discrepancies between values reported at death and holdings one year earlier are largest for liquid assets; (iii) closely held firms strategically reduce book values by inflating liabilities; and (iv) heirs show no labor supply response despite large reported inheritance changes.

Two potential channels that are not directly observed in the microdata but can be inferred from aggregate evidence are trust funds and offshore arrangements. Appendix Figure C.2 shows that trust fund sizes in Taiwan surged rapidly following the 2017 tax increase, expanding by 43% between 2017 and 2020. Offshore wealth plays another significant role. Appendix Figure C.3, based on estimates from Alstadsæter et al. (2018), shows that in 2007 offshore wealth equaled 22% of GDP in Taiwan, ranking 7th globally. Appendix Figure C.4 further shows that Taiwan has the highest per-capita ownership of shell companies in the Panama Papers.

A common feature of these avoidance vehicles is the presence of sunk costs. Establishing trusts or offshore shells requires upfront costs that are irreversible upon dissolution. Once these structures are in place, taxpayers have little incentive to unwind them following a tax cut but strong incentives to expand their use when taxes rise, generating the asymmetry observed empirically. We formalize this intuition in the next section.

# 5 An Illustrative Model of Asymmetry Under Fixed Costs

Building on the empirical findings documented above, this section presents an illustrative two-period model that shows how sunk costs in avoidance give rise to asymmetric estate tax responses. To convey the mechanism in the simplest qualitative way, we abstract from rational expectations regarding policy uncertainty. Agents perceive initial tax rates as permanent and subsequent rate changes are treated as unanticipated shocks. The model demonstrates why the elasticity identified from a tax increase better reflects the long-run behavioral response relevant for policy evaluation.

## 5.1 Setup

A continuum of agents indexed by $i$, each endowed with fixed wealth $w$, lives for two periods: Middle Age ($t = 1$) and Near Death ($t = 2$). In $t = 1$, agents know with certainty that they will die in $t = 2$ facing an estate tax rate $\tau \in [0, 1]$. They maximize the net-of-tax bequest left to their heirs upon death.

**Tax avoidance technology.** Let $s_{it} \geq 0$ denote agent $i$'s sheltered wealth stock in $t$. Accessing tax avoidance technology requires a fixed entry cost $\kappa_{it}$. We capture the cost advantage of early planning by assuming fixed costs increase with age: $\kappa_{i2} = \phi \kappa_{i1}$ with $\phi > 1$. $\kappa_{i1}$ is heterogeneous across agents and follows a continuous cumulative distribution function $G(\cdot)$ with support on $[0, \infty)$.

Conditional on participation, agents pay variable cost $\Gamma(s_{it})$, where $\Gamma' > 0$, $\Gamma'' > 0$, and $\Gamma'(0) = 0$. We assume $\Gamma'(w) > 1$ to ensure that even under a confiscatory tax rate the marginal cost of sheltering the last unit of wealth exceeds the marginal tax saving, so full sheltering is not optimal. Costs are paid on the accumulated stock and are sunk at the time of payment. Increasing shelter from $s_{i1}$ to $s_{i2}$ costs $\Gamma(s_{i2}) - \Gamma(s_{i1})$, while reducing shelter recovers nothing.

**Agent's problem.** Agents choose $\{s_{i1}, s_{i2}\}$ to maximize terminal net-of-tax estates:

$$\max_{s_{i1}, s_{i2} \geq 0} \quad U_i = \underbrace{(1-\tau)\cdot(w - s_{i2})}_{\text{after-tax estates}} + \underbrace{s_{i2}}_{\text{sheltered}} - \underbrace{C(s_{i1}, s_{i2}; \kappa_{i1}, \kappa_{i2})}_{\text{sheltering cost}}$$

where the total sheltering cost function is:

$$C(s_{i1}, s_{i2}; \kappa_{i1}, \kappa_{i2}) = 1[s_{i1} > 0]\cdot\kappa_{i1} + \Gamma(s_{i1}) + 1[s_{i2} > 0, s_{i1} = 0]\cdot\kappa_{i2} + \max\left\{0, \Gamma(s_{i2}) - \Gamma(s_{i1})\right\}$$

The fixed cost $\kappa_{i2}$ is paid only if the agent did not participate in $t = 1$, and the incremental variable cost reflects that reducing the sheltered stock yields no cost recovery.

**Equilibrium.** Since $\kappa_{i2} > \kappa_{i1}$, delaying participation is strictly dominated. Thus, agents either never enter or enter in $t = 1$ and maintain the same sheltered position in $t = 2$.

1. Optimal sheltered stock (intensive margin): Conditional on participation ($s_{i1} > 0$), the optimal sheltered amount is determined by the F.O.C. equating the marginal tax saving to the marginal cost:

   $$\tau = \Gamma'(s_{i1}^*) \quad \Rightarrow \quad s_{i1}^* = s_{i2}^* = s^*(\tau)$$

   Since the marginal cost is increasing ($\Gamma'' > 0$), the optimal stock $s^*(\tau)$ is strictly increasing in $\tau$, i.e., higher tax rates raise the marginal tax saving from sheltering.

2. Participation threshold (extensive margin): An agent enters if the utility under participation ($U_i^P$) exceeds non-participation ($U_i^{NP}$). The participation condition is:

   $$U_i^{NP} \leq U_i^P \Rightarrow \kappa_{i1} \leq \tau s^*(\tau) - \Gamma(s^*(\tau)) \equiv \bar{\kappa}(\tau)$$

The threshold $\bar{\kappa}(\tau)$ is strictly increasing in $\tau$, i.e., higher tax rates increase the benefit of sheltering, raising the fixed cost threshold below which participation is profitable.

The population consists of those who are: (i) Never-participants and (ii) Participants who entered in $t = 1$ and maintain in $t = 2$. The share of the population participating is $P(\tau) = G(\bar{\kappa}(\tau))$. The aggregate stock of sheltered wealth is $S_{agg}(\tau) = G(\bar{\kappa}(\tau)) \cdot s^*(\tau)$.

## 5.2 A Shock in Tax Rates

Now, consider an unexpected exogenous shock in $t = 2$ that shifts the tax rate from $\tau$ to $\tau'$. The decisions in $t = 1$ are sunk. We analyze the responses of (i) Non-participants who chose $s_{i1} = 0$ and (ii) Participants who chose $s_{i1} = s^*(\tau) > 0$.

### 5.2.1 Case 1: Tax Increase ($\tau' > \tau$)

**Non-participants ($s_{i1} = 0$).** Agents who did not previously shelter solve:

$$\max_{s_{i2} \geq 0} \quad (1 - \tau') \cdot (w - s_{i2}) + s_{i2} - C(0, s_{i2})$$

Participation in $t = 2$ requires $\kappa_{i2} \leq \bar{\kappa}(\tau')$, which implies $\kappa_{i1} \leq \bar{\kappa}(\tau')/\phi$. The participation decision depends on the size of the tax change. If the tax change is large enough that $\bar{\kappa}(\tau')/\phi > \bar{\kappa}(\tau)$, agents whose $\kappa_{i1} \in (\bar{\kappa}(\tau), \bar{\kappa}(\tau')/\phi]$ will participate. For an infinitesimal increase, $\bar{\kappa}(\tau')/\phi \to \bar{\kappa}(\tau)/\phi < \bar{\kappa}(\tau)$ since $\phi > 1$, so the extensive margin is locally zero.

**Participants ($s_{i1} = s^*(\tau) > 0$).** Existing participants reoptimize:

$$\max_{s_{i2} > 0} \quad (1 - \tau') \cdot (w - s_{i2}) + s_{i2} - C(s^*(\tau), s_{i2})$$

The F.O.C. is $\tau' = \Gamma'(s^*_{i2})$. This yields a new optimal level $s^*_{i2}(\tau') = s^*(\tau') > s^*(\tau)$. All existing participants increase their sheltering.

**Aggregate change in sheltered wealth.** Sheltered wealth increases via intensive margin expansion, with new participation occurring only if the tax hike sufficiently raises the participation threshold (captured by the max operator).

$$\Delta S^{Hike}_{agg} = G(\bar{\kappa}(\tau)) \cdot [s(\tau') - s(\tau)] + s^*(\tau') \cdot \max\left\{0, G(\bar{\kappa}(\tau')/\phi) - G(\bar{\kappa}(\tau))\right\}$$

#### 5.2.2 Case 2: Tax Cut ($0 < \tau' < \tau$)

**Non-participants ($s_{i1} = 0$).** The threshold for participating falls to $\bar{\kappa}(\tau') < \bar{\kappa}(\tau)$. Agents who abstained at $\tau$ will strictly abstain at $\tau'$. No new participation occurs.

**Participants ($s_{i1} = s^*(\tau) > 0$).** Existing participants reoptimize:

$$\max_{s_{i2} \geq 0} \quad (1 - \tau') \cdot (w - s_{i2}) + s_{i2} - C(s^*(\tau), s_{i2})$$

For $s_{i2} \leq s^*(\tau)$, the incremental cost term is zero and the objective function collapses to $\tau' s_{i2}$, which is strictly increasing in $s_{i2}$. Therefore, among choices that do not trigger new variable costs, the agent chooses $s_{i2}^* = s^*(\tau)$. For $s_{i2} > s^*(\tau)$, expansion is dominated because the agent must pay incremental costs, and the marginal payoff at the boundary is $\tau' - \Gamma'(s^*(\tau)) = \tau' - \tau < 0$. Thus, the optimal choice is the corner solution $s_{i2}^* = s^*(\tau)$.

**Aggregate sheltered wealth.** Since neither margin adjusts, the aggregate sheltered stock remains unchanged, i.e., $\Delta S_{agg}^{Cut} = 0$.

### 5.3 Asymmetrical Responses

We illustrate the asymmetry by comparing the marginal response of aggregate estates $E_{agg} = W - S_{agg}$ w.r.t. the net-of-tax rate.

**Tax increase.** Consider a marginal increase in the tax rate, which is a decrease in the net-of-tax rate. Only existing participants respond, as the extensive margin is locally zero for infinitesimal changes. The marginal response of aggregate estates is:

$$\frac{dE_{agg}}{d(1-\tau)^-} = -\frac{-dS_{agg}}{d(1-\tau)|_{\tau \to \tau^+}} = \int_0^{\bar{\kappa}(\tau)} \frac{ds_i}{d\tau^+} \, dG(\kappa_{i1}) = G(\bar{\kappa}(\tau)) \cdot \frac{1}{\Gamma''(s^*)}$$

**Tax cut.** Consider a marginal decrease in the tax rate, which is an increase in the net-of-tax rate. Neither margin adjusts: non-participants remain out, while existing participants maintain their shelter because reducing it recovers nothing. Hence $dE_{agg}/d(1-\tau)^+ = 0$.

**Asymmetry.** The difference between the two is:

$$\frac{dE_{agg}}{d(1-\tau)^-} - \frac{dE_{agg}}{d(1-\tau)^+} = \frac{G(\bar{\kappa}(\tau))}{\Gamma''(s^*)} > 0$$

Since $\Gamma'' > 0$ and $G(\bar{\kappa}(\tau)) > 0$, the difference is strictly positive. Thus, the elasticity identified from tax increases is larger than that from tax cuts.

## 5.4 Discussion

The illustrative model clarifies the interpretation of the two elasticities estimated in the data. When taxes fall, previously established avoidance structures remain in place because their setup costs are sunk, attenuating responses even though the frictionless optimum would involve less sheltering. Conversely, when taxes rise, existing participants intensify their use of existing structures. For this reason, the tax-increase elasticity more closely reflects the long-run behavioral response absent sunk costs.

We back out the implied sunk costs by parameterizing the model with the tax-increase elasticity and assuming a quadratic cost function $\Gamma(s) \propto s^2$. At a 10% pre-reform tax rate and an estimated tax-increase elasticity of 2.76, the model implies that participants shelter approximately 23.5% of total wealth (see derivation in Appendix Section D.1), a level consistent with findings in Leenders et al. (2023).[15] This corresponds to a fixed cost for the marginal participant of $\bar{\kappa} \approx 1.5\%$ of the taxable estate. For the average estate in our sample of 244 million TWD ($\approx$ 7.6 million USD; Appendix Table B.3), this implies an entry cost of 3.7 million TWD ($\approx$ 116,000 USD). This magnitude is economically plausible, capturing not only explicit fees for establishing sophisticated structures (e.g., trusts, offshore entities) but also opportunity costs of high-net-worth individuals' time coordinating with tax planners and wealth managers.

The simple framework captures the qualitative asymmetry between tax increases and cuts but predicts a counterfactual zero response to tax cuts. Empirically, we observe immediate and large responses to tax increases but small, nonzero, and gradual adjustments to tax cuts. An extension to an overlapping-generations setting would rationalize such dynamics. A tax increase triggers an immediate intensive-margin expansion among those who already maintain avoidance structures. Tax cuts, however, generate a nonzero but gradual response as older cohorts locked into avoidance under previously higher rates are progressively replaced by new entrants facing lower incentives to incur setup costs.

These results have important policy implications. Interpreting our tax-increase elasticity of 2.76 as the long-run response, we apply Piketty and Saez (2013)'s sufficient statistics formula for revenue-maximizing rate $\tau^{revmax} = 1/(1 + a \cdot \varepsilon)$, where $a$ is the Pareto distribution parameter that measures the thinness of the taxable estate distribution. We

[15] Leenders et al. (2023) show in Panel b of Figure 3 that the amnesty participants sheltered around 30% of their total wealth in the Netherlands, Colombia (Londoño-Vélez and Ávila Mahecha, 2024), and Scandinavia (Alstadsæter et al., 2019).

estimate $a = 1.34$ from the pre-reform treated sample, similar to $a \approx 1.4$ in the U.S. (Saez and Zucman, 2016).[16] This yields a revenue-maximizing top rate of 21%, which is close to the average top rate of 23% applied to bequests to children in OECD countries with bequest taxes (OECD, 2021).[17] This contrasts with prior elasticities of around 0.1, often estimated from local variations or tax cuts, which would imply implausibly high revenue-maximizing rates near 90%. Although Taiwan's institutional setting is not necessarily generalizable, the avoidance technologies available to the wealthy are not unique. For instance, Hines (2023) shows that trusts are central to facilitating intergenerational transfers in the U.S., while offshore accounts have been documented in the U.S. (Johannesen et al., 2020; Guyton et al., 2021), the Netherlands (Leenders et al., 2023), and Scandinavia (Alstadsæter et al., 2018).

# 6 Conclusion

This study leverages two large estate tax reforms in Taiwan to analyze behavioral responses to estate taxation. We find that responses to both reforms are rapid and persistent but asymmetric, with much stronger reactions to the tax increase than to the cut. The asymmetry is concentrated in liquid and easily adjusted components such as financial assets, deposit savings, charitable contributions and allowances. Using three pieces of evidence on how decedents and heirs adjust, we show that these responses mainly reflect wealth-shifting avoidance rather than real wealth changes. First, divergences between reported values at death and values reported a year earlier arise in liquid items but not in third-party-reported assets. Second, closely held firms strategically inflated liabilities to reduce book values as owners approached death. Third, heirs' labor supply did not adjust despite sizable inheritance shocks induced by the reforms.

We interpret these findings through a sunk-cost avoidance framework, implying that individuals retain existing avoidance strategies when taxes fall but expand them when rates rise. Our findings have important policy implications. First, the substantial tax avoidance in response to estate tax changes highlights the need for stronger enforcement measures, such as expanding third-party reporting and international cooperation to detect offshore assets. Second, the mechanism suggests that the elasticity identified from a tax

[16] Table 1 in Saez and Zucman (2016) implies that $a$ ranges between 1.35 and 1.43 across top wealth groups.

[17] Based on top statutory rates for bequests to children in Panel A of Figure 3.11 in OECD (2021). The average covers all OECD countries with an active bequest tax, including those applying a zero rate to parent-to-children bequests. Restricting to countries with nonzero rates yields an average of 30%.

increase better approximates the structural long-run response, so the direction of the reform used for identification must be considered when informing policy. Finally, the role of sunk costs in avoidance likely extends to other settings where tax planning involves fixed costs, such as corporate income and wealth taxes.

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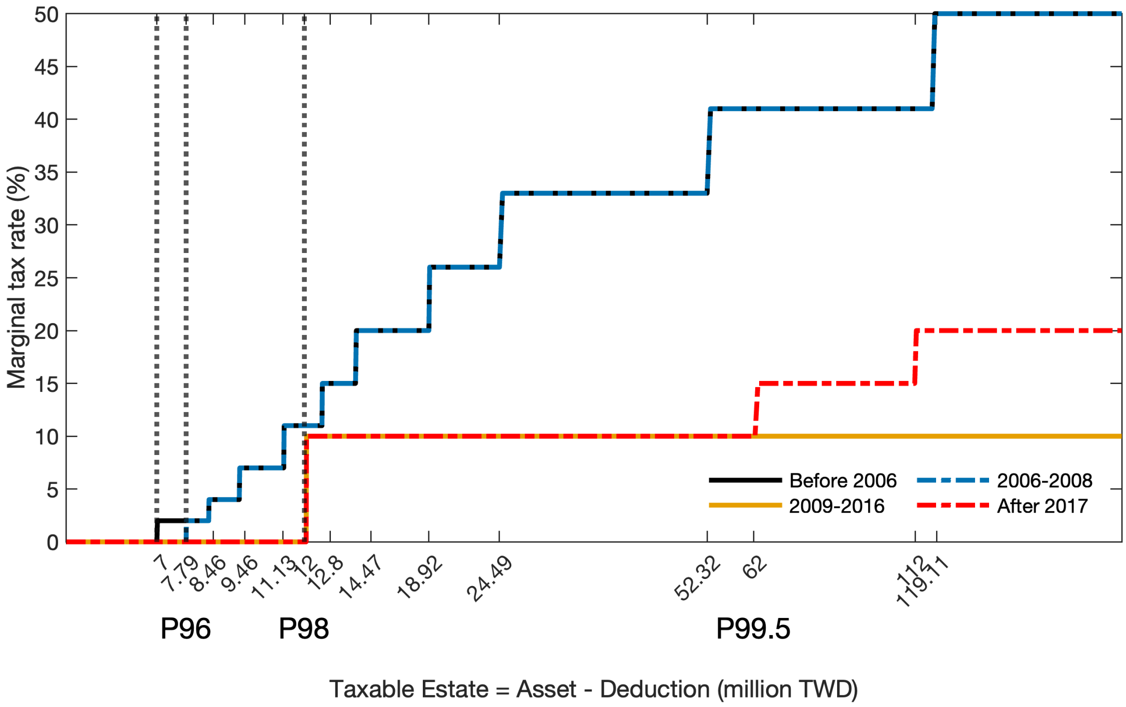
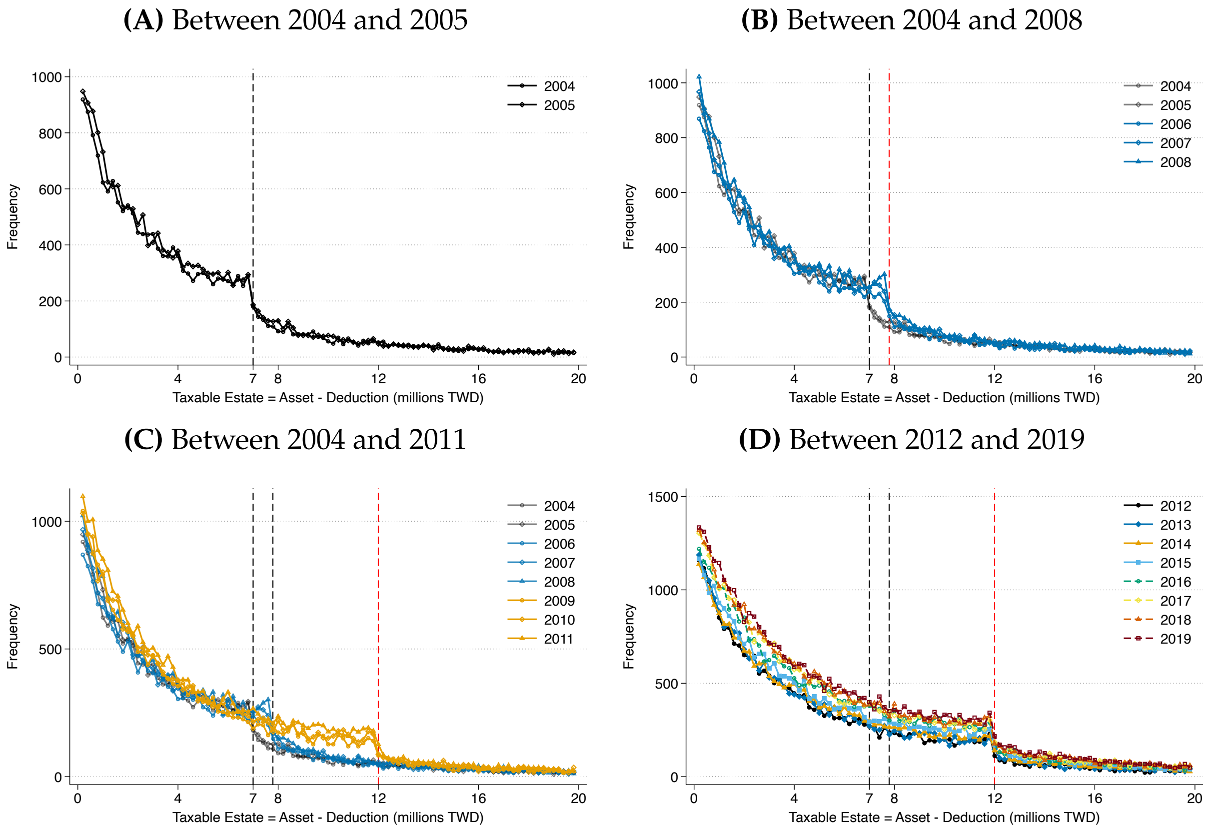
# Figures

**Figure 1:** Estate Tax Schedule Over Time




*Notes:* This figure presents the estate tax schedules over time. The black line represents the schedule before 2006, with an exemption threshold of 7 million TWD (approximately 219K USD) and marginal tax rates (MTRs) ranging between 2% and 50%. The blue line plots the schedule between 2006 and 2008, where the exemption threshold increased to 7.79 million TWD, with MTRs remaining unchanged. The orange line draws the schedule between 2009 and 2016, where the exemption threshold is 12 million TWD and a flat MTR of 10%. The red dashed line is the schedule after 2017, where the exemption threshold stays the same but two new MTR brackets are introduced for those above 62 million TWD. The corresponding percentiles for 7.79 million, 12 million, and 62 million TWD are the 96th, 98th, and 99.5th percentiles, respectively. This figure is discussed in Section 2.1 and Section 3.3.

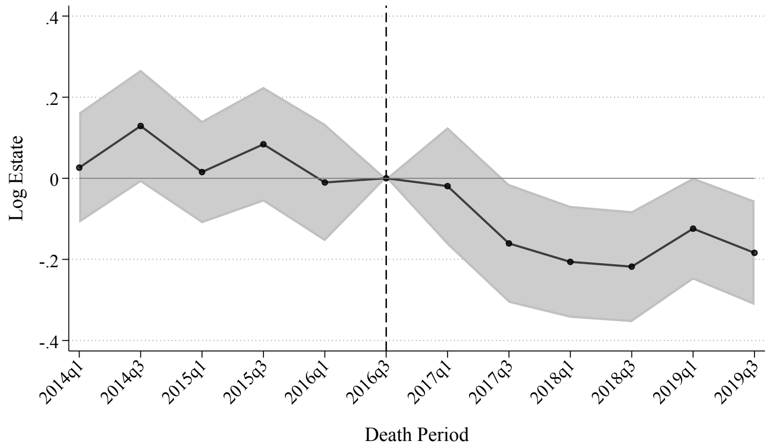
**Figure 2:** Density of Taxable Estates Around Thresholds Over Time



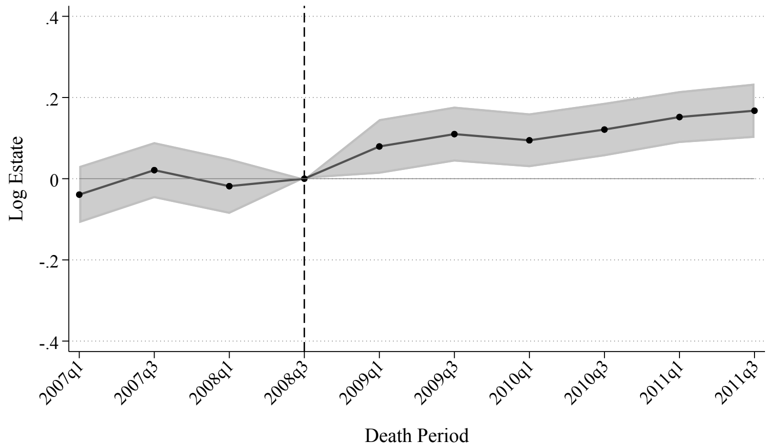

*Notes:* This figure presents the distribution of individuals around the exemption thresholds over time. Individuals are binned every 200,000 TWD (approximately 6,250 USD) interval. Panel A shows the distribution between 2004 and 2005 and the black dashed line corresponds to the 7 million TWD threshold. Panel B shows the distribution between 2004 and 2008. The black dashed line is the old 7 million TWD threshold and the red one is the new 7.79 million TWD threshold. Panel C shows the distribution between 2004 and 2011. The black dashed lines on the left are the old 7 and 7.79 million TWD thresholds and the red dashed line is the new 12 million TWD threshold. Panel D presents the distributions in later years, showing that the excess mass at the 2009-induced threshold remains even ten years later. This figure is discussed in Section 2.3.

**Figure 3:** The Effects of Estate Tax Reforms on Taxable Estates

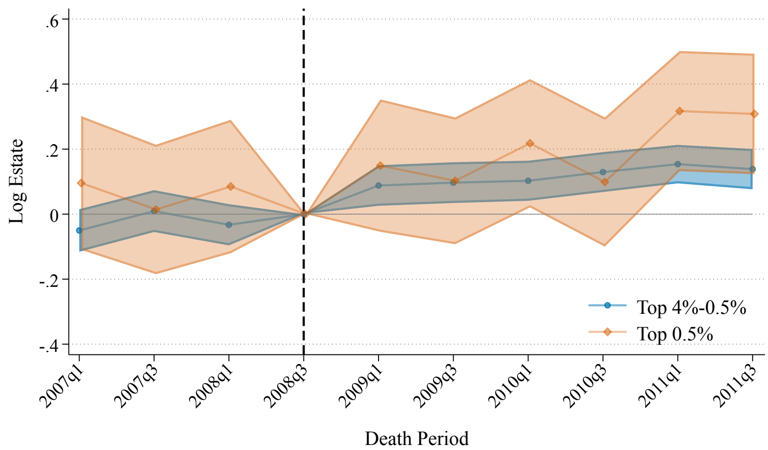
**(A)** 2017 Tax Increase: Top 0.5%




**(B)** 2009 Tax Cut: Top 4%

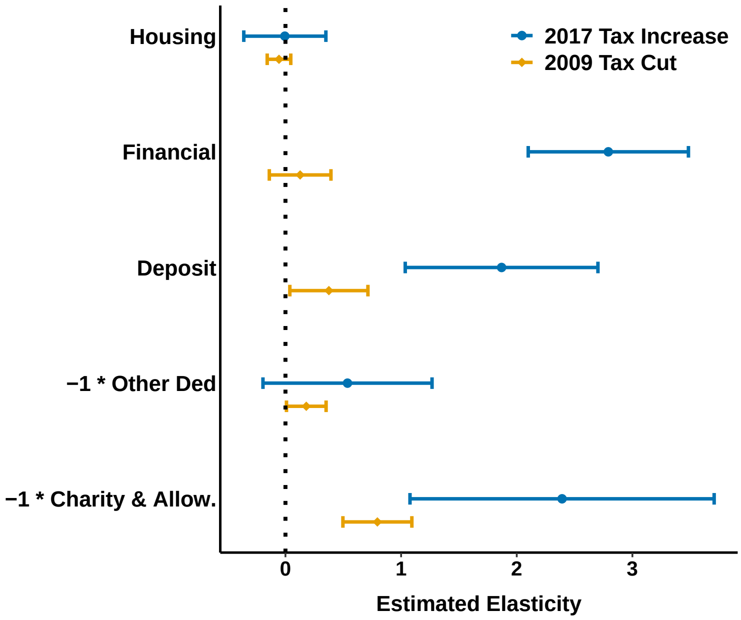



**(C)** 2009 Tax Cut: Top 4%-0.5% and Top 0.5%




*Notes:* The figure plots the estimated coefficients of the interaction term and their 95% confidence intervals from Equation (2). Panel A shows the result of the 2017 tax increase where the treated are those predicted to be the top 0.5% and the control are those predicted to be between the 90th and 96th percentiles. Panel B and C refer to the results of the 2009 tax cut, which affected the top 4%. The treated in Panel B are those predicted to be above the top 4% and the control are those predicted to be between the 90th and 96th percentiles. Panel C splits the top 4% treated group into top 4%-0.5% and top 0.5% where the control remains the same. This figure is discussed in Section 3.3.

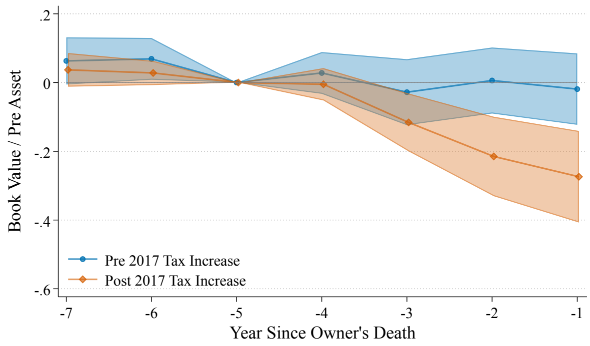
**Figure 4:** Elasticity Estimates of Each Item



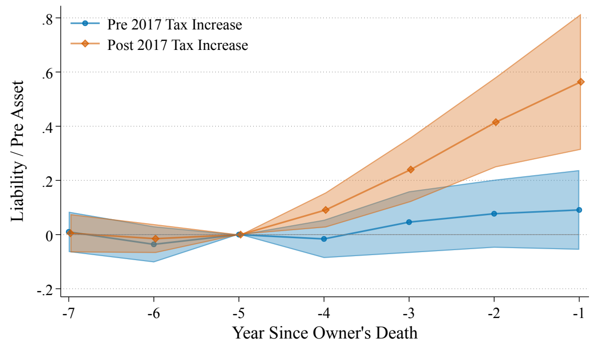

*Notes:* This figure reports item-level elasticities with respect to the net-of-tax rate for components of the estate base: housing, financial assets, deposit savings, other deductions, and charitable contributions and allowances. For ease of comparison with asset categories, we plot deduction elasticities with signs flipped, i.e., we multiply the estimates for other deductions and charitable contributions and allowances by −1. Estimates for the 2017 tax increase are shown in blue and for the 2009 tax cut in orange. First-stage and reduced-form estimates from Equations (3) and (4) are reported in Appendix Tables C.1 and C.2. The treated group comprises individuals predicted to be in the top 0.5% of the distribution; controls are those predicted to be in the P90-P96. This figure is discussed in Section 4.1.

**Figure 5:** The Effect of 2017 Tax Increase on Closely Held Firms

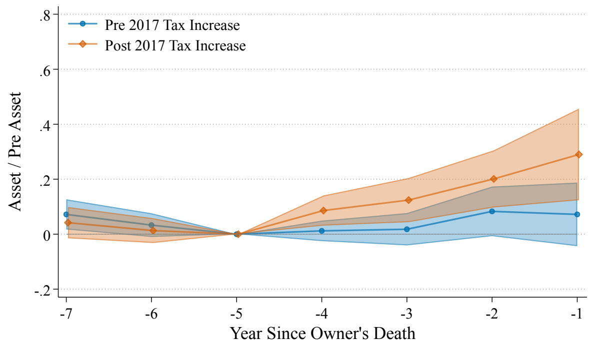
**(A)** Book Value / Pre-Asset



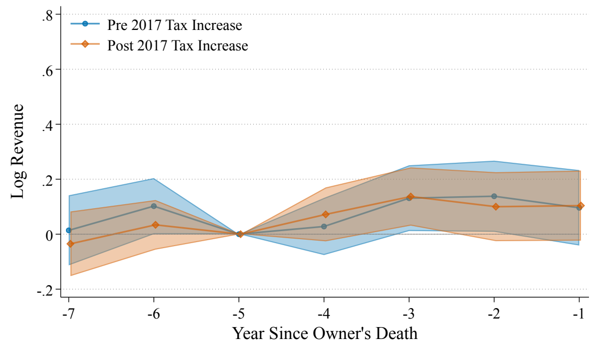

**(B)** Liability / Pre-Asset




**(C)** Asset / Pre-Asset




**(D)** Log Sales Revenues

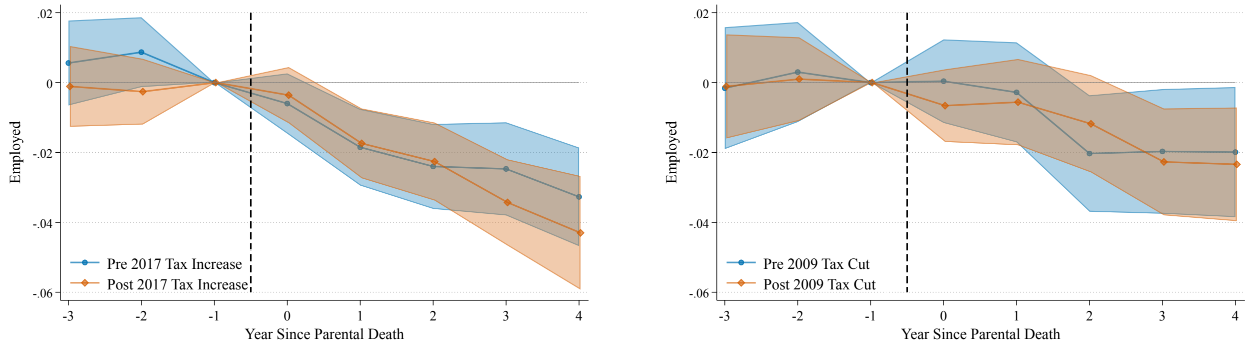



*Notes:* This figure reports event-study estimates with 95% confidence intervals from Equation (5). Blue markers represent owners who died before the 2017 tax increase, and orange markers represent those who died after. Panel (A) plots book values, defined as assets minus liabilities. Panels (B) and (C) show liabilities and assets separately. Values in Panels (A) to (C) are scaled by the average firm assets measured five to seven years before death. Panel (D) reports the logarithm of sales revenues. The treated group consists of firms whose owners were subject to the 2017 tax increase (predicted above the top 0.5%) and who at any point in the seven years before death held more than one-third of the firm ownership. The treated and control groups include 1,235 and 1,600 unique firms, respectively. This figure is discussed in Section 4.2.2.

**Figure 6:** The Effects of Reforms on Heirs' Outcomes

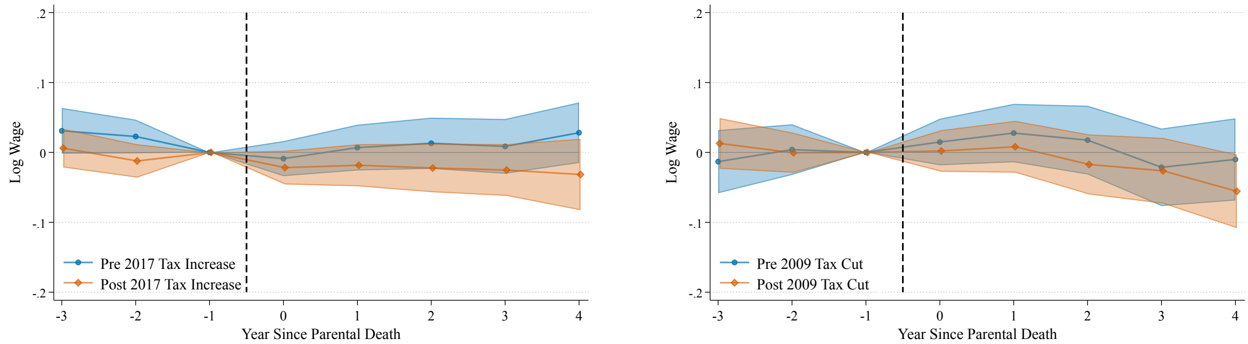
I. Extensive margin: Probability of being employed

(A) 2017 Tax Increase (B) 2009 Tax Cut




II. Intensive margin: Log labor income

(C) 2017 Tax Increase (D) 2009 Tax Cut




*Notes:* This figure plots event-study estimates with 95% confidence intervals from a specification analogous to Equation (5), where the unit of analysis is heirs and the base period is $k = -1$. Standard errors are clustered at the heir level. The treated group includes heirs whose parents are predicted to be in the top 0.5% of the estate distribution, and the control group includes heirs whose parents are between the 90th and 96th percentiles. Blue markers represent heirs whose parents died before the reform, and orange markers represent those whose parents died after. The 2017 samples contain 7,969 treated heirs and 80,031 controls; the 2009 samples contain 5,047 treated heirs and 65,772 controls. Panels A and B show the extensive margin, defined as having positive labor income. Panels C and D show the intensive margin, measured as the logarithm of labor income. This figure is discussed in Section 4.2.3.

# Tables

**Table 1:** Diff-in-diff Estimates and Implied Elasticities

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 2017 Tax Increase | 2009 Tax Cut | | |
| | All | All | Splitting into subgroups | |
| | Top 0.5% | Top 4% | Top 4%-0.5% | Top 0.5% |
| **Panel A: First stage** | | | | |
| $\widehat{T}_i$ | -0.074 | 0.099 | 0.072 | 0.331 |
| | (0.001) | (0.001) | (0.001) | (0.005) |
| Observations | 29,576 | 25,569 | 24,510 | 16,634 |
| **Panel B: Reduced form** | | | | |
| $\widehat{T}_i \times Post_i$ | -0.204 | 0.131 | 0.138 | 0.154 |
| | (0.029) | (0.016) | (0.015) | (0.044) |
| Observations | 63,627 | 71,234 | 68,397 | 46,410 |
| **Panel C: Implied Elasticity** | | | | |
| $\varepsilon$ | 2.757 | 1.323 | 1.917 | 0.465 |
| | (0.394) | (0.162) | (0.210) | (0.133) |

*Notes:* This table shows the diff-in-diff estimates and implied elasticities of taxable estates with respect to the net-of-tax rate estimated from Equations (3) and (4), as described in Section 3.2. Panel A presents the first-stage effect on the change in log net-of-tax rate using pre-reform samples. Panel B reports the reduced-form effect on log taxable estates using all samples. Panel C shows the implied elasticities calculated using the Delta method as reduced-form scaled by first-stage. Column (1) shows the result of the 2017 tax increase. Columns (2)-(4) are the results of the 2009 tax cut where (2) uses the full treated group top 4%. Column (3)-(4) split the top 4% into top 4%-0.5% and top 0.5%. Robust standard errors are in parentheses. This table is discussed in Section 3.3.

**Table 2:** Elasticity of Estate/Inheritance w.r.t Net-of-tax Rate in Past Literature

| Paper | Variation | Context | Outcome variable | Elasticity w.r.t. $1 - \tau$ |
|---|---|---|---|---|
| **Panel A: OLS** | | | | |
| Kopczuk and Slemrod (2001) | Time-series | U.S. | Estate | Noisy, [-0.11, 0.09] |
| Joulfaian (2006) | Time-series | U.S. | Estate | 0.14 (s.e. 0.05) |
| **Panel B: Diff-in-diff** | | | | |
| Mas-Montserrat (2019) | Decrease (top 5%-1%) | Spain | Inheritance | 1.88 (s.e. 0.23) |
| Our study$^{\dagger}$ | Decrease (top 4%-0.5%) | Taiwan | Estate | 1.92 (s.e. 0.21) |
| Our study$^{\dagger}$ | Decrease (top 0.5%) | Taiwan | Estate | 0.47 (s.e. 0.13) |
| Our study$^{\dagger}$ | Increase (top 0.5%) | Taiwan | Estate | 2.76 (s.e. 0.39) |
| **Panel C: Bunching (exemption kink)** | | | | |
| Escobar et al. (2019) | Bunching (0% to 10%) | Sweden | Inheritance | 1.53 (s.e. 0.10) |
| Our study$^{\dagger}$ | Bunching (0% to 2%) | Taiwan | Estate | 2.56 (s.e. 0.66) |
| Our study$^{\dagger}$ | Bunching (0% to 10%) | Taiwan | Estate | 0.45 (s.e. 0.12) |
| **Panel D: Bunching (within-positive kink)** | | | | |
| Glogowsky (2021) | Bunching (7% to 50%) | Germany | Inheritance | 0.03 (s.e. 0.01) |
| Escobar et al. (2019) | Bunching (10% to 20%) | Sweden | Inheritance | 0.34 (s.e. 0.10) |
| Escobar et al. (2019) | Bunching (20% to 30%) | Sweden | Inheritance | 0.02 (s.e. 0.10) |
| Our study$^{\dagger}$ | Bunching (11% to 15%) | Taiwan | Estate | 0.10 (s.e. 0.19) |
| Our study$^{\dagger}$ | Bunching (15% to 20%) | Taiwan | Estate | 0.27 (s.e. 0.19) |
| Our study$^{\dagger}$ | Bunching (20% to 25%) | Taiwan | Estate | 0.28 (s.e. 0.19) |
| Our study$^{\dagger}$ | Bunching (25% to 33%) | Taiwan | Estate | 0.04 (s.e. 0.26) |
| **Panel E: RDD** | | | | |
| Escobar (2017) | Decrease | Sweden | Inheritance | 0.76 (s.e. 0.31) |

*Notes:* This table compares our elasticity estimates (marked with $^{\dagger}$) with those in the existing literature, as described in Section 3.3. We include studies with the outcome variable being either taxable estates or inheritances in the literature. Panel A reports older time-series OLS evidence from the U.S. Panel B through Panel E present more recent micro-level approaches, with the only published paper being Glogowsky (2021). Kopczuk and Slemrod (2001)'s number is from Table 3 Column III. Joulfaian (2006) defined taxable estates as estate tax revenues divided by the average tax rate and reported an estimated elasticity w.r.t. tax rate. We convert this by multiplying the number with $-(1-\tau)/\tau$ and we use 0.4 for $\tau$. Mas-Montserrat (2019)'s estimate is taken from Column 4 in Table 4, using the specification with marginal tax rates. Escobar et al. (2019)'s exemption kink estimate is from Figure C1, and the within-positive kink estimates are from Figure C2. Glogowsky (2021)'s number is from Column 5 in Table 1, using the pre-2009 period. Escobar (2017)'s reduced-form coefficient is reported in Figure 1(a). As the net-of-tax rate change is not reported, we compute the implied elasticity by assuming that the average net-of-tax rate before the repeal was 0.8, based on the average estate size in Table 1. Standard errors are computed using the delta method when the necessary inputs are available. In cases where only reduced-form coefficients are reported and the necessary information on the first stage is not provided, we approximate the standard errors of the implied elasticity by assuming that the reported t-statistic remains constant when rescaling. While this approach is an approximation rather than an exact calculation, it is the best feasible method given the information reported. This table is discussed in Section 3.3.

**Table 3:** Elasticity of Each Item and Decomposition, Top 0.5%

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | 2017 Tax Increase | | | 2009 Tax Cut | | |
| | Elasticity | Weight (normalized) | Contribution | Elasticity | Weight (normalized) | Contribution |
| **Panel A: Assets** | | | | | | |
| Housing | -0.005 | 44.5% | -0.008 | -0.056 | 47.6% | -0.117 |
| | (0.181) | | | (0.052) | | |
| Financial assets | 2.792 | 14.7% | 1.367 | 0.127 | 10.1% | 0.055 |
| | (0.353) | | | (0.136) | | |
| Deposit savings | 1.869 | 5.8% | 0.361 | 0.376 | 3.8% | 0.062 |
| | (0.425) | | | (0.172) | | |
| *Assets subtotal* | | | 1.720 | | | 0.001 |
| **Panel B: Deductions** | | | | | | |
| Other deductions | -0.537 | 29.4% | -0.524 | -0.180 | 31.6% | -0.250 |
| | (0.373) | | | (0.087) | | |
| Charity & exemptions | -2.392 | 5.6% | -0.448 | -0.795 | 6.9% | -0.240 |
| | (0.671) | | | (0.152) | | |
| *Deductions subtotal* | | | -0.972 | | | -0.490 |
| **Panel C: Estates (Assets – Deductions)** | | | 2.692 | | | 0.491 |
| *% contribution from assets* | | | 63.9% | | | 0.3% |
| *% contribution from deductions* | | | 36.1% | | | 99.7% |

*Notes:* This table presents the decomposition analysis in Section 4.1 for the top 0.5%. Columns (1) and (4) report the item-level elasticity estimates. Standard errors are in parentheses. Columns (2) and (5) show normalized weights, defined as each item's share of estates in the pre-reform treated group, rescaled so that it sums to 100% across all asset and deduction items: $w^{\text{norm}}_{\text{item}/E} = \frac{w_{\text{item/E}}}{w_{\text{A/E}}+w_{\text{D/E}}}$ where $w_{\text{item}/E}$ is the item's share of the estate and the denominator is the sum of the weights of assets and deductions. Columns (3) and (6) report each item's contribution to the overall elasticity, calculated as the product of elasticity and the item's unnormalized estate share. Panel A covers asset components, including housing, financial assets, and deposit savings; assets subtotal is the sum of their contributions. Panel B covers deduction components, including other deductibles and charity and exemptions; deductions subtotal is the sum of their contributions. Panel C aggregates to estates, defined as assets subtotal minus deduction subtotal. % contribution from assets and % contribution from deductions report the share of total estate elasticity attributable to each category, calculated as the ratio of the subtotal contribution to the overall estate elasticity. This table is discussed in Section 4.1.

**Table 4:** Comparing Reported Values at Death with Pre-Death Asset Holdings

| | (1) $\frac{\Delta \text{Housing}}{\text{Wealth}_{t-3}}$ | (2) $\frac{\Delta \text{Financial}}{\text{Wealth}_{t-3}}$ | (3) $\frac{\Delta \text{Deposit}}{\text{Wealth}_{t-3}}$ |
|---|---|---|---|
| **Panel A: 2017 Tax Increase** | | | |
| $\widehat{T_i} \times Post_i$ | 0.003 | -0.108 | -0.015 |
| | (0.042) | (0.013) | (0.007) |
| Baseline mean (treated) | 0.397 | 0.277 | 0.060 |
| Observations | 63,459 | 63,459 | 63,459 |
| **Panel B: 2009 Tax Cut** | | | |
| $\widehat{T_i} \times Post_i$ | 0.086 | 0.007 | 0.102 |
| | (0.065) | (0.012) | (0.008) |
| Baseline mean (treated) | 0.405 | 0.101 | 0.035 |
| Observations | 46,272 | 46,272 | 46,272 |

*Notes:* This table presents the estimated coefficient on the interaction term in Equation (4), where the outcome variables are replaced by the difference between the reported value of the item at death and the value held one year before death, scaled by decedent's total wealth three years before death. The treated group is those who are predicted to be in the top 0.5%, while the control group includes those who are predicted to be between the 90th and 96th percentiles. Column (1) represents the difference between the reported housing values and those from one year prior. Column (2) represents the difference between reported financial assets at death (stocks and other financial instruments) and observed stockholdings one year prior, as other financial instruments are not available in the lifetime data. Column (3) represents the difference between the reported deposit savings and the imputed deposit savings from one year prior. Baseline mean is the pre-reform mean of the dependent variable for the treated group. Robust standard errors in parentheses. This table is discussed in Section 4.2.1.

**Table 5:** The Effect of Estate Tax Reforms on Heirs' Inheritance

| | (1)<br>2017 Tax Increase | (2)<br>2009 Tax Cut |
|---|---|---|
| $\widehat{T}_{j(i)} \times Post_{j(i)}$ | -44.143 | 8.698 |
| | (9.535) | (2.415) |
| Baseline mean (treated) | 53.94 | 13.17 |
| Observations | 88,000 | 70,819 |

*Notes:* This table presents the estimated coefficient on the interaction term in Equation (6). The dependent variable is average inheritance per heir, measured in 2016 million TWD and defined as the reported after-tax estate minus gifts within two years, divided by the number of heirs. The treated group consists of heirs whose parents are predicted to be in the top 0.5%, while the control group includes those whose parents are predicted to be between the 90th and 96th percentiles. Column (1) reports estimates for the 2017 tax increase, and Column (2) for the 2009 tax cut. Baseline mean is the pre-reform mean of the dependent variable for the treated group. Standard errors are in parentheses. This table is discussed in Section 4.2.3.

# Online Appendix

## A Appendix to Section 2: Institutional Context & Data

### A.1 Tax Assessment

**Table A.1:** Details of the Breakdown of Estates

| Item | Description |
|---|---|
| **Panel A: Assets** | |
| Housing | Land and houses are third-party-reported where the government could verify from the property tax data. Housing values are determined by government-announced prices, which are typically updated every two years and lower than the market prices. |
| Financial Assets | Include publicly listed and privately held stock, and other financial instruments. Stock ownership is third-party-reported which the government can verify from the dividend income tax records. Publicly listed stock is assessed at the market price on the death date. Privately held stock is valued at the net assets of the company on the death date. Other financial assets are self-reported items such as trusts, insurance, farm output, credits, cash, and jewelry. |
| Deposit savings | Domestic bank savings are self-reported, although the government has a third-party-reported proxy interest income from the individual income tax records. |
| **Panel B: Deductions** | |
| Charity and Allowances | Charitable contributions are donations to nonprofit organizations. Allowances include spousal claims and estate taxes paid within the last five years. Spousal claims are based on Article 1030-1 of Taiwanese Civil Law: "Upon dissolution of the statutory marital property regime, the remainder of the property acquired by the husband or wife during marriage, after deducting the debts incurred during the continuance of the marriage relationship, if any, shall be equally distributed between the husband and the wife." If the surviving spouse owns less wealth than the decedent, they may claim half of the decedent's wealth before it is counted as part of the estate. |

*Continued on next page*

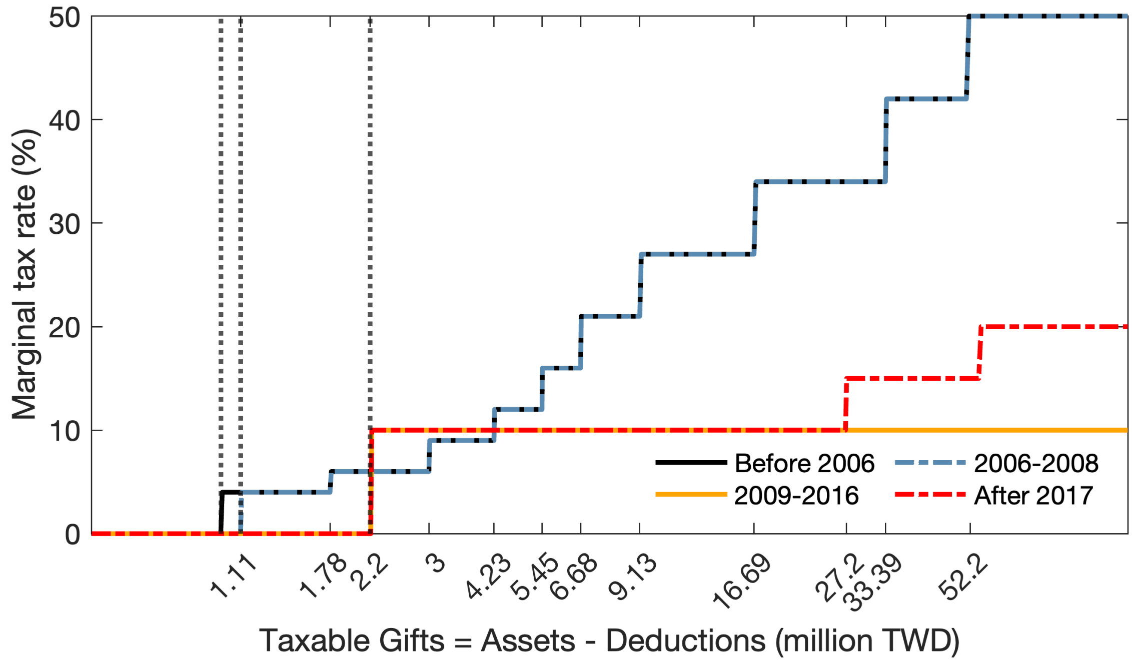
**Table A.1:** (Continued)

| Item | Description |
|---|---|
| Other Deductions | These include debts, farm, funeral, parent, dependent, spousal, and disability deductions. Debt deductions require submission of relevant loan statements. Farm deductions represent the value of agriculturally used properties. To claim a farm deduction, a certificate from the government must be provided, verifying that the land is used for agricultural purposes. Dependent deductions were 450,000 TWD per dependent before 2009, increasing to 500,000 TWD after 2014. Parent deductions were 1 million TWD per parent before 2009 and 1.23 million TWD after 2014. Spousal deductions were 4.45 million TWD before 2009, increasing to 4.93 million TWD after 2014. Disability deductions were 5.57 million TWD before 2009, increasing to 6.18 million TWD after 2014. Funeral deductions were 1.11 million TWD before 2009 and 1.23 million TWD after 2009. |

*Notes:* This table details the items in the estates. Panel A lists asset components and Panel B lists deductions. This table is discussed in Section 2.1.

## A.2 Gift Tax Schedule

**Figure A.1:** Gift Tax Schedule Over Time




*Notes:* This figure presents the gift tax schedules over time. The black line represents the schedule before 2006, with an exemption threshold of 1 million TWD and marginal tax rates (MTRs) ranging between 4% and 50%. The blue line plots the schedule between 2006 and 2008, where the exemption threshold increased to 1.11 million TWD, with MTRs remaining unchanged. The orange line draws the schedule between 2009 and 2016, where the exemption threshold is 2.2 million TWD and a flat MTR of 10%. The red dashed line is the schedule after 2017, where the exemption threshold stays the same but two new MTR brackets are introduced for those above 27.2 million TWD. This figure is discussed in Section 2.1.

## A.3 Graphical Bunching Evidence

We augment our analysis using the bunching method following Saez (2010). This approach allows us to focus on taxpayers near specific kinks in the tax schedule who have similar levels of estates. By comparing behaviors around kinks that are similar in estates but differ in the type of tax change (exemption kinks versus within-positive kinks), we can better isolate the impact of tax change type on taxpayer behavior. To make our estimation comparable with the existing studies, we recover the counterfactual distribution near the kink points by fitting a seventh-order polynomial and bootstrap the standard errors à la Chetty et al. (2011).

**Exemption kinks.** First, we examine the exemption kinks and present the results in Figure A.2. Panel A illustrates the exemption kink at 12 million TWD in the flat-rate schedule during 2009–2019, associated with a tax rate change from 0% to 10%. Following the existing literature, we construct counterfactual distributions using a seventh-order polynomial, represented by the orange line. We report the excess mass estimate as $b$ = 2.68 (s.e. 0.74) and the elasticity estimate as $e$ = 0.45 (s.e. 0.12).

In Panel B, we analyze the exemption kink at 7.79 million TWD during the progressive schedule in the 2006–2008 period, associated with a smaller tax rate change from 0% to 2%. The excess mass and elasticity estimates are $b$ = 1.99 (s.e. 0.52) and $e$ = 2.56 (s.e. 0.66), respectively.

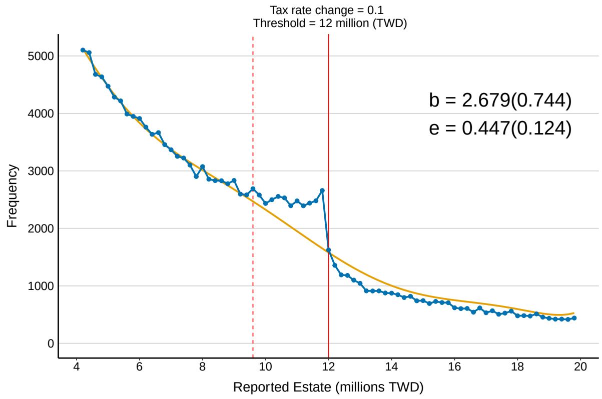
These findings suggest that individuals have a strong incentive to bunch at exemption thresholds. However, the different elasticity estimates imply that individuals do not respond proportionally more to the magnitude of the tax rate change at exemption thresholds, suggesting that there are factors beyond the magnitude of the tax rate influencing taxpayer behavior.

**Within-positive kinks.** Next, we examine the within-positive-kinks in the progressive schedule during 2006–2008. Panels C to F present the density distributions around the fourth through seventh kinks at 12.80, 14.47, 18.92, and 24.49 million TWD, respectively.[18] In all cases, we cannot reject the null hypothesis of no bunching. In other words, the estimated elasticities at these within-positive-kinks are statistically insignificant.

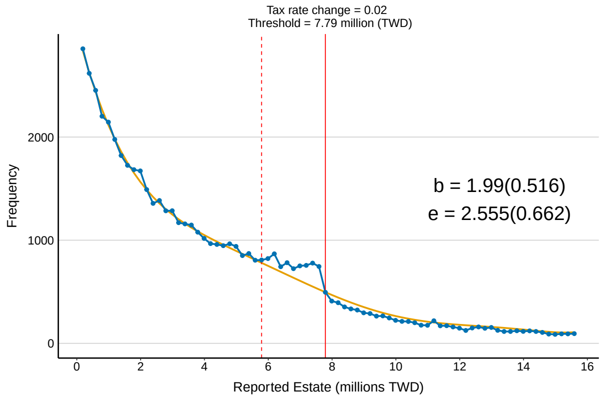
[18]We exclude the within-positive-kinks at 8.46 million and 9.46 million TWD because they are too close to the exemption thresholds; as shown in Panel B, there is no bunching around those kinks. For within-positive-kinks above 24.49 million TWD, there are too few observations for credible estimation.

**Figure A.2:** Bunching Estimates Using Exemption Kinks vs. Within-positive Kinks

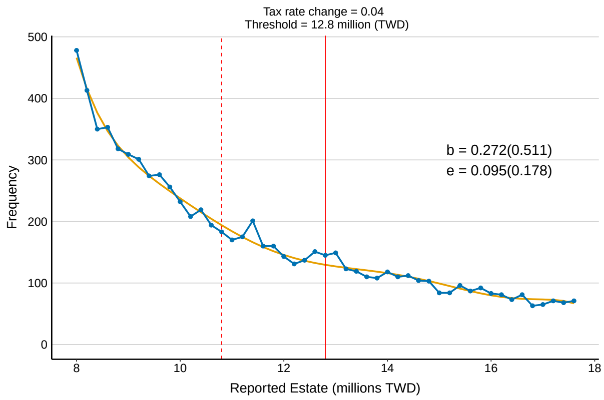
**(A)** Exemption: 12M TWD, 2009-2019



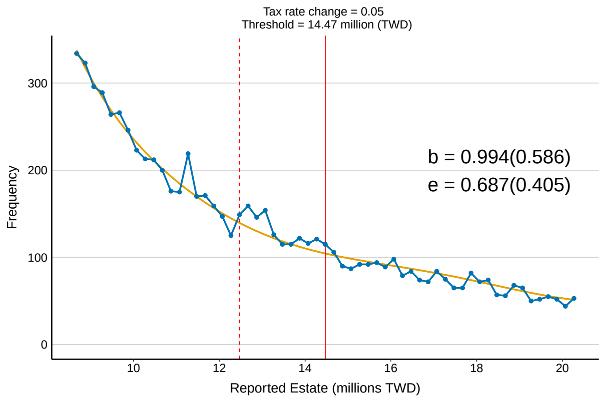

**(B)** Exemption: 7.79M TWD, 2006-2008



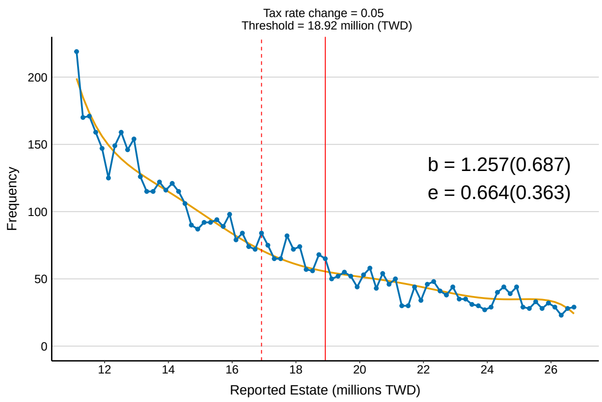

**(C)** Within-positive: 12.8M TWD, 2006-2008



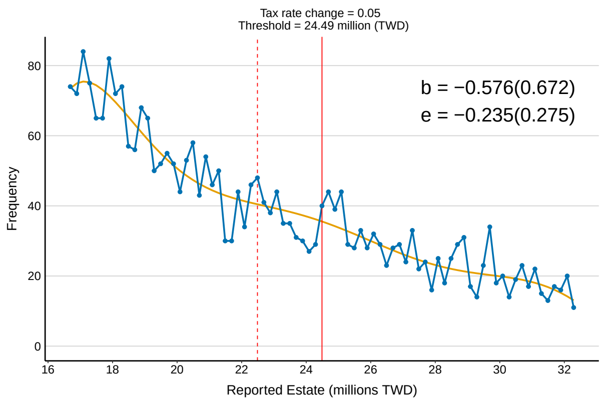

**(D)** Within-positive: 14.47M TWD, 2006-2008




**(E)** Within-positive: 18.92M TWD, 2006-2008




**(F)** Within-positive: 24.49M TWD, 2006-2008




*Notes:* This figure groups individuals into bins of 200,000 TWD of taxable estates and plots the frequency around different kinks, categorized by whether the kink is around an exemption threshold (exemption kink) or where tax rates adjust within the positive region (within-positive kink). Panel A focuses on the years between 2009 and 2019, when the exemption threshold was 12 million TWD. Panels B through F focus on the progressive schedule between 2006 and 2008, when the exemption threshold was 7.79 million TWD. Panel B shows the density around the exemption threshold, while Panels C through F present the densities around the within-positive kinks at 12.8, 14.47, 18.92, and 24.49 million TWD, respectively. This figure is discussed in Section 2.3.

# B Appendix to Section 3: Empirical Analysis

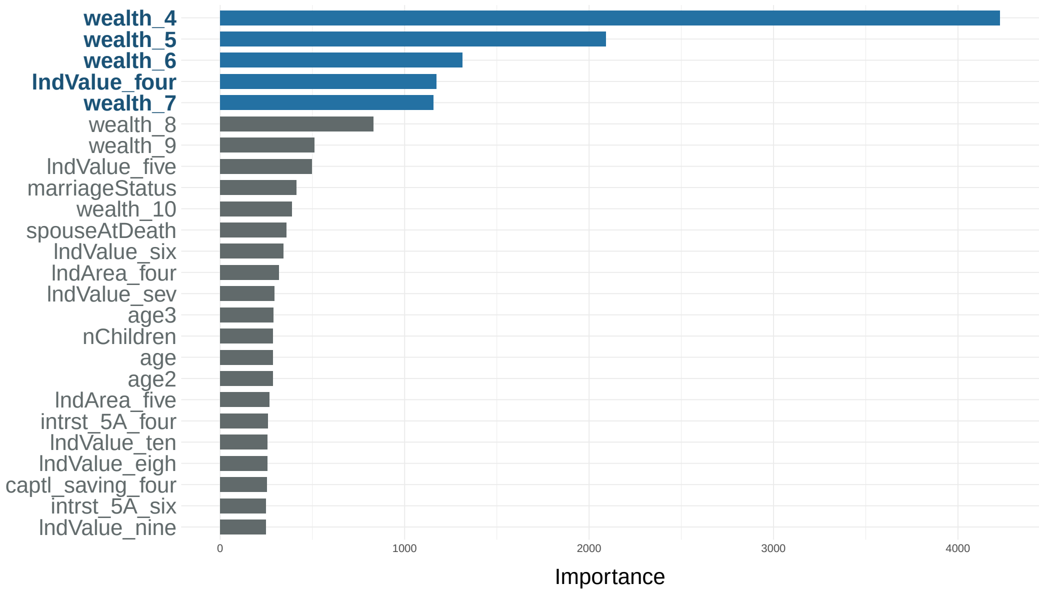
## B.1 2017 Prediction Algorithm Details

**Procedures.** To construct decedents' counterfactual estates in the absence of the reform, we use a random-forest algorithm to predict a decedent's taxable estates at death based on her pre-death wealth, income, and demographic information. The procedure is as follows:

- Within those who died before the reform (2014-2016), select 70% as a training sample and the remaining 30% as a hold-out sample
- Train a random forest model where the outcome variable is a discrete group variable indicating the classification of one's percentile of estate: i) below P90, ii) P90-P96, iii) P96-P99.5, iv) above P99.5
- Predictors are the values and quantities of land, houses, publicly-listed stock, privately-held stock, capitalized interest income, all measured four to ten years before death, and demographics of gender, age, number of kids, marital status, parents alive, spouse alive etc
- In the above step, we tune parameters for the number of trees, number of variables possibly split at each node, minimal node size
- Repeat the above step for each set of parameters and calculate the corresponding precision and recall rates for each set of parameters
- Choose the best parameter set
- Apply it to the hold-out sample in the beginning to compute the precision and recall rates
- In the final step, we apply the trained algorithm to all death samples in our studied period (2014-2019)
- As we observe everyone's pre-reform predetermined wealth, income, and demographic variables, which serve as the predictors in the algorithm, we can predict everyone's counterfactual estate percentile group had the reform not happened

**Important Predictors.** We report the most important top 25 predictors in our algorithm in Figure B.1. The top 5 are: wealth four years before death, wealth five years before death, wealth 6 years before death, wealth 7 years before death, and land values four years before death.

**Figure B.1:** Top 25 Important Predictors (2017)




*Notes:* This figure shows the top 25 predictors in the random forest algorithm for the 2017 prediction. The top five predictors, shown in blue, are total wealth four years before death, total wealth five years before death, total wealth six years before death, land value four years before death, and total wealth seven years before death, respectively. This figure is discussed in Section 3.1.

**Performance.** Table B.1 presents the prediction performance, measured in precision and recall rates using the hold-out samples. Precision rate is the number of individuals being correctly predicted in the group over the number of individuals being predicted in the group. Recall rate is the number of individuals being correctly predicted in the group over the number of individuals actually in the group. Overall, the precision rates range from 61% to 82% by group, and the recall rates range from 60% to 88%.

**Table B.1:** Performance of 2017 Prediction Algorithm

| Group | Precision | Recall |
|---|---|---|
| P90 | 81.8% | 87.8% |
| P90-P96 | 60.5% | 55.3% |
| P96-P99.5 | 66.3% | 62.1% |
| P99.5 | 76.0% | 59.5% |

*Notes:* This table reports the precision and recall rates for each group in the random forest algorithm used for the 2017 prediction using the hold-out sample. Precision is defined as the share of individuals correctly predicted to belong to a group out of all individuals predicted to be in that group. Recall is defined as the share of individuals correctly predicted to belong to a group out of all individuals actually in that group. This table is discussed in Section 3.1.

## B.2 2009 Prediction Algorithm Details

**Procedures.** To construct decedents' counterfactual estates in the absence of the reform, we use a random-forest algorithm to predict a decedent's taxable estates at death based on her past wealth and demographic information. The procedure is as follows:

- Within those who died before the reform (2007-2008), select 70% as a training sample and the remaining 30% as a hold-out sample[19]
- Train a random forest model where the outcome variable is a discrete variable indicating the group of the individual: i) below P90, ii) P90-P96, iii) P96-P99.5, iv) above P99.5
- Predictors are the values and quantities of land, houses, publicly-listed stock, privately-held stock, capitalized interest income, total income, earned income, whether a business owner, all measured three years before death, and demographics of gender, age, number of kids, marital status, parents alive, spouse alive etc
- In the above step, tune parameters for the number of trees, number of variables possibly split at each node, and minimal node size
- Repeat the above step for each set of parameters and calculate the corresponding precision and recall rates for each set of parameters
- Choose the best parameter set

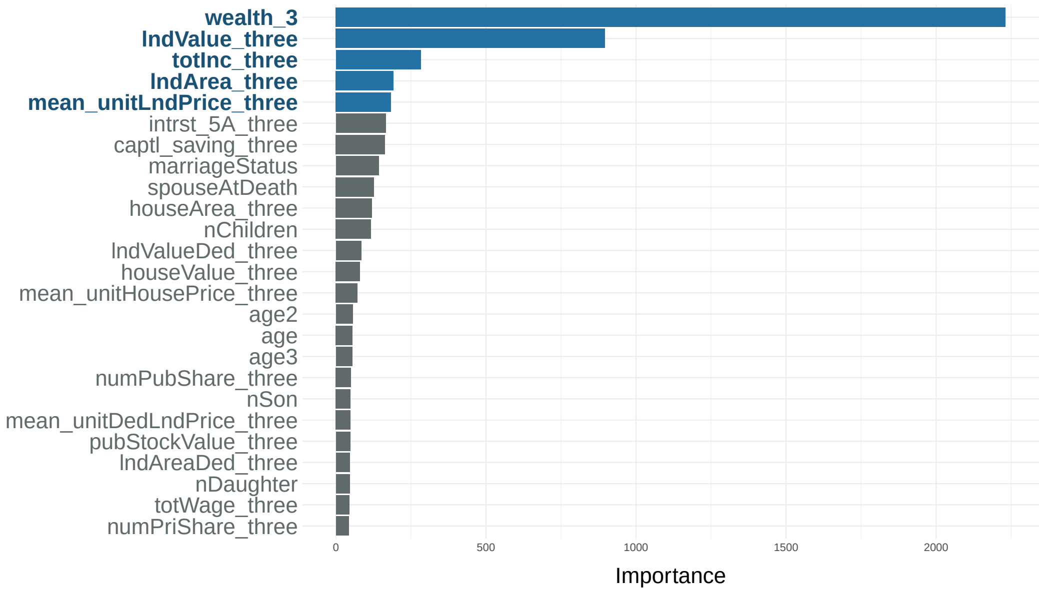
[19]We restrict the sample to 2007–2008 deaths because our wealth data begins in 2004. This ensures that lagged wealth measures (e.g., three years prior to death) are observable. For example, for a 2007 decedent, wealth three years prior corresponds to the 2004 data.

- Apply it to the hold-out sample in the beginning and compute the precision and recall rates

- In the final step, we apply the trained algorithm to all death samples in our studied period (2007-2011)

**Important Predictors.** We report the most important top 25 predictors in our algorithm. Figure B.2 presents the overall top 25 most important predictors in our algorithm (2007-2008). The top 5 are: total wealth three years before death, land values three years before death, total income three years before death, quantity of land three years before death, and the unit land price of land three years before death.

**Figure B.2:** Top 25 Important Predictors (2009)




*Notes:* This figure shows the top 25 predictors in the random forest algorithm for the 2009 prediction. The top five predictors, shown in blue, are total wealth, land value, total income, land area, and the average unit price of land, all measured three years before death. This figure is discussed in Section 3.1.

**Performance.** We report the performance of the prediction algorithm of the 2009 reform using the hold-out sample.

**Table B.2:** Performance of 2009 Prediction Algorithm

| Group | Precision | Recall |
|---|---|---|
| P90 | 70.9% | 75.8% |
| P90-P96 | 60.3% | 59.4% |
| P96-P99.5 | 63.1% | 59.2% |
| P99.5 | 67.9% | 52.7% |

*Notes:* This table reports the precision and recall rates for each group in the random forest algorithm used for the 2009 prediction using the hold-out sample. Precision is defined as the share of individuals correctly predicted to belong to a group out of all individuals predicted to be in that group. Recall is defined as the share of individuals correctly predicted to belong to a group out of all individuals actually in that group. This table is discussed in Section 3.1.

## B.3 Summary Statistics

**Table B.3:** Summary Statistics of Predicted Treated and Control Groups Before Reforms

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | 2017 Tax Increase | | 2009 Tax Cut | | |
| | Control | Treated | Control | Treated | |
| | P90-P96 | >P99.5 | P90-P96 | P96-P99.5 | >P99.5 |
| Age | 76.34 | 77.85 | 72.88 | 74.33 | 75.07 |
| Male (%) | 67.19 | 73.39 | 71.40 | 73.57 | 78.66 |
| Spouse Alive (%) | 50.32 | 60.47 | 53.76 | 54.40 | 65.40 |
| Number of Children | 3.32 | 3.55 | 3.59 | 3.80 | 3.96 |
| Taxable Estate | 8,619.5 | 244,126.0 | 5,353.5 | 14,033.6 | 119,824.1 |
| Observations | 27,407 | 2,169 | 15,575 | 8,935 | 1,059 |

*Notes:* This table presents summary statistics for pre-reform decedents in the predicted treated and control groups. For the 2017 tax increase, columns (1) and (2) report statistics for the control group (predicted 90th-96th percentiles) and treated group (predicted above 99.5th percentile), respectively, using decedents who died between 2014 and 2016. For the 2009 tax cut, column (3) reports the control group (predicted 90th-96th percentiles), while columns (4) and (5) report two treated subgroups (predicted 96th-99.5th percentiles and above 99.5th percentile, respectively), using decedents who died between 2007 and 2008. Male and spouse alive are reported as percentages. Taxable estate values are in thousands of 2016 TWD. This table is discussed in Section 3.1 and Section 5.4.

## B.4 Measurement Error in the Predicted Treatment Dummy

We discuss the measurement error bias in our main specification. When treatment status is predicted using machine-learning methods, the resulting misclassification error does not necessarily lead to attenuation bias, as shown in Battaglia et al. (2025). However, to build intuition, it is helpful to consider a simpler textbook case in which misclassification produces attenuation bias. This simplified case provides a transparent way to see how the bias enters both the reduced form and the first stage, and why it cancels in the Wald ratio. The reduced-form specification is:

$$\log y_i = \alpha_0 + \alpha_1 \widehat{T_i} \times Post_i + \alpha_2 \widehat{T_i} + \alpha_3 Post_i + u_i$$

Taking expectations conditional on $\widehat{T_i}$ and $Post_i$:

$$\begin{aligned}
E(y_i|\widehat{T_i} = 1, Post_i = 1) &= \alpha_0 + (\alpha_1 + \alpha_2) \cdot P(T_i = 1|\widehat{T_i} = 1) + \alpha_3 \\
E(y_i|\widehat{T_i} = 1, Post_i = 0) &= \alpha_0 + \alpha_2 \cdot P(T_i = 1|\widehat{T_i} = 1) \\
E(y_i|\widehat{T_i} = 0, Post_i = 1) &= \alpha_0 + (\alpha_1 + \alpha_2) \cdot P(T_i = 1|\widehat{T_i} = 0) + \alpha_3 \\
E(y_i|\widehat{T_i} = 0, Post_i = 0) &= \alpha_0 + \alpha_2 \cdot P(T_i = 1|\widehat{T_i} = 0)
\end{aligned}$$

Conditional on the sample,

$$\hat{\alpha}_1 \xrightarrow{p} \alpha_1 \cdot \left[P(T_i = 1|\widehat{T_i} = 1) - P(T_i = 1|\widehat{T_i} = 0)\right]$$

On the other hand, the first stage is:

$$\log\left(1 - \tau_i^{Post}\right) - \log\left(1 - \tau_i^{Pre}\right) = \beta_0 + \beta_1 \widehat{T_i} + v_i$$

Assuming the mapping from predictors to treatment status is the same before and after the reform in the absence of the reform, the same bias applies to the first stage. So, conditional on the sample,

$$\hat{\beta}_1 \xrightarrow{p} \beta_1 \cdot \left[P(T_i = 1|\widehat{T_i} = 1) - P(T_i = 1|\widehat{T_i} = 0)\right]$$

Our elasticity estimator is the Wald ratio $\hat{\epsilon} = \hat{\alpha}_1/\hat{\beta}_1$. As both the numerator and denominator are multiplied by the same attenuation bias, the bias cancels out.

## B.5 Robustness check: Stability of Algorithm

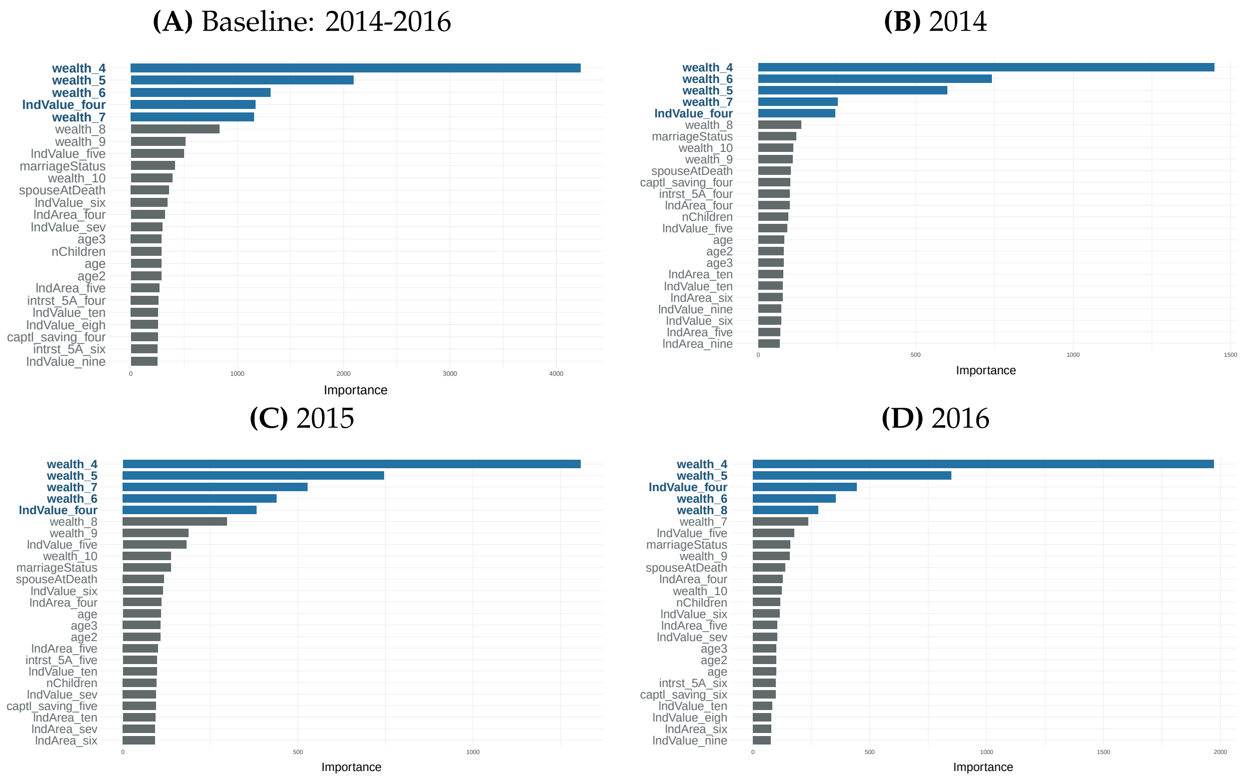
To assess the stability of the prediction algorithm, we conduct a series of checks using only the pre-reform period data. These checks show that the model's performance and the importance of predictors are stable over time.

### B.5.1 2017: Year-by-year cross-validation

We take each pre-reform calendar year (2014, 2015, 2016), randomly assign 70% to training and 30% to hold-out, estimate the model on the training slice, and compute precision and recall on the hold-out slice.

**Most-important predictors.** Panel A in Figure B.3 presents the overall top 25 most important predictors in our algorithm (2014-2016). Panels B, C, and D present the results for 2014, 2015, and 2016 individually. In all cases, the most recent total wealth variables rank highest. While the ordering of predictors ranked two through six occasionally shifts, these changes have no substantive effect on the results.

**Figure B.3:** Top 25 Important Predictors Using Different Subsamples (2017)




*Notes:* This figure shows the top 25 important predictors using different subsamples as a robustness check for the 2017 prediction. Panel (A) presents the baseline period, 2014–2016, used in the main prediction for the 2017 tax increase. Panel (B) uses only the 2014 sample, Panel (C) uses only the 2015 sample, and Panel (D) uses only the 2016 sample. This figure is discussed in Section 3.4.

**Predictive performance.** Table B.4 presents the prediction performance, measured in precision and recall rates using the hold-out samples. Precision rate is the number of individuals being correctly predicted in the group over the number of individuals being predicted in the group. Recall rate is the number of individuals being correctly predicted in the group over the number of individuals actually in the group.

Column (1) and (2) refer to the precision and recall rates of our baseline performance of the 2017 prediction algorithm, which uses 70% 2014-2016 as the training samples and 30% as the hold-out. Then, to show the stability over time, in Column (3) to (8) we split them into 2014, 2015, and 2016, respectively. Overall, when splitting them by year, the results are similar, landing support on the stability of the algorithm.

**Table B.4:** Year-by-year Prediction Performance (2017)

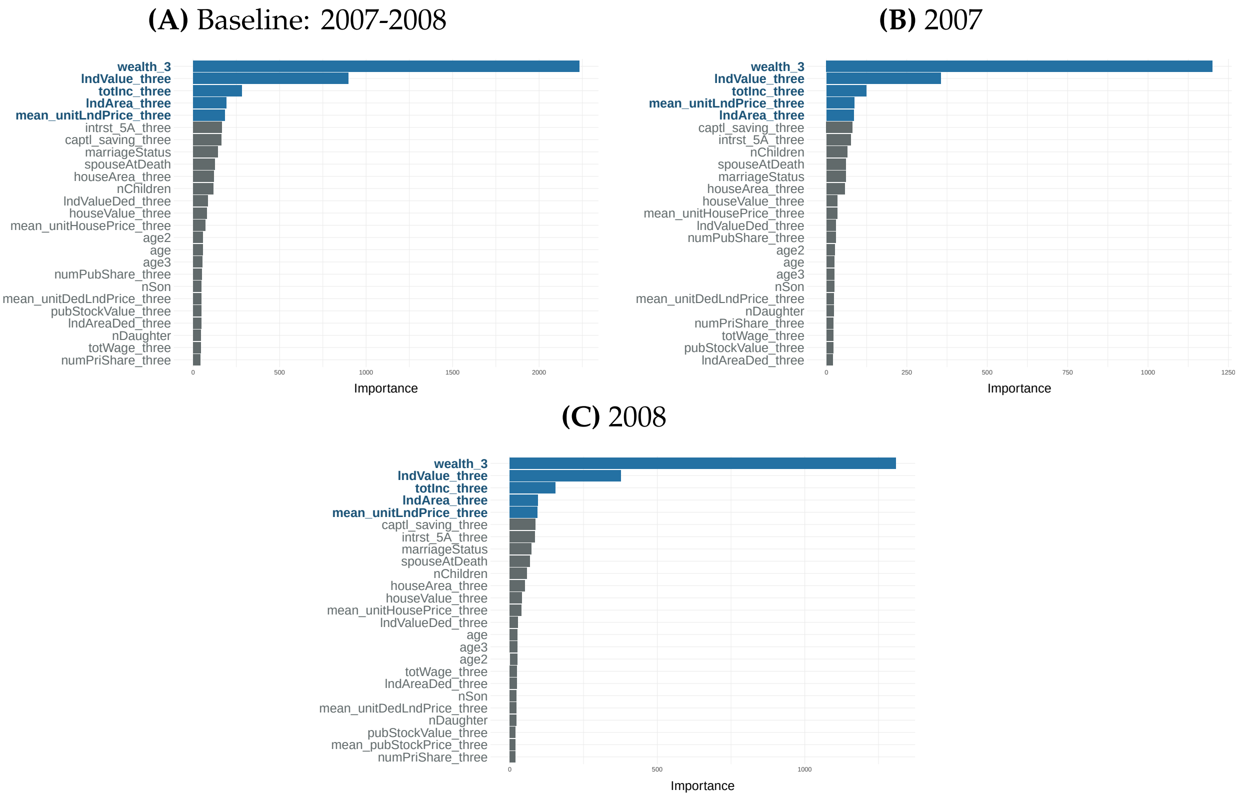
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | 2014-2016 (Baseline) | | 2014 | | 2015 | | 2016 | |
| Group | Precision | Recall | Precision | Recall | Precision | Recall | Precision | Recall |
| P90 | 81.8% | 87.8% | 78.5% | 88.1% | 82.4% | 88.1% | 84.0% | 87.5% |
| P90-P96 | 60.5% | 55.3% | 60.1% | 54.2% | 60.7% | 56.1% | 60.7% | 55.5% |
| P96-P99.5 | 66.3% | 62.1% | 67.6% | 57.8% | 67.3% | 62.0% | 64.4% | 66.3% |
| P99.5 | 76.0% | 59.5% | 78.5% | 56.7% | 75.3% | 63.0% | 74.8% | 59.0% |

*Notes:* This table shows the predictive performance of different subsamples. Column (1)-(2) is the baseline period, 2014-2016, used in the main prediction for the 2017 tax increase. Column (3)-(4) use only the 2014 sample, Column (5)-(6) use only the 2015 sample, and Column (7)-(8) use only the 2016 sample. Precision is defined as the share of individuals correctly predicted to belong to a group out of all individuals predicted to be in that group. Recall is defined as the share of individuals correctly predicted to belong to a group out of all individuals actually in that group. This table is discussed in Section 3.4.

### B.5.2 2009: Year-by-year cross-validation

We take each pre-reform calendar year (2007, 2008), randomly assign 70% to training and 30% to hold-out, estimate the model on the training slice, and compute precision and recall on the hold-out slice.

**Figure B.4:** Top 25 Important Predictors Using Different Subsamples (2009)




*Notes:* This figure shows the top 25 important predictors using different subsamples as a robustness check for the 2009 prediction. Panel (A) presents the baseline period, 2007-2008, used in the main prediction for the 2009 tax cut. Panel (B) uses only the 2007 sample. Panel (C) uses only the 2008 sample. This figure is discussed in Section 3.4.

**Table B.5:** Year-by-year Prediction Performance (2009)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | 2007-2008 (Baseline) | | 2007 | | 2008 | |
| Group | Precision | Recall | Precision | Recall | Precision | Recall |
| P90 | 70.9% | 75.8% | 69.7% | 75.2% | 71.9% | 76.4% |
| P90-P96 | 60.3% | 59.4% | 60.8% | 58.8% | 60.0% | 60.0% |
| P96-P99.5 | 63.1% | 59.2% | 62.2% | 59.4% | 64.0% | 59.1% |
| P99.5 | 67.9% | 52.7% | 61.8% | 48.1% | 73.1% | 56.6% |

*Notes:* This table shows the predictive performance of different subsamples. Column (1)-(2) is the baseline period, 2007-2008, used in the main prediction for the 2009 tax cut. Column (3)-(4) use only the 2007 sample, Column (5)-(6) use only the 2008 sample. Precision is defined as the share of individuals correctly predicted to belong to a group out of all individuals predicted to be in that group. Recall is defined as the share of individuals correctly predicted to belong to a group out of all individuals actually in that group. This table is discussed in Section 3.4.

#### B.5.3 2017: Cross-year prediction (train on year A, test on year B)

We next train the model on one pre-reform year ($t$) and test it on a different pre-reform year ($t + 1$ or $t + 2$). This imitates our main application, where the model trained on 2014–2016 predicts the post-reform years. Table B.6 shows the predictive performance. At the one-year horizon, training on 2014 and testing on 2015, or training on 2015 and testing on 2016, exhibit similar precision and recall rates. When the horizon is stretched to two years, i.e. trained on 2014 and tested on 2016, there is a slight decline in precision rate for the upper groups, while the recall rate does not change much. Overall, they indicate that the model generalizes well across time in the pre-period, reinforcing the validity of our identification assumption.

**Table B.6:** Cross-Year Training Prediction Performance (2017)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Train 2014 Test 2015 | | Train 2015 Test 2016 | | Train 2014 Test 2016 | |
| Group | Precision | Recall | Precision | Recall | Precision | Recall |
| P90 | 84.6% | 80.8% | 84.6% | 85.4% | 86.9% | 77.5% |
| P90–P96 | 56.6% | 57.0% | 59.0% | 58.1% | 55.5% | 57.1% |
| P96–P99.5 | 59.0% | 67.5% | 63.6% | 64.3% | 52.3% | 69.3% |
| P99.5 | 69.7% | 68.5% | 69.9% | 60.8% | 60.7% | 68.1% |

*Notes:* This table reports the predictive performance of different cross-year training and hold-out samples. Columns (1)–(2) use 2014 as the training sample and 2015 as the hold-out sample. Columns (3)–(4) use 2015 as the training sample and 2016 as the hold-out sample. Columns (5)–(6) use 2014 as the training sample and 2016 as the hold-out sample. Precision is the share of individuals correctly predicted to belong to a group out of all individuals predicted to be in that group. Recall is the share of individuals correctly predicted to belong to a group out of all individuals actually in that group. This table is discussed in Section 3.4.

#### B.5.4 2009: Cross-year prediction (train on year A, test on year B)

**Table B.7:** Cross-Year Training Prediction Performance (2009)

| | (1) | (2) |
|---|---|---|
| | Train 2007 Test 2008 | |
| Group | Precision | Recall |
| P90 | 72.5% | 73.8% |
| P90-P96 | 58.1% | 60.8% |
| P96-P99.5 | 63.0% | 58.9% |
| P99.5 | 72.4% | 49.0% |

*Notes:* This table reports the predictive performance of different cross-year training and hold-out samples. Columns (1)–(2) use 2007 as the training sample and 2008 as the hold-out sample. Precision is the share of individuals correctly predicted to belong to a group out of all individuals predicted to be in that group. Recall is the share of individuals correctly predicted to belong to a group out of all individuals actually in that group. This table is discussed in Section 3.4.

## B.6 Robustness check: Simple wealth-based classification

For transparency and robustness, we use a wealth-based rule that classifies treated and control groups based on their pre-reform total wealth without using any machine learning tools. This is motivated by the fact that the most recent total wealth variables have consistently been the most influential predictors in our random forest algorithm. For the 2017 tax increase, we define the treated group as those whose wealth four and five years before death were at the top 0.5%, and the control group as those between the 90th and 96th percentiles. Similarly, for the 2009 tax cut, we use wealth three years before death to define these groups.

**Table B.8:** Elasticity Estimates Using Simple Wealth-based Classification

| | (1) 2017 Tax Increase | (2) | (3) 2009 Tax Cut | (4) |
|---|---|---|---|---|
| | All | All | Splitting into subgroups | |
| | Top 0.5% | Top 4% | Top 4%-0.5% | Top 0.5% |
| **Panel A: First stage** | | | | |
| $\widehat{T_i}$ | -0.060 | 0.091 | 0.061 | 0.284 |
| | (0.001) | (0.001) | (0.001) | (0.005) |
| Observations | 21,594 | 22,561 | 21,288 | 14,172 |
| **Panel B: Reduced form** | | | | |
| $\widehat{T_i} \times Post_i$ | -0.194 | 0.092 | 0.104 | 0.127 |
| | (0.037) | (0.018) | (0.017) | (0.044) |
| Observations | 49,595 | 62,028 | 58,729 | 39,056 |
| **Panel C: Implied Elasticity** | | | | |
| $\varepsilon$ | 3.233 | 1.011 | 1.705 | 0.447 |
| | (0.619) | (0.198) | (0.280) | (0.155) |

*Notes:* This table reports elasticity estimates using a simple wealth-based classification, where treatment status is assigned solely on the basis of pre-death total wealth, without machine-learning predictions. For the 2017 tax increase, we define the treated group as those whose wealth four and five years before death were in the top 0.5%, and the control group as those between the 90th and 96th percentiles. Similarly, for the 2009 tax cut, we use wealth from three years prior to death to define these groups. Column (1) shows the result of the 2017 tax increase. Columns (2)-(4) are the results of the 2009 tax cut where (2) uses the full treated group top 4%. Column (3)-(4) split the top 4% into top 4%-0.5% and top 0.5%. Robust standard errors are in parentheses. This table is discussed in Section 3.4.

## B.7 Robustness check: Alternative Gradient Boosting Algorithm

In our baseline prediction, we use a random forest algorithm. To test the sensitivity of the choice of an algorithm, we replace it with a gradient boosting algorithm. We first present the 2017 reform results, ordered by the important predictors, the prediction performance, and analogously for the 2009 reform.

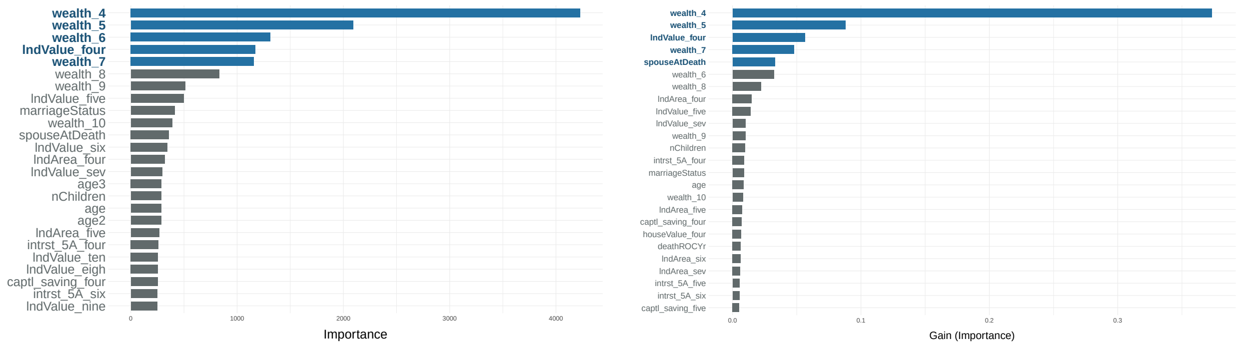
### B.7.1 2017 Reform

**Important predictors.** Figure B.5 shows the comparison of the top 25 important predictors, where Panel A is the baseline prediction algorithm using the random forest and Panel B uses the gradient boosting algorithm. Overall, the top influential predictors are very similar.

**Figure B.5:** Top Important Predictors: Baseline v.s. Gradient Boosting (2017)

**(A)** Baseline: Random forest

**(B)** Alternative: Gradient boosting




*Notes:* This figure presents the top 25 most important predictors under alternative algorithms for the 2017 tax increase. Panel (A) reports the baseline estimation using the random forest algorithm. Panel (B) reports the results using the gradient boosting algorithm. This figure is discussed in Section 3.4.

**Predictive performance.** Table B.9 presents the prediction performance comparison between the random forest (baseline) and gradient boosting. Overall, the precision and recall rates across groups are highly identical.

**Table B.9:** Prediction Performance: Baseline v.s. Gradient Boosting (2017)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Random forest (baseline) | | Gradient boosting | |
| Group | Precision | Recall | Precision | Recall |
| P90 | 81.8% | 87.8% | 81.7% | 87.7% |
| P90-P96 | 60.5% | 55.3% | 60.5% | 55.5% |
| P96-P99.5 | 66.3% | 62.1% | 66.6% | 61.3% |
| P99.5 | 76.0% | 59.5% | 74.3% | 61.6% |

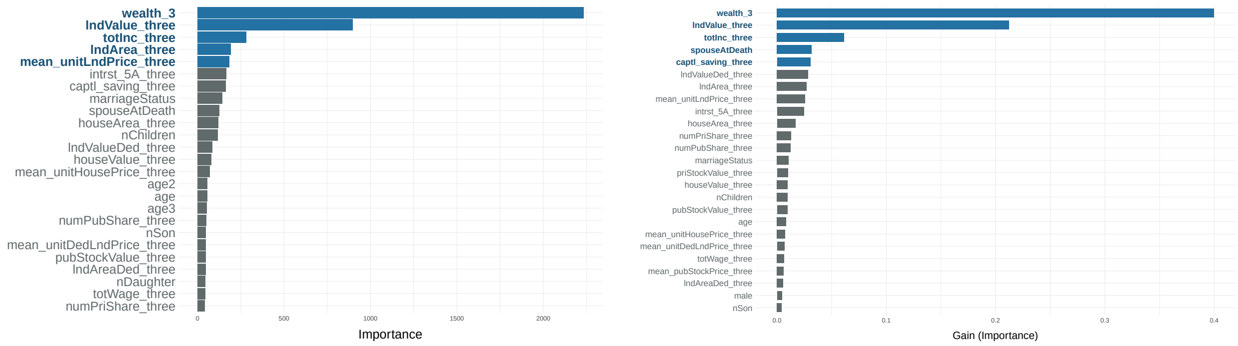
*Notes:* This table reports the precision and recall rates under alternative algorithms for the 2017 tax increase. Column (1)-(2) present the baseline estimation using the random forest algorithm. Column (3)-(4) report the resuls using the gradient boosting algorithm. Precision is defined as the share of individuals correctly predicted to belong to a group out of all individuals predicted to be in that group. Recall is defined as the share of individuals correctly predicted to belong to a group out of all individuals actually in that group. This table is discussed in Section 3.4.

### B.7.2 2009 Reform

**Important predictors.** Figure B.5 presents the comparison of the top 25 important predictors, where Panel A is the baseline random forest algorithm and Panel B is the gradient boosting algorithm. Overall, the top initial predictors are very similar.

**Figure B.6:** Top Important Predictors: Baseline v.s. Gradient Boosting (2009)

**(A)** Baseline: Random forest **(B)** Alternative: Gradient boosting




*Notes:* This figure presents the top 25 most important predictors under alternative algorithms for the 2009 tax cut. Panel (A) reports the baseline estimation using the random forest algorithm. Panel (B) reports the results using the gradient boosting algorithm. This figure is discussed in Section 3.4.

**Predictive performance.** Table B.10 presents the prediction performance comparison between the random forest (baseline) and gradient boosting. Overall, the precision and recall rates are highly identical.

**Table B.10:** Prediction Performance: Random Forest v.s. Gradient Boosting (2009)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Random forest (baseline) | | Gradient boosting | |
| Group | Precision | Recall | Precision | Recall |
| P90 | 70.9% | 75.8% | 69.7% | 74.2% |
| P90-P96 | 60.3% | 59.4% | 58.2% | 58.5% |
| P96-P99.5 | 63.1% | 59.2% | 63.3% | 57.9% |
| P99.5 | 67.9% | 52.7% | 68.2% | 51.2% |

*Notes:* This table reports the precision and recall rates under alternative algorithms for the 2009 tax cut. Column (1)-(2) present the baseline estimation using the random forest algorithm. Column (3)-(4) report the results using the gradient boosting algorithm. Precision is defined as the share of individuals correctly predicted to belong to a group out of all individuals predicted to be in that group. Recall is defined as the share of individuals correctly predicted to belong to a group out of all individuals actually in that group. This table is discussed in Section 3.4.

## B.8 Robustness check: Alternative Placebo Reform Year

To test the robustness of our prediction method, we use the years 2012, 2013, 2014, and 2015 as placebo reform years and show that no effect is detected under these scenarios. The procedure is as follows. In the case of the placebo reform year 2012, we take those who died between 2009 and 2011 and train a random-forest algorithm as previously described to categorize their estate groups. We then apply the trained algorithm to the death samples between 2009 and 2013 so that every dead person has a counterfactual estate group that they belong to. We define those who are above the top 0.5% percentile as the treated and those between the P90-P96 percentiles as the control as before. We estimate Equation 4 and present the estimates in Table B.11. In all scenarios, the estimated coefficients are statistically insignificant, indicating that no effect on taxable estate is detected.

**Table B.11:** Treatment Effect of Placebo Reform Years

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Period | 2009-2013 | 2010-2014 | 2011-2015 | 2012-2016 |
| Placebo Reform | 2012 | 2013 | 2014 | 2015 |
| $\widehat{T_i} \times Post_i$ | 0.043 | -0.063 | 0.045 | -0.044 |
| | (0.040) | (0.041) | (0.036) | (0.034) |
| Observations | 42,265 | 42,758 | 46,600 | 49,991 |

*Notes:* This table reports the treatment effect estimates from Equation (4), where reform years are randomly assigned as placebo tests. Column (1) assigns 2012 as the reform year, Column (2) assigns 2013, Column (3) assigns 2014, and Column (4) assigns 2015. Robust standard errors are in parentheses. This table is discussed in Section 3.4.

## B.9 Robustness check: Alternative Control Groups

We show the robustness to alternative definitions of our control groups in both reforms. The choice of control group involves a tradeoff: we would like to select a group whose estates evolve similarly to the treated group (closer to the top percentiles) while avoiding contamination from the reform (further down in the distribution). For the 2017 increase, elasticity estimates are similar when using alternative definitions, except for the immediately lower group, potentially because these taxpayers behaved in a forward-looking manner and attempted to avoid crossing the threshold, consistent with Garbinti et al. (2023). For the 2009 tax cut, the estimates are similarly consistent across alternative control groups, except again for the immediately below group.

**Table B.12:** Elasticity Estimates Using Alternative Control Groups

| | (1) 2017 Tax Increase | (2) | (3) 2009 Tax Cut | (4) |
|---|---|---|---|---|
| | Top 0.5% | Top 4% | Top 4%-0.5% | Top 0.5% |
| P90-P93 | 2.985 | 1.641 | 2.507 | 0.600 |
| | (0.502) | (0.185) | (0.249) | (0.134) |
| P93-P96 | 3.250 | 0.281 | 0.621 | 0.156 |
| | (0.488) | (0.177) | (0.243) | (0.134) |
| P96-P98 | 2.791 | | | |
| | (0.494) | | | |
| P98-P99.5 | 0.683 | | | |
| | (0.540) | | | |
| P90-96 (baseline) | 2.757 | 1.310 | 1.917 | 0.465 |
| | (0.394) | (0.161) | (0.210) | (0.133) |

*Notes:* This table presents elasticity estimates using alternative definitions of the control group. Column (1) reports the results for the 2017 tax increase, where the treated group is the top 0.5% and the control group is alternatively defined as percentiles 90–93, 93–96, 96–98, or 98–99.5. Columns (2)–(4) report the results for the 2009 tax cut. Column (2) uses the full treated group (top 4%). Columns (3) and (4) split the treated group into the top 4%–0.5% and the top 0.5%, respectively. Standard errors are in parentheses. This table is discussed in Section 3.4.

## B.10 Robustness check: Anticipation

Appendix Table B.13 presents the estimated elasticities assuming that there is an anticipatory behavior from decedents. We define the reform year as a year before the true reform year, train the algorithm using the newly defined pre-reforms samples, predict the treated and control groups, and estimate the diff-in-diff.

**Table B.13:** Elasticity Estimates with Anticipatory Behavior

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 2017 Tax Increase | | 2009 Tax Cut | |
| | Baseline | Anticipated | Baseline | Anticipated |
| **Panel A: First stage** | | | | |
| $\widehat{T}_i$ | -0.074 | -0.067 | 0.331 | 0.333 |
| | (0.001) | (0.001) | (0.005) | (0.007) |
| Observation | 29,576 | 18,863 | 16,634 | 8,458 |
| **Panel B: Reduced form** | | | | |
| $\widehat{T}_i \times Post_i$ | -0.204 | -0.166 | 0.154 | 0.151 |
| | (0.029) | (0.036) | (0.044) | (0.056) |
| Observation | 63,627 | 64,811 | 46,410 | 48,652 |
| **Panel C: Elasticity** | | | | |
| $\varepsilon$ | 2.757 | 2.478 | 0.465 | 0.453 |
| | (0.394) | (0.539) | (0.133) | (0.168) |

*Notes:* This table reports elasticity estimates under the assumption that individuals anticipated the reforms by assigning the treatment year one year earlier, i.e., 2016 for the 2017 tax increase and 2008 for the 2009 tax cut. Panel A presents the first-stage effect on the change in log net-of-tax rate using pre-reform samples. Panel B reports the reduced-form effect on log taxable estates using all samples. Panel C shows the implied elasticities calculated using the Delta method as reduced-form scaled by first-stage. Columns (1)–(2) present the results for the 2017 tax increase, where Column (1) is our baseline estimate and Column (2) incorporates anticipation. Columns (3)–(4) present the analogous results for the 2009 tax cut. In both reforms, the control groups are those predicted between the 90th and 96th percentiles and the treated group is the top 0.5%. Standard errors are in parentheses. This table is discussed in Section 3.4.

## B.11 Robustness: Macroeconomic Condition

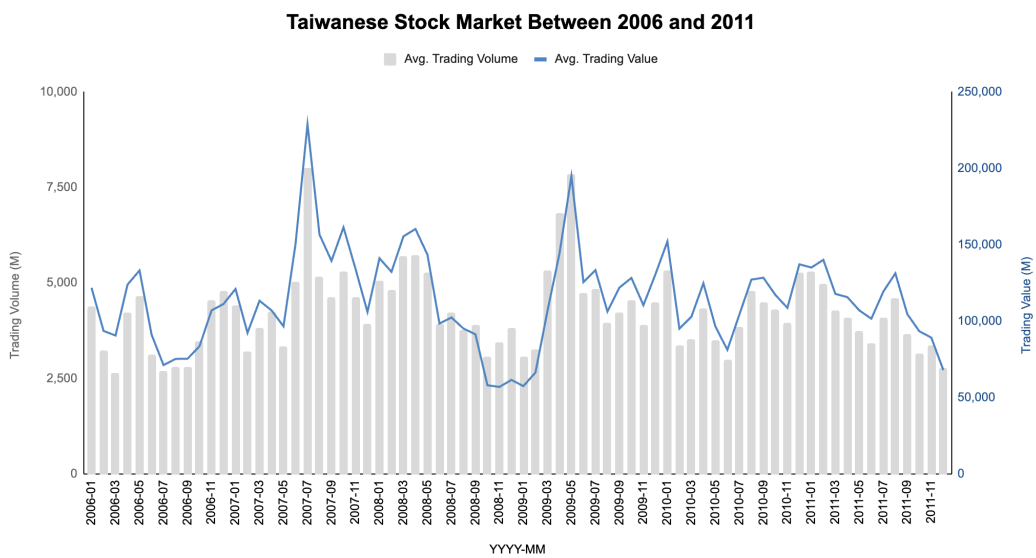
One of our main findings is that we observe a stronger response to the tax increase than to the tax cut. A potential explanation could be the macroeconomic context, as the financial crisis coincided with the 2009 tax cut, while the economy was relatively stable during the 2017 tax increase. If this difference matters, one could argue that the smaller response to the 2009 tax cut reflects individuals being constrained in adjusting their assets amid a collapsing financial market. We provide three explanations to show this is not the case.

First, as shown later in Section 4.1, when we break down estate items, deposit savings, a liquid and movable asset that can be adjusted even during a recession, are still valued differently between a tax increase and a cut.

Second, if the concern is the stock market being "illiquid" during the recession, we show that trading in the Taiwanese Stock Exchange rebounded relatively quickly during the financial crisis. As shown in Appendix Figure B.7, trading volume dipped towards the end of 2008 but began to recover in the first quarter of 2009. By April 2009, trading volume had returned to pre-crisis levels, suggesting that the stock market was still very dynamic at the time.

Third, to further support our argument, we use individuals' pre-reform stock holding fraction to proxy their exposure to the financial crisis. We classify the top 0.5% of decedents into two groups: those with a stock holding fraction above the mean (highly exposed) and those below the mean (less exposed), and we estimate the specifications in Equations (3) and (4) separately for each group. Appendix Table B.14 presents the elasticity estimates for each group. The results indicate that the 95% confidence intervals overlap and there is no differential response based on stock holding fraction, lending support to the argument that exposure to the financial crisis does not explain the observed asymmetry in responses.

**Figure B.7:** Taiwanese Stock Exchange Trading Volume and Values 2006–2011




*Notes:* This figure presents the trading volume and values in the Taiwanese Stock Exchange between 2006 and 2011. The source is `https://www.twse.com.tw/en/`. This figure is discussed in Section 3.4.

**Table B.14:** Elasticity Estimates Split by Pre-reform Fraction of Stockholding, Top 0.5%

| | (1) Low Stock | (2) High Stock |
|---|---|---|
| **Panel A: First stage** | | |
| $\widehat{T_i}$ | 0.312 | 0.366 |
| | (0.006) | (0.009) |
| Observations | 13,546 | 3,088 |
| **Panel B: Reduced form** | | |
| $\widehat{T_i} \times Post_i$ | 0.142 | 0.151 |
| | (0.051) | (0.082) |
| Observations | 37,824 | 8,586 |
| **Panel C: Implied Elasticity** | | |
| $\varepsilon$ | 0.455 | 0.413 |
| | (0.164) | (0.224) |

*Notes:* This table reports the elasticity estimates for the top 0.5% during the tax cut, split by their pre-reform fraction of stockholdings as a proxy for exposure to the financial crisis. Column (1) presents the results for individuals whose stockholdings relative to total wealth before the reform were below the average, while Column (2) presents the results for those above the average. Robust standard errors are in parentheses. This table is discussed in Section 3.4.

## B.12 Robustness check: Gifting

The goals of this robustness check are twofold. First, we show that changes in taxable estates after both reforms are not due to changes in gifting. Second, we analyze the incentives involved by considering the costs of estate, gifting, and other avoidance strategies, as well as an individual's expected survival within two years (as this affects whether gifts count toward the estate tax). This allows us to explore the tradeoffs between these factors and test if these mechanisms hold empirically.

**Changes in gifting do not account for changes in estates.** For the first goal, we estimate Equation (4) with gifts made within two years before death, scaled by pre-reform average wealth. Appendix Table B.15 presents the results: Column (1) shows that, after the 2017 tax increase, decedents reduced gifts as a share of baseline wealth by about 2.6 percentage points (pp), suggesting that decreased estates are not due to increased gifting, as we would expect an increase in gifting if it were driving the decrease in taxable estates. Column (2) indicates no significant change in gifting after the 2009 tax cut, supporting that gifting does not explain the increased estates.

**Table B.15:** The Effect of Reforms on Gifts Within Two Years Before Death, Top 0.5%

| | (1)<br>2017 Tax Increase | (2)<br>2009 Tax Cut |
|---|---|---|
| $\widehat{T}_i \times Post_i$ | -0.026 | 0.003 |
| | (0.006) | (0.005) |
| Observations | 63,627 | 46,410 |

*Notes:* This table reports the treatment effect results of Equation (4) where the dependent variable is gifts made within two years before death, scaled by pre-reform average total wealth. The treated group comprises individuals predicted to be in the top 0.5%, and the control group comprises those predicted to be in the P90–P96 range. Column (1) shows the results of the 2017 tax increase, and Column (2) shows those of the 2009 tax cut. Robust standard errors are in parentheses. This table is discussed in Section 3.4.

**Mechanism and predictions.** Why do we see different gifting responses between the two reforms? We analyze the incentives in each reform, as shown in Appendix Table B.16. Gifting responses depend on: (i) the relative cost of leaving an estate versus gifting, (ii) the relative cost of gifting versus other tax avoidance strategies. In addition, these channels differ in whether individuals expect to live or die within the two-year window.

Panel A in Appendix Table B.16 outlines the mechanisms underlying the 2017 reform, where both estate and gift taxes increased. For individuals expecting to die within two years, gifting provides no tax advantage, as any gifts made within this timeframe are included in the estate tax base. As a result, the relative cost between estate and gifting does not matter. However, the increased cost of gifting relative to other avoidance strategies matters, leading individuals to reduce gifting in favor of other strategies. Overall, we predict that these individuals will gift less.

For those expecting to live longer than two years, the effect is less clear. The increased relative cost of gifting versus leaving an estate creates a mixed incentive: they may choose to gift more to reduce a now higher future estate tax, but this incentive is reduced by the higher cost of gifting. In addition, the increased cost of gifting relative to other avoidance strategies may discourage gifting. The combined effects of these two channels result in an ambiguous impact on gifting behavior.

Panel B discusses the predictions of the 2009 reform when both estate and gift taxes decreased. For individuals expecting to die within two years, the relative cost between estate and gifting remains irrelevant. Although the cost of gifting relative to other avoidance strategies is now lower, it is unlikely that individuals will switch from other avoidance strategies to gifting, as gifts will be counted toward the tax base. Therefore, we expect no change in gifting behavior.

For those expecting to live longer than two years, the relative cost between estate and gifting yields an ambiguous effect: they may choose to gift more to reduce future estate tax liability, but the lower cost of leaving an estate could reduce the incentive to gift now. Regarding the relative cost between gifting and other avoidance strategies, the lower gift tax encourages a shift from other strategies to gifting, which could lead to increased gifting. The combined effect of these channels is thus ambiguous.

We test these predictions in Appendix Table B.17, using sudden death as a proxy for individuals' expectations of survival.[20] The results of the 2017 tax increase are in Columns (1) and (2). Nonsudden deaths (those likely expecting to die within two years) reduced gifting relative to baseline wealth by about 2.5 pp, while sudden deaths showed no change. For the 2009 tax cut, shown in Columns (3) and (4), nonsudden deaths showed no change in the ratio of gifting to baseline wealth, while sudden deaths showed a 3.5 pp increase. This increase suggests that the combined effect of gifting to reduce estate and shifting from other avoidance strategies to gifting due to the lowered gift tax dominates the reduced cost of leaving an estate. These findings are consistent with our predictions.

[20] We define a death being sudden if the cause of death is one of the following: traffic accidents, natural disasters, lightning strikes, workplace accidents, acute asthma, strokes, choking, drowning, fire.

**Table B.16:** Predictions of Gifting Behavior When Estate and Gift Taxes Change

| | **Expect to Die Within Two Years (nonsudden deaths)** | **Expect to Live Within Two Years (sudden deaths)** |
|---|---|---|
| **Panel A: 2017 Both Taxes Increase** | | |
| Estate vs. Gifting | Gifting does not reduce estate ⇒ **No change** | Gifting can reduce estate, but gifting is now more costly ⇒ **Ambiguous**. Could gift more or less |
| Gifting vs. Other Avoidance | Want to switch gifting to other avoidance due to higher cost of gifting ⇒ **Gift less** | Want to switch gifting to other avoidance as gifting is now more costly ⇒ **Gift less** |
| **Overall Prediction** | **Gift less** | **Ambiguous** |
| **Panel B: 2009 Both Taxes Decrease** | | |
| Estate vs. Gifting | Gifting does not reduce estate ⇒ **No change** | Gifting reduces estate, but estate is now cheaper ⇒ **Ambiguous**. Could gift more or less |
| Gifting vs. Other Avoidance | Will not want to switch from other avoidance to gifting as it will count toward tax base ⇒ **No change** | May switch other avoidance to gifting due to reduced cost of gifting ⇒ **Gift more** |
| **Overall Prediction** | **No change** | **Ambiguous** |

*Notes:* This table summarizes the predicted effects of estate and gift tax changes on gifting behavior under different scenarios. Panel A considers the 2017 reform, in which both estate and gift taxes increased, and Panel B considers the 2009 reform, in which both taxes decreased. The scenarios are separated by expected survival: nonsudden deaths proxy for individuals who likely anticipated death within two years, while sudden deaths proxy for those who did not. The table outlines how the relative cost of leaving an estate, making gifts, or using other avoidance strategies shapes predicted gifting responses in each case. This table is discussed in Section 3.4.

**Table B.17:** The Effect of Reforms on Gifts Within Two Years Before Death By Cause of Death, Top 0.5%

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | 2017 Tax Increase | | 2009 Tax Cut | |
| | Non-sudden | Sudden | Non-sudden | Sudden |
| $\widehat{T}_i \times Post_i$ | -0.025 | -0.145 | 0.002 | 0.035 |
| | (0.006) | (0.092) | (0.006) | (0.016) |
| Observations | 63,061 | 557 | 45,703 | 529 |

*Notes:* This table reports the treatment effect results of Equation (4), where the dependent variable is gifts made within two years before death, scaled by pre-reform average total wealth. The treated group comprises individuals predicted to be in the top 0.5% of the estate distribution, and the control group comprises those predicted to be between the 90th and 96th percentiles. Columns (1)–(2) present results for the 2017 tax increase, where Column (1) corresponds to nonsudden deaths and Column (2) to sudden deaths. Columns (3)–(4) present analogous results for the 2009 tax cut. Robust standard errors are in parentheses. This table is discussed in Section 3.4.

# C Appendix to Section 4: Mechanism

## C.1 Source of Responses: Elasticity Estimates Details by Item

**Table C.1:** Elasticity by Item Computation for 2017 Tax Increase, Top 0.5%

| | (1)<br>Housing | (2)<br>Financial | (3)<br>Deposit | (4)<br>OthDed | (5)<br>Charity & Allow. |
|---|---|---|---|---|---|
| **Panel A: First stage** | | | | | |
| $\widehat{T}_i$ | -0.074 | -0.074 | -0.074 | -0.074 | -0.074 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| **Panel B: Reduced form** | | | | | |
| $\widehat{T}_i \times Post_i$ | 32.26 | -4,256.75 | -1,124.07 | 1,108.97 | 1,329.51 |
| | (973.31) | (535.96) | (254.96) | (770.42) | (372.74) |
| Pre-period mean | 72,698.56 | 20,603.28 | 8,130.71 | 27,942.14 | 7512.00 |
| Rel. change | 0.0004 | -0.2066 | -0.1383 | 0.0397 | 0.1770 |
| | (0.0134) | (0.0260) | (0.0314) | (0.0276) | (0.0496) |
| **Panel C: Implied Elasticity** | | | | | |
| | -0.0054 | 2.7919 | 1.8689 | -0.5365 | -2.3919 |
| | (0.1811) | (0.3534) | (0.4251) | (0.3730) | (0.6710) |

*Notes:* This table reports item-level elasticity estimates with respect to the net-of-tax rate for the top 0.5% of the estate distribution during the 2017 tax increase. Panel A shows the first-stage effect on the change in log net-of-tax rate. Panel B reports the reduced-form effects on reported values of each item, along with the treated group's pre-period means in thousands TWD and relative changes (scaled by treated group's pre-reform mean). Panel C presents the implied elasticities using the Delta method. Robust standard errors are in parentheses. This table is discussed in Section 4.1.

**Table C.2:** Elasticity by Items Computation for 2009 Tax Cut, Top 0.5%

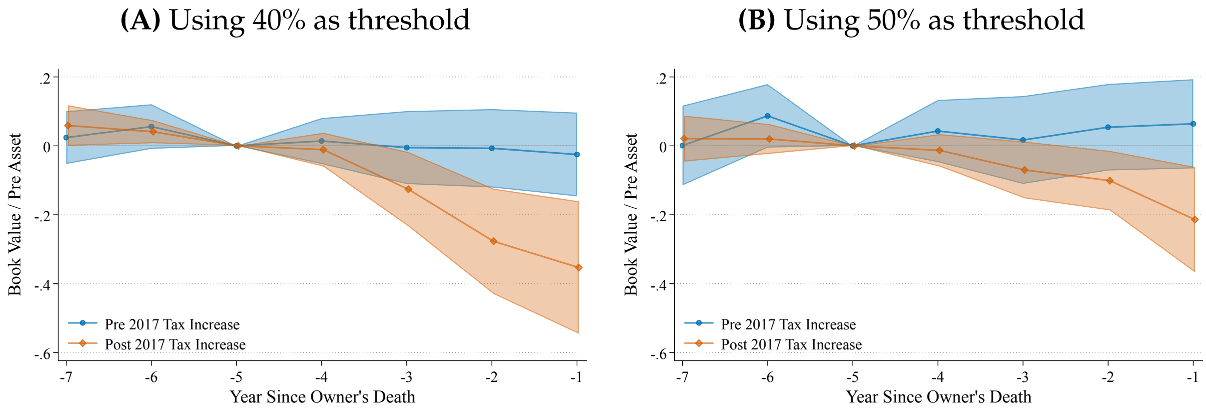
| | (1) Housing | (2) Financial | (3) Deposit | (4) OthDed | (5) Charity & Allow. |
|---|---|---|---|---|---|
| **Panel A: First stage** | | | | | |
| $\widehat{T}_i$ | 0.331 | 0.331 | 0.331 | 0.331 | 0.331 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| **Panel B: Reduced form** | | | | | |
| $\widehat{T}_i \times Post_i$ | -1,133.04 | 480.78 | 551.26 | -1,892.94 | -1,967.53 |
| | (1,049.02) | (515.33) | (252.73) | (915.88) | (375.24) |
| Pre-period mean | 61,155.57 | 11,447.78 | 4,434.14 | 31,734.51 | 7,479.61 |
| Rel. change | -0.0185 | 0.0420 | 0.1243 | -0.0596 | -0.2631 |
| | (0.0172) | (0.0450) | (0.0570) | (0.0289) | (0.0502) |
| **Panel C: Implied Elasticity** | | | | | |
| | -0.056 | 0.127 | 0.376 | -0.180 | -0.795 |
| | (0.052) | (0.136) | (0.172) | (0.087) | (0.152) |

*Notes:* This table reports item-level elasticity estimates with respect to the net-of-tax rate for the top 0.5% of the estate distribution during the 2009 tax cut. Panel A shows the first-stage effect on the change in log net-of-tax rate. Panel B reports the reduced-form effects on reported values of each item, along with the treated group's pre-period means in thousands TWD and relative changes (scaled by treated group's pre-reform mean). Panel C presents the implied elasticities using the Delta method. Robust standard errors are in parentheses. This figure is discussed in Section 4.1.

## C.2 The Effect of 2017 Reform on Closely Held Firms

We test the robustness of our results to alternative ownership thresholds. In our baseline, a firm is classified as closely held if the decedent owned more than one-third of the company. Here, we show that the findings are similar when applying stricter thresholds of 40% and 50%. Panel (A) of Figure C.1 presents book value trends using the 40% cutoff, while Panel (B) applies the 50% cutoff. In both cases, firms whose owners died after the 2017 tax increase experienced a decline in book value prior to death.

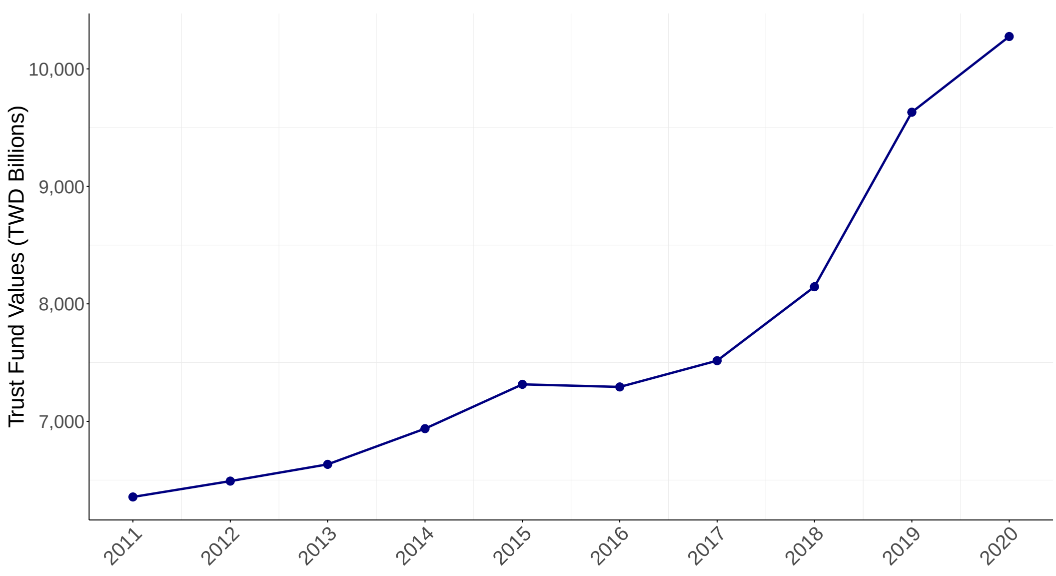
**Figure C.1:** Robustness to Alternative Thresholds for Closely Held Firms




*Notes:* This figure reports event-study estimates with 95% confidence intervals from Equation (5). Blue markers represent owners who died before the 2017 tax increase, and orange markers represent those who died after. Panel (A) defines the treated group as firms whose owners were subject to the 2017 tax increase and who at any point in the seven years before death held more than 40% of the company's ownership, while Panel (B) uses 50% as the threshold. The number of treated and control firms are 1,029 and 1,597, respectively, in Panel (A), and 748 and 1,596 in Panel (B). The outcome variable is book value, defined as assets minus liabilities, scaled by the average firm assets measured five to seven years before death. This figure is discussed in Section 4.2.2.

## C.3 Aggregate Trust Fund Sizes Over Time

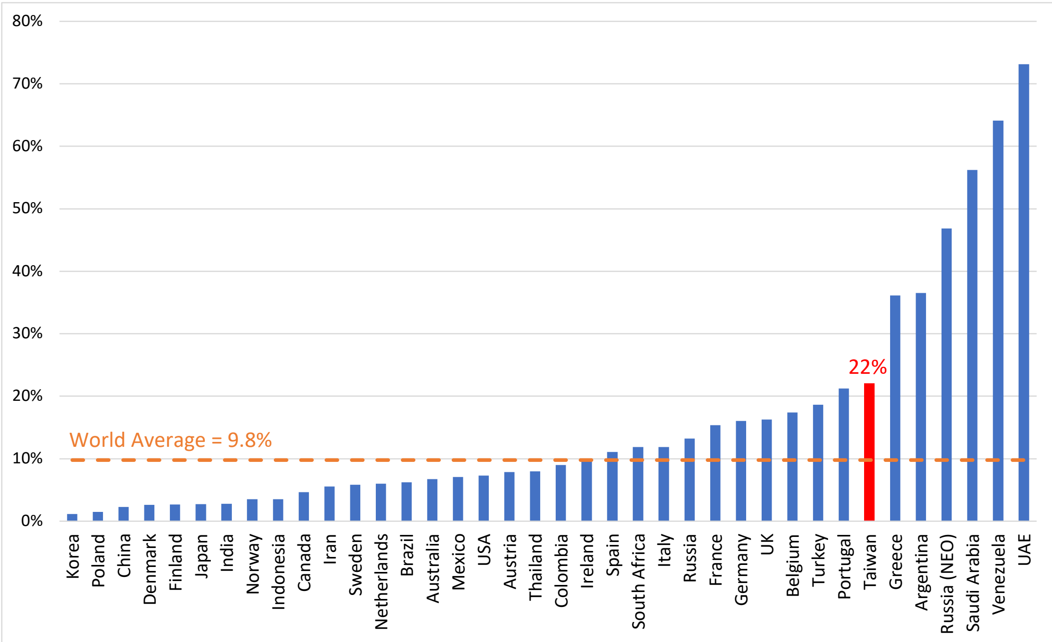
**Figure C.2:** Trust Fund Sizes in Taiwan Over Time




*Notes:* This figure shows the trust fund sizes in Taiwan between 2011 and 2020. Data source: `https://www.trust.org.tw/tw/about/annual-reports`. This figure is discussed in Section 4.2.3.

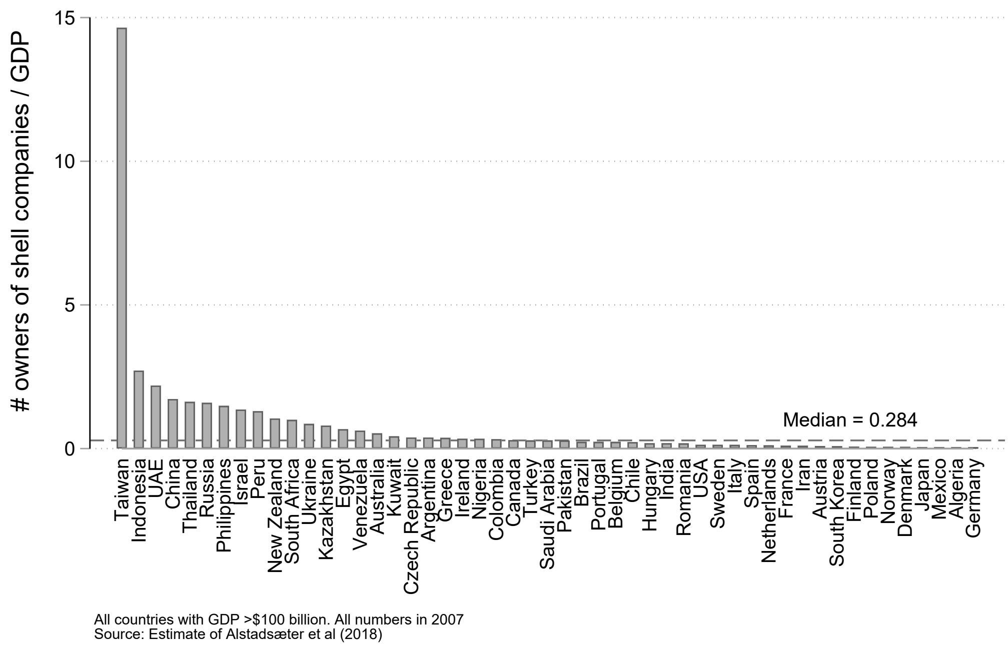
## C.4 Taiwanese Offshore Wealth as a Share of GDP

**Figure C.3:** Taiwanese Fraction of Offshore Wealth Over GDP




*Notes:* This figure plots the fraction of offshore wealth as a share of GDP. The global average is 9.8%. The data source is Alstadsæter et al. (2018). The sample is restricted to economies with GDP above $200 billion in 2007. This figure is discussed in Section 4.2.3.

**Figure C.4:** Taiwan's Share of Offshore Wealth




*Notes:* This figure shows the number of shell companies appeared in the Panama Paper Leaks over GDP. The median is 28.4%. The data source is Alstadsæter et al. (2018). The sample is restricted to economies with GDP above $100 billion in 2007. This figure is discussed in Section 4.2.3.

# D Appendix to Section 5: An Illustrative Model of Asymmetry Under Fixed Costs

## D.1 Quantifying Fixed Cost

We assume a quadratic variable cost function $\Gamma(s) = \frac{1}{2\psi}s^2$ where $\psi > 0$ governs the efficiency of the avoidance technology. The first-order condition for the intensive margin implies that sheltered wealth is linear in the tax rate: $s^* = \psi\tau$.

To quantify the fixed cost $\bar{\kappa}$, we first identify the optimal sheltered stock relative to the taxable estate, $s^*/E$, which could be informed empirically by $\tau$ and $\hat{\varepsilon}$ from the data. The elasticity is defined as:

$$\varepsilon = \frac{dE}{d(1-\tau)}\frac{1-\tau}{E} = \frac{ds}{d\tau}\frac{1-\tau}{E}$$

Substituting the F.O.C., $ds/d\tau = \psi = s^*/\tau$ allows us to express the ratio of sheltered to taxable estate as a function of the elasticity and the tax rate:

$$\frac{s^*}{E} = \frac{\tau\varepsilon}{1-\tau}$$

Using the pre-reform tax rate $\tau = 0.1$ and our estimated elasticity $\varepsilon = 2.76$, we obtain a ratio of $s^*/E = 30.7\%$. To benchmark this against the figures found in the literature, we convert this ratio to a share of total wealth ($s^*/W = 23.5\%$). This magnitude falls within the empirical range in the literature. For instance, Leenders et al. (2023) document that amnesty participants sheltered approximately 30% of their total wealth in the Netherlands, Colombia (Londoño-Vélez and Ávila Mahecha, 2024), and Scandinavia (Alstadsæter et al., 2019).

To find $\bar{\kappa}$, recall that the marginal participant enters when the fixed cost equals the utility gain from sheltering, $\bar{\kappa} = \tau s^* - \Gamma(s^*) = \frac{\tau s^*}{2}$. Expressing this relative to the taxable estate yields:

$$\frac{\bar{\kappa}}{E} = \frac{1}{2}\tau\left(\frac{s^*}{E}\right) = \frac{1}{2}\cdot 0.10 \cdot 0.307 \approx 1.5\%$$

Given the average treated estate in our sample of 244 million TWD ($\approx$ 7.6 million USD; Column (2) in Appendix Table B.3), the model implies a fixed entry cost of approximately 3.7 million TWD ($\approx$ 116,000 USD).