Behavioral Responses to Estate Taxation: Evidence from Taiwan*

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Abstract

We quantify behavioral responses to estate taxation by exploiting two large reforms in Taiwan. Using comprehensive administrative data and a difference-in-difference design, we show that the response of reported estates to the reforms is quick, persistent, and exhibits an asymmetry. We estimate elasticities of reported estates with respect to the net-of-tax rate of 2.76 (s.e. 0.39) for the tax increase and 1.31 (s.e. 0.16) for the tax cut. The asymmetry arises because liquid items such as financial assets, deposit savings, and charitable exemptions respond significantly more to a tax increase. The quick adjustments in reported estates, combined with a null effect on labor supply behavior among both decedents and heirs, suggest the responses are likely driven by tax avoidance. The observed asymmetry can be explained by tax avoidance with sunk costs: taxpayers increase avoidance during a tax increase but are less responsive to a tax cut due to previously incurred avoidance costs. We set up a tax avoidance model and derive sufficient statistics, characterized by our estimated elasticities, to assess the welfare impact of tax reforms. Our analysis shows that using the tax cut elasticity, which is attenuated due to sunk costs, would underestimate the welfare cost and overestimate the net welfare effect of a tax increase by 61%.

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1 Introduction

Although extensive research indicates increased wealth concentration (Piketty and Zucman, 2014; Saez and Zucman, 2016; Alvaredo et al., 2017), wealth-transfer taxes are becoming less popular worldwide. Since 2000, ten OECD countries have abolished their estate or inheritance taxes (OECD, 2021). Even in countries like the U.S. that still use them, the exemption threshold has increased twelvefold.[1] This decline in wealth-transfer taxes contrasts with growing concerns that inheritance fuels wealth inequality (The New York Times, 2023; The Guardian, 2024). While some argue that these taxes promote equality, others believe they create distortions and encourage avoidance and evasion, undermining their potential to reduce inequality.

Despite recent progress in theoretical work on optimal estate tax design (Farhi and Werning, 2013; Piketty and Saez, 2013), empirical research on estate taxation remains limited for several reasons. First, detailed microdata on estates are scarce and often truncated, as governments typically collect data only on estates above exemption thresholds. Second, exogenous tax variation among top estate taxpayers is rare, and when available, constructing credible empirical designs is difficult because an individual's estate is only observed upon death, that is, *once* for everyone. Thus, for those who died before a tax change, we cannot observe their post-reform estates; for those who died after the reform, we do not know what their estates would have been absent the reform. This challenge of death being an absorbing state is also encountered in other fields, such as studies on the impact of economic shocks on mortality (Sullivan and von Wachter, 2009; Finkelstein et al., 2024). Given these limitations, earlier studies on estate taxation often rely on state-level and time-series variation (Slemrod and Kopczuk, 2001; Holtz-Eakin and Marples, 2001; Joulfaian, 2006), while recent work uses bunching methods (Escobar et al., 2019; Glogowsky, 2021).

In this paper, we address these empirical challenges by leveraging detailed administrative data and rich policy variation in the Taiwanese context. We are able to link estate tax data to full-population records of wealth and income. Importantly, the estate data consistently include all deceased individuals (henceforth referred to as "decedent"), even those below the exemption threshold over time, thus resolving the truncation issue in previous studies. This consistent coverage is crucial when the tax system changes. To causally identify policy-relevant parameters, we exploit two large estate tax reforms: a tax cut in 2009 that replaced progressive rates of 2% to 50% with a flat rate of 10%, and a tax increase in 2017 that restored a progressive schedule (see Figure 1). These changes provide unprecedentedly large and rich variations in exemption thresholds and tax rates for studying responses to estate tax changes.

To estimate the effect of estate tax changes, we employ a difference-in-difference strategy comparing decedents whose pre-reform estates are above the reform cutoff (treated) to those further below in the estate distribution (control). As noted, a key challenge is that

[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).

for decedents who died after the reform, we do not observe what their estates would have been had they died before the reform, complicating the definition of treatment status. To address this, we leverage the exhaustive panel data on individuals' pre-reform wealth and demographic characteristics to construct their counterfactual absent the reform.

In our baseline estimation, we train a machine learning algorithm on the pre-reform sample to predict each decedent's treatment status, defined by whether their estate percentile falls above the reform cutoff (thus being treated by a tax change) or further below. The algorithm uses various predictors, including total wealth, wealth components, all measured four or more years before death, and demographic variables. The algorithm identifies recent pre-death total wealth as the most informative predictor in determining a decedent's treatment status. Assuming that the relationship between treatment status and predictors would have remained the same in the absence of the reform, we can predict the treatment status of those who died after the reform using their pre-reform variables, which are predetermined and unaffected by the reform. This enables us to assign a predicted treatment status to all decedents in our sample, both before and after the reform.

The validity of our difference-in-difference design relies on two assumptions. The first is the classic parallel trends assumption, which states that treated and control groups would have followed parallel trends absent the reform. The second assumption is that the relationship between treatment status and the algorithm's predictors would have remained the same in the absence of the reform. While these assumptions are not empirically testable, we provide evidence to support their validity. Our event study analysis shows no pretrend in reported estates between the treated and control groups, lending support to the parallel trends assumption. In addition, we show that both the performance and the top influential predictors of the algorithm remain stable across the pre-reform years, suggesting no structural changes in the relationship between lagged wealth components and estates at death that coincide with the reform. We also conduct placebo tests by randomly assigning reform years while applying the same prediction procedures and find no detectable treatment effect.

Leveraging the two reforms that moved in opposite directions, our analysis yields two findings. First, we observe a rapid, strong, and persistent response in reported estates to both reforms. Following the 2017 tax increase, which affected the top 0.5% of decedents, reported estates decreased by 18% after a year and remained around 20% lower three years later. The estimated elasticity of reported estates with respect to the net-of-tax rate is 2.76 (s.e. 0.39). A similarly quick and persistent response was also observed in the 2009 tax cut, which affected the top 4% of decedents. There was an 8% increase in reported estates in the first year after the reform, and the response grew to 20% three years after. The tax cut yields an estimated elasticity of reported estates with respect to the net-of-tax rate of 1.31 (s.e. 0.16). The initial response in reported estates was immediate for both reforms. However, the trajectory of the changes differed: after the tax increase, the decrease in reported estates remained stable over time, whereas following the tax cut, the increase in reported estates grew more gradually in the subsequent years.

Our second finding is the presence of heterogeneity in the *direction* and *type* of tax changes. Among the top 0.5% decedents in both reforms, the elasticity is 2.76 (s.e. 0.39) for the tax increase and 0.47 (s.e. 0.13) for the tax cut, with the former being almost six times higher. Regarding the *type* of tax changes, the 2009 tax cut increased the exemption threshold and reduced tax rates, meaning that some decedents faced a total removal of their tax liability (a repeal), while others had their tax reduced to a non-zero rate (a within-positive decrease). We find that the repeal group has an elasticity of 2.89 (s.e. 0.26), which is greater than that of the groups experiencing within-positive rate decreases. To further support these findings, we complement the analysis with a bunching approach, estimating bunching at exemption-threshold kinks, where individuals move from being untaxed to taxed, and within-positive kinks, where marginal rates change within the positive region. The results confirm that exemption-threshold kinks induce significantly larger responses than within-positive kinks.

In the second part of the paper, we investigate why responses were stronger during the tax increase by examining the anatomy of estate items. Breaking down estates into different asset and deductible items, we find that the asymmetrical responses stem from liquid items like financial assets, deposit savings, and charity and exemption responding more strongly during a tax increase than a tax cut. We further quantify the contributions from assets and deductions in each reform and show that during the 2017 tax increase, 64% of the elasticity of estates is attributed to assets and 36% to deductions. In contrast, during the 2009 tax cut, almost all of the responses can be attributed to deductions.

Are these responses tax avoidance or real changes like labor supply? We explore the channels through which decedents and their heirs adjust. On the decedents' side, we examine the effect of the reforms on their pre-death housing and stock holdings, which are the overlapping assets observed in both the estate and wealth data. We find that the drop in financial assets reported at death following the 2017 tax increase can be partially observed in the decreases in stock before death, especially in privately-listed firms, where many estate taxpayers hold significant ownership stakes and may have influence over the valuation of their shares. In addition, we show the reforms had no effect on labor income, suggesting that decedents did not adjust their labor supply in response to the tax changes.

On the heirs' side, we examine the effects of tax changes on their inheritance and labor supply behavior. The impact on heirs depends on how decedents adjust: *a priori*, if decedents adjust their estates mainly through tax avoidance, the true value heirs receive remains unchanged, and heirs' behavior is likely unaffected. To test this, we identify the decedents' children and track their behavior before and after their parents' death using two sources of variation: (i) the timing and level of inheritance, which is commonly used in the past literature, and (ii) the change in inheritance induced by the estate tax reforms, a source of quasi-experimental variation that has not been previously explored.

Using variation in timing and level of inheritance, we observe around 2% decrease in labor supply on the extensive margin after the decedent's death, with no effect on the intensive margin. This aligns with the past literature that reports a reduction in labor supply on the extensive margin, with less conclusive results on the intensive margin. However, when leveraging variation induced by the tax reforms, we find no difference between those whose parents died before and after the tax changes. The lack of adjustment in their labor supply suggests that the true value of inherited estates remained unchanged.

These findings, along with the quick changes in the reported estate that responded within a year after the reforms, suggest that the decedents' responses we capture are likely driven by tax avoidance rather than real changes. The asymmetrical responses between the tax increase and the tax cut also support this interpretation. As individuals engage in tax planning during their lifetime, they incur avoidance costs such as setting up offshore accounts, establishing trust funds, or paying professional tax planners. When taxes increase, they may intensify their avoidance. However, when taxes decrease, they may not reduce their avoidance by as much since they have already invested in these avoidance instruments, and reversing prior decisions can incur additional fees.

In the last part of the paper, we use our empirical results to conduct welfare evaluation in the context of a tax avoidance model based on Chetty (2009). In this framework, an individual with initial wealth decides on consumption and estate sheltering, which incurs a sheltering cost. The model can be extended into a two-period framework that rationalizes the asymmetrical responses between a tax increase and a tax cut: in the first period, individuals make decisions as in the static framework. In the second period, if the tax rate changes, individuals can adjust their sheltering, but the marginal cost of reducing previously sheltered assets in the first period is higher than that of increasing them, due to potential penalties or fees such as dissolving trust funds or repatriating offshore wealth. As a result, the model predicts a stronger response to tax increases than tax cuts.

Based on the framework, the welfare effect is characterized by the elasticity of reported estates, as informed by our reduced-form estimates. Our empirical findings show that responses to tax increases and cuts differ both in magnitude and in timing: during the 2017 tax increase, where no sunk costs were present, the response was immediate and stable over time, yielding an elasticity estimate that accurately reflects the welfare effect of the tax increase. In contrast, the 2009 tax cut, introduced when taxpayers had already incurred sunk costs from prior tax planning, showed an immediate response that gradually increased over time as taxpayers adjusted incrementally, resulting in an attenuated elasticity.

This distinction has important welfare implications. For a tax increase, using the elasticity estimated from the 2017 tax increase (without sunk costs) will provide an accurate assessment of the welfare effect. However, if the government mistakenly applies the elasticity estimated from the 2009 tax cut (with sunk costs) to evaluate the welfare effect of a tax increase, it will overestimate the welfare effect by 61% due to underestimating the welfare cost. For a tax cut, both short-run (with sunk costs) and long-run (without sunk costs) elasticities are relevant, as the cumulative welfare impact depends on how quickly individuals recover from these sunk costs.

In summary, the observed asymmetry in behavioral responses to estate taxation has nontrivial welfare implications and may extend to other taxes, such as corporate income taxation or wealth taxation, where tax avoidance involves similar fixed costs. For example, setting up offshore operations in tax havens often requires fixed costs, which could result in a comparable asymmetry in response.

Related Literature and Contributions. Our paper makes several contributions to the literature on behavioral responses to estate and inheritance taxation, which can be divided into two waves: an earlier wave in the early 2000s that used state-level or time-series variation on U.S. data (Holtz-Eakin and Marples, 2001; Slemrod and Kopczuk, 2001; Joulfaian, 2006), and a more recent wave using European microdata and modern empirical methods such as bunching and difference-in-difference approaches (Goupille-Lebret and Infante, 2018; Mas-Montserrat, 2019; Escobar et al., 2019; Glogowsky, 2021).

First, we leverage exceptionally large and rich tax variation. The two reforms allow us to study the heterogeneity in responses across types (repeal vs. within-positive rate changes) and directions (increase vs. decrease) of tax changes. To our knowledge, we are the first to distinguish between an increase and a decrease in estate tax rates and document the resulting asymmetrical responses in the estate and wealth taxation literature. While asymmetry between tax increases and cuts has been observed in the context of value-added taxes (Benzarti et al., 2020, 2024; Benzarti, 2024), the context of estate taxes presents a different scenario. We attribute the asymmetry to sunk costs in tax avoidance strategies: once individuals invest in tax planning that incurs avoidance costs, it is difficult to reverse when tax rates fall.

Second, we propose a novel methodological approach by developing a difference-in-difference design suited to the context of estate data, where having only a single observation of each individual's estate complicates the definition of treatment intensity. This allows us to go beyond prior studies that rely on bunching (Goupille-Lebret and Infante, 2018; Escobar, 2017; Glogowsky, 2021) or regression discontinuity designs (Escobar et al., 2019), which may only capture local effects, particularly when adjustment frictions or the lumpy nature of estates are present (Kleven, 2021). Similar prediction methods have been applied in other contexts. For example, Jakobsen et al. (2024) apply them to predict wealth following the repeal of the Swedish wealth tax. Building on these techniques, we leverage detailed wealth and demographic data to construct counterfactual estates in the absence of tax changes, enabling us to capture a broader effect of estate responses to tax changes.

Third, the combination of large reforms and detailed data allows us to go beyond estimating the aggregate response of estates by conducting an in-depth decomposition and estimating the elasticity of each asset type separately. In addition, by comparing asset values during a person's lifetime with values reported at death for overlapping items, we capture a fuller picture of tax avoidance adjustments. These findings have significant implications for tax policy design targeting the very wealthy. Identifying which assets are most adjusted in response to tax changes helps policymakers pinpoint areas where tax avoidance is prevalent and underscores the need for stronger enforcement measures, such as enhanced third-party reporting and international collaboration to track offshore wealth.

Our paper also extends the literature on the effects of inheritance receipts on heirs' labor supply, often referred to as the Carnegie effect (Holtz-Eakin et al., 1993; Elinder et al., 2012; Bø et al., 2019; Nekoei and Seim, 2023). Prior studies usually rely on variation in the timing and level of inheritance, and find that receiving an inheritance reduces labor supply on the extensive margin, with mixed evidence on the intensive margin. We contribute to this literature by exploiting a novel source of variation: changes in inheritance induced by estate tax reforms. Unlike the timing of a parental death, which heirs might expect and prepare for, quasi-experimental tax-induced changes in inheritance reduce the potential for anticipatory behavior. Comparing heirs whose parents died before and after the tax changes, we find no significant effect on heirs' labor supply, despite substantial differences in the inheritances received due to the reforms. We interpret our results as evidence that the observed changes in estates are driven by tax avoidance rather than real behaviors.

The rest of this paper is organized as follows. Section 2 describes the institutional context of estate taxes in Taiwan and the data. Section 3 presents the empirical strategies and results of the effect of estate taxation on decedents. Section 4 investigates the mechanism behind the responses. In Section 5, we build a model that connects our empirical findings to aggregate welfare impacts. Section 6 concludes.

2 Institutional Context & Data

2.1 Institutional Context

Estate taxation has a long history in Taiwan that dates back to the 1940s and has undergone several reforms over the years. This section describes the tax schedule during the period covered by our data, from 2004 to 2019. Like in other developed countries, Taiwan's estate taxes primarily affect individuals at the 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%. Estate tax revenues account for around 1% of Taiwan's total tax revenues, which is higher than the 0.5% average in other developed countries (OECD, 2021).

Tax Base. The Taiwanese estate taxation is levied on the amount of the estate left by a decedent. This system is also adopted by the U.K., the U.S., and South Korea, and contrasts with the inheritance tax system used in most European countries, where the tax is levied on the amount inherited by an heir.

An estate is defined as assets minus deductions. Assets include housing, financial assets, and deposit savings. For Taiwanese residents, the estate tax covers worldwide assets. For non-residents, it applies only to assets located in Taiwan.

Housing assets include land and houses. Financial assets include publicly-listed and privately-listed stock, bonds, futures, trusts, insurance, and other financial instruments. Deposit savings are savings in banks.

Deductions include charity/exemptions and other deductibles. Charitable contributions/exemptions include donations to nonprofit organizations, spousal exemption claimed by the living spouse according to the Civil Law, and estate taxes paid within the last five years. Other deductibles include 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 typically adjusted every two years and serve as the basis for property taxes. These prices are generally lower than market prices. Ownership of these assets is third-party-reported and can be verified from the property tax records.

Financial assets include publicly- and privately-listed stock, and other self-reported assets. The ownership and quantities of publicly- and privately-listed stock are third-party-reported, as they can be verified from the dividend tax records. Publicly-listed stock is valued at the market price on the date of death, while privately-listed stock is assessed based on the company's net assets on the date of death. Privately-listed stock is more prone to misreporting, as companies do not usually keep daily balance sheets, and many estate taxpayers closely hold these firms to the extent that they have the power to adjust their valuation. Aside from stock, this category also includes self-reported assets like trusts, insurance, farm output, credits, cash, jewelry, and offshore assets. The government has limited power to detect offshore wealth.

Deposit savings are self-reported but can be cross-verified through third-party-reported interest income information. Individuals must provide relevant bank statements when filing. Appendix Table A.1 presents the details.

There are two types of penalties for underreporting. For amounts owed under 35K TWD (approximately 1,200 USD), the individual must pay the tax with interest but incurs no fine. For amounts exceeding 35K TWD, fines range from one to two times the tax owed. In cases of tax fraud, penalties can include fines of one to three times the owed tax and criminal charges may be pursued.

Estate Tax Schedules. Figure 1 illustrates the estate tax schedule in Taiwan between 2004 and 2019. Between 2004 and 2005, it was a progressive schedule where the MTRs ranged between 2% and 50%. The exemption threshold was 7 million TWD (around 230K 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 23rd, 2009. First, the exemption threshold was raised by approximately 50%, from 7.79 million TWD to 12 million TWD (equivalent to around 260K USD to 400K USD). Second, the progressive tax rates that were previously between 11% and 50% were replaced with a flat rate of 10%. The announcement was rather unanticipated since the government only took office in May 2008, and the estate tax reform was not part of the political discourse during the election.

The reform aimed to achieve several objectives. First, the tax cut was expected to attract wealthy individuals to repatriate offshore investments. It was reported that on average 1.2

trillion TWD (40 billion USD) of funds was sent offshore per year before 2008. The government believed that the reform could stop the funds from going overseas, especially during the unstable financial outlook of the time. Second, the lower tax rate was intended to discourage tax planning. Third, the higher exemption threshold was designed to benefit small and medium-sized family businesses in terms of succession planning. However, the significant tax cut sparked protests from the main opposition party, the Democratic Progressive Party (DPP), at the time, as they claimed it would exacerbate inequality.

In the 2016 presidential election, the DPP candidate proposed raising estate taxes to reduce inequality and fund the Long-Term Care Services Act 2.0. After winning the election, the government introduced two new MTRs of 15% and 20% for the top 0.5% of estates, effective May 17th, 2017.

[Figure 1 here]

Gift Tax Schedules. Taiwan also imposes a gift tax, which is levied on the annual amount of gifts made by a donor. The gift tax schedule closely follows the estate tax schedule.[2] Appendix Figure A.1 shows the gift tax schedule over time. Between 2004 and 2005, the gift tax had a progressive schedule with an exemption threshold set at 1 million TWD (approximately 30K USD). In 2006, the exemption threshold was adjusted for inflation, increasing to 1.11 million TWD, while the MTRs remained unchanged. In 2009, the exemption threshold was raised to 2.2 million TWD, and the progressive tax rates were replaced by a flat 10% rate. In 2017, the flat rate was reverted to a progressive schedule, where two new MTRs of 15% and 20% were introduced.

To deter donors from using gifting to avoid estate taxes, Taiwan regulates that gifts given within two years before death are included in the estate tax base. The values of these gifts are reassessed based on the timing of death. If a gift tax was previously paid on these gifts, the amount can be deducted from the estate tax liability. However, if there is a difference in the values of the gifts between the time of the gift and the time of death, the donor must pay the difference in their estate tax liability.

Our paper examines the effect of these reforms on the estate tax base, ensuring that near-death gifts are included to avoid potential tax leakage. This setting provides two advantages. First, it is policy relevant to countries that regulate near-death gifting such as the U.S. and the U.K.[3] Second, the two-year rule ensures that near-death gifts are included, so the effects we capture would not be dominated by the channel of gift leakages but represent meaningful measures that have important policy implications.

2.2 Data

We utilize the following datasets provided by the Fiscal Information Agency in Taiwan. While Taiwan does not have a wealth tax, wealth information could be derived from other

[2] In the U.S., the estate and gift tax schedules are also tightly linked and subject to the same rate.

[3] In the U.S., the gross estate contains gifts made within three years of death (I.R.C. §2035(a)(1)). In the U.K., gifts given within seven years of death are counted towards the estate tax base, although differential tax rates would be applied depending on the timing (I.T.A. 1984 §113A).

tax sources. The overlapping assets observed both at death (from estate data) and during one's lifetime (from property and dividend income tax data) are housing, publicly-listed, and privately-listed stock.

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, cause of death, reported estates, assets, and deductions. In the subfiles of the records, assets are stored by housing, financial assets, and deposit savings.

Property Tax (2004-2019). The property tax dataset provides 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.

Dividend Income Tax (2004-2019). The dividend income tax data records ownership of shares in publicly and privately listed firms. It includes individual IDs, firm IDs, and the number of shares held.

Individual Income Tax (2004-2021). The income tax records include information on various income types, such as interest, dividend, and labor 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 capitalized 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, while privately-listed stock is valued based on the net assets reported on the company's balance sheet from corporate income tax data. For deposit savings, we capitalize interest income following the method described in Saez and Zucman (2016).

Sample Construction. To construct our sample, we start by extracting individuals who died above the age of 30 between 2004 and 2019. We then merge their estate records and lifetime information from the property, dividend, and income tax data.

2.3 Graphical Evidence on Attentiveness to Exemption Thresholds

To illustrate that individuals pay attention to the exemption threshold and behaviorally respond, we present motivating graphical evidence on the dynamic bunching around the exemption thresholds.

In Figure 2, we bin individuals into 200K TWD intervals and trace the distribution of estates around the exemption thresholds 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 grey dashed line. In 2006, the exemption threshold was adjusted by 10% to account for inflation, moving from the grey dash line to the red dash line (7.79 million TWD). As shown in Panel B, within a year, the excess mass at the old kink disappears and an excess mass appears at the new exemption threshold. The response is persistent and strong through the end of 2008.

[Figure 2 here]

A similar pattern happened when the 2009 reform increased the exemption threshold substantially by 150% (to 12 million TWD). In Panel C, the excess mass shifted from the original threshold (the grey dashed line) to the new threshold (the red dashed line) within a year. The bunching at the new threshold becomes clearer and stronger over time. To highlight the persistence of the response, Panel D shows the distribution for all subsequent years. The frequencies increase over time as the population grows, and the excess mass formed in response to the 2009 reform remains evident until the last year of our data. This indicates that individuals are highly attentive to the threshold and adjust their estate reporting accordingly.

3 Empirical Analysis

3.1 Difference-in-difference Approach

Goal and Challenges. Our goal is to causally estimate the effect of estate tax changes on reported estates by using the two reforms as sources of variation and setting up a difference-in-difference design comparing those whose pre-reform estates are above the reform cutoff (treated) and those below (control). Define $\text{EstatePercentile}_{t(i)}(\tau)$ the estate reported by decedent i who dies in year t under the tax schedule τ. τ is a vector of parameters that defines the tax schedule and can be the pre-reform tax schedule τ_0 or the post-reform τ_1. Treatment status is defined as $\text{Treated}_i = \mathbb{1}[\text{EstatePercentile}_{t(i)}(\tau_\mathbf{0}) \geq C]$ where C is the reform cut-off.

For individuals who died *before* the reform, we directly observe $\text{EstatePercentile}_{t(i)}(\tau_0)$ from the data, making it straightforward to assign them a treatment status. However, for individuals who died *after* the reform, we face a challenge. We only observe their estates under the post-reform tax schedule, $\text{EstatePercentile}_{t(i)}(\tau_1)$. This creates a problem because using $\text{EstatePercentile}_{t(i)}(\tau_1)$ to define the treatment status would create endogeneity, as the estates under the new system may already reflect behavioral adjustments in response to the reform.

Proposed Prediction Solution: The Idea. To address the above challenge, we propose a method to predict each individual's treatment status, specifically whether their estate percentile under the pre-reform tax schedule would have been above the reform cutoff, by leveraging panel data on individuals' pre-death wealth, income, and demographics information.

Using only those who died *before* the reform, we model the relationship between treatment status and their pre-death information as follows:

$$\mathbb{1}[\text{EstatePercentile}_{t(i)}(\tau_0) \geq C] = f(\mathbf{W_{t(i)-k}}, \mathbf{W_{t(i)-k-1}}, ..., \mathbf{X_i}) + \epsilon$$

where $f(.)$ is a function modeling one's treatment status to a set of predictors. The vector $\mathbf{W_{t(i)-k}}$ is a vector of wealth 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. ϵ is a prediction error.

For all decedents, regardless of dying before or after the reform, we observe their $\mathbf{W_{t(i)-k}}, \mathbf{W_{t(i)-k-1}}, ..., \mathbf{X_i}$, which are all predetermined before the reform and not influenced by 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. In other words, the predicted treatment status is $\widehat{\text{Treated}}_i = \hat{f}(\mathbf{W_{t(i)-k}}, \mathbf{W_{t(i)-k-1}}, ..., \mathbf{X_i})$.

Once the predicted treatment status is constructed, we can estimate a simple difference-in-difference regression:

$$y_i = \alpha_0 + \alpha_1 \widehat{\text{Treated}}_i \times \text{Post}_i + \alpha_2 \widehat{\text{Treated}}_i + \alpha_3 \text{Post}_i + u_i$$

where y_i is the realized estate of decedent i. $\widehat{\text{Treated}}_i$ is the predicted treatment status of i. $Post_i$ is 1 if i dies after the reform.

Proposed Prediction Solution: The Implementation. We outline the implementation using the 2017 reform as an example below, though the same approach applies to the 2009 reform. In the previous paragraphs, we described the prediction concept in a context where the classification of treatment status is binary for simplicity. In practice, we classify decedents into multiclasses based on their estate percentile: (i) under 90th percentile; (ii) between the 90th and 96th percentiles; (iii) between the 96th and 99.5th percentiles, and (iv) above the 99.5 percentile.

Next, we train a random forest algorithm on the pre-reform decedents who died between 2014 and 2016. In other words, the $f(.)$ 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 a set of predictors, such as total values of wealth and different wealth component values, all measured four to ten years before death, and demographic variables.[4] Appendix B.1 provides further details on the prediction procedures. The algorithm identifies recent years' total wealth values, as the most influential predictors (see Appendix Figure B.2), and model performance is presented in Appendix Table B.2.

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.

[4]We pick years more than four years far back to ensure that when we predict the estate percentiles of those who died after the reform, all predictors predetermined and unaffected by the reform. For example, for individuals who died in 2019 (the latest year in the study), all predictors are measured four to ten years before death, which would be years before 2015.

Event Study Specification. We allocate decedents into two-quarter bins (180 days) based on their death dates and estimate the following event-study specification:

$$\begin{aligned} y_i = \alpha_0 + \sum_{k \neq \text{BasePeriod}} \alpha_{1k} \cdot \widehat{\text{Treated}}_i \times \mathbb{1}[\text{DeathPeriod}_i = k] \\ + \alpha_2 \cdot \widehat{\text{Treated}}_i + \sum_{k \neq \text{BasePeriod}} \alpha_3 \cdot \mathbb{1}[\text{DeathPeriod}_i = k] + u_i \end{aligned} \tag{1}$$

where y_i represents the estate outcome of decedent i. Given that the 2017 tax reform affected the top 0.5%, we define decedents as treated if they are predicted to be in the class above the 99.5 percentile and as control if they are predicted to be in the class between the 90th and 96th percentiles. Similarly, for the 2009 reform affecting the top 4%, we define the treated as those predicted to be in the class above the 96th percentile, with the control group defined in the same way as the 2017 reform. DeathPeriod_i represents the death period of decedent i, grouped into 180-day intervals. The base period, denoted as 'BasePeriod', is defined as the 180 days immediately before each reform.

The identifying assumptions are as follows. The first assumption is the classic parallel trend assumption in a diff-in-diff design. That is, in the absence of reform, the trends of the outcomes of the treated and control would have been parallel. A threat to this is anticipatory behavior; if individuals anticipated the tax changes and adjusted their behavior accordingly, it could bias our estimates. To alleviate the concern, we perform robustness checks in Section 3.5 by assuming the reform happened a year earlier in both reforms and show they yield similar results. Furthermore, as shown in the event study graphs in the later section, there is no pretrend in the reported estates between the control and treated groups, supporting the validity of the parallel trends assumption.

The second assumption, specific to our prediction algorithm, is that the performance of the algorithm is the same before and after the reform in the absence of reform. Although this assumption is not directly testable, we provide supporting evidence in Section 3.5 by demonstrating that both the algorithm's performance and the top important predictors remain stable across the pre-reform years. This ensures there is no structural break in the prediction model. In addition, we conduct placebo tests by randomly assigning reform years and applying the same prediction procedures to the samples and detect no treatment effect.

Elasticity Identification. We would like to know the elasticity of the reported 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 reported estate would change in %.

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

The elasticity is the effect of the reform on reported 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:

$$\text{First stage:} \quad \log\left(1-\tau_i^{\text{Post}}\right)-\log\left(1-\tau_i^{\text{Pre}}\right)=\beta_0+\beta_1\widehat{\text{Treated}}_i+v_i \tag{2}$$

$$\text{Reduced form:} \quad \log \text{Estate}_i=\alpha_0+\alpha_1\widehat{\text{Treated}}_i\times \text{Post}_i+\alpha_2\widehat{\text{Treated}}_i+\alpha_3\text{Post}_i+u_i \tag{3}$$

In our baseline estimation of the first stage, we use those who died before the reform and construct the mechanical marginal tax rate change on the left-hand side by assigning the old and new marginal tax rates based on their realized reported estates. The coefficient β_1 would imply how much more the treated are exposed to the tax rate changes in the absence of the behavioral effect. In the reduced form, we use decedents from both periods to estimate the treatment effect of the reform on the reported estates. Then, we take the ratio of the two stages to arrive at the implied elasticity. Under the assumption that the prediction error of the algorithm is the same pre- and post-reform absent the reform as stated previously, the prediction errors in the predicted treatment status in the two stages offset each other, yielding an unbiased estimated elasticity.

Using pre-reform decedents to estimate the first stage offers the following advantages. First, it avoids behavioral adjustments. Decedents in the post-reform period may adjust their behavior in response to the reform, potentially underestimating the true mechanical tax rate change. Second, as we later augment the diff-in-diff approach with a bunching approach, using pre-reform decedents to estimate the first stage enhances the comparability between the two approaches. In the bunching method, the tax rate difference is the mechanical difference derived from the distance between one's location and the tax discontinuity, which is more similar to the mechanical tax rate difference using the pre-reform decedents constructed here. In Section 3.5, as a robustness check, we also present the elasticity results using the average tax rate.

3.2 Difference-in-difference 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). In Panel A of Figure 3, we plot the estimated coefficients and their 95% confidence intervals from the specification (1). The vertical dashed line marks the timing of the reform, which occurred in May 2017. The event study reveals no statistically significant differences in the pre-trends of the treated and control groups, lending support to the parallel assumption. After the tax increase, the reported estates started to decline quickly. They dropped by 18% by the end of 2017 and oscillated around 20% until the end of 2019.

[Figure 3 here]

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 tax rates compared to the control. The reduced form estimates indicate that they decreased their reported 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, reported estates decreased by 2.76%.

[Table 1 here]

2009 Tax Cut. We apply the same empirical strategy to the 2009 tax cut, which impacted the top 4%. We define our 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 so that we can consistently compare the two reforms.

Panel B in Figure 3 presents the event study of the 2009 tax cut, estimated using the same Specification (1). The vertical dashed line represents the timing of the reform in January 2009. Consistent with the 2017 reform, there are no statistically significant differences in the pretrend of the treated and control groups, supporting the parallel trend assumption. Following the reform, the treated group (top 4%) had a quick increase in their estates, rising by around 8% by the end of 2009. The response continued to grow to nearly 20% by the end of 2011.

Column (2) of Table 1 presents the elasticity estimates for the 2009 reform where the treated group is the top 4%. On average, the treated faced a 10% increase in their net-of-tax rates, and they increased their reported estates by 13.1%. This yields an elasticity of 1.31 (s.e. 0.16). In other words, when the net-of-tax rate increases by 1%, there is a 1.31% increase in the reported estates. Comparing the trends between the 2017 tax increase and the 2009 tax cut, we observe that while responses were immediate in both cases, the response to the 2017 tax increase was stable over time, whereas the response to the 2009 tax cut was more gradual. We will return to this point later when we explore the underlying mechanism behind the asymmetry between the two reforms

Next, we investigate the heterogeneity in the responses across different types and levels of the tax changes induced by the 2009 reform, which raised the exemption threshold from 7.79 to 12 million TWD and replaced the progressive MTRs that previously ranged between 11% and 50% with a flat rate of 10% (s.e.e Figure 1). Due to the increase in the exemption threshold, some individuals have their tax liabilities entirely removed (a repeal), while others have their tax rates lowered to a non-zero rate (a within-positive rate change).

To examine differential responses to these varying types of tax changes, repeal v.s. within-positive rate change, we further split the top 4% treated group into three subgroups: top 4%-2% (repeal), top 2%-0.5%, and top 0.5%. The first group is the group that experienced the repeal, whereas the latter two faced a within-positive rate change. The top 0.5% also serves as a reference group for consistent comparisons with the treated group of the 2017 tax increase. We estimate the same Specification (1) separately for these subgroups where the control group is always those between the 90th-96th percentiles.

Panel C illustrates the evolution of the reported estates of these three subgroups. The estimates of the repeal group are in grey, those of the top 2%-0.5% are in blue, and those of the top 0.5% are in orange. Again, we find no statistical difference in the pre-trends of these groups and the control. In terms of responsiveness, both the repeal and the top 2%-0.5% show rapid increases in their reported 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 fewer observations. While there is an upward trend in their reported estates in the first two years after the reform, the estimates are not statistically significant. However, starting from 2011, their responses show a salient increase and reach around 30% increase by the end of 2011.

When we consider the magnitude of the tax rate changes they faced (the first stage), the discrepancy across them becomes more salient. Column (3) through Column (5) of Table 1 present the results of the repeal (top 4%-2%) and the within-positive decrease (top 2%-0.5% and top 0.5%), respectively. The three groups experienced very different net-of-tax rate changes, with the repeal group facing the smallest change (4.5%) and the top 0.5% experiencing the largest change (33.1%). In terms of their reduced forms, the 95% confidence intervals for these groups come close. On average, the repeal group increased their reported estates by 13%, the top 2%-0.5% group by 20.4%, and the top 0.5% group by 15.4%. These differences yield different levels of elasticity: the repeal group has an elasticity of 2.89 (s.e. 0.27), while the within-positive rate change groups have elasticities of 1.59 (s.e. 0.17) for the top 2%-0.5% and 0.47 (s.e. 0.13) for the top 0.5%, respectively.

Summary. There are two key observations from Table 1. First, within the 2009 tax cut, there is a heterogeneous response between those who faced a repeal versus those who faced a within-positive rate change, with the repeal having a much larger elasticity. However, a limitation of this comparison is that these groups differ not only in the type of tax change but also in their estate levels. To address this concern and reinforce our argument that the type of tax change drives the differential responses, we turn to an alternative bunching approach in Subsection 3.3. This method allows us to compare kinks of different types that are close to each other in the estate distribution, alleviating concerns about differences in estate levels.

Second, across reforms, there is an asymmetric elasticity for the top 0.5%. The elasticity resulting from the tax increase is almost six times greater than that of the tax cut. Although asymmetric tax incidence has been documented in other contexts (Benzarti et al., 2020, 2024), to our knowledge, it has never been shown in the estate tax literature. In Section 3.4, we compare our elasticity estimates to those in the existing literature and delve deeper into the mechanism of this asymmetry in Section 4.

3.3 Alternative Bunching Approach

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 seven-order polynomial and bootstrap the standard errors à la Chetty et al. (2011).

3.3.1 Results

Exemption kinks. First, we examine the exemption kinks and present the results in Figure 4. 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 with the magnitude of the tax rate change at exemption thresholds, suggesting that there are factors beyond the magnitude of tax rate influencing taxpayer behavior. We discuss this further in Section 3.4.

[Figure 4 here]

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.[5] 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.

3.4 Discussion: Elasticities

Comparison Diff-in-diff v.s. Bunching. Comparing the estimates from the two approaches, we observe two similarities. First, both methods find quick and persistent responses. In the diff-in-diff results, the event study graphs show the responses began within the first year after the reform and persisted through the last year of the studied period following both reforms. Consistently, as shown earlier in Section 2.3, the bunching results imply that when the exemption threshold shifted, the excess mass also followed quickly and became more salient over time.

[5] 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.

The second similarity is that both approaches indicate that responses to exemption thresholds are bigger than responses to within-positive rate changes. In the diff-in-diff results, the repeal group has an elasticity of 2.89 (s.e. 0.27), which is larger than those of the group who face a decrease to a non-zero rate. In the bunching results, we only detect bunching at the exemption kinks, but not in any other within-positive kinks, suggesting that exemption kinks induce a much stronger response. The elasticity estimates from exemption kinks are sensitive to the magnitude of the tax rate changes used, though. The one estimated from a 0%-10% tax rate change is 0.45 (s.e. 0.12) and the one from a 0%-2% is 2.55 (s.e. 0.66). This suggests that decedents are responding to the exemption kinks not solely because of the tax rate difference, but because there are additional factors beyond the tax rate change. Such a phenomenon aligns with the recent findings in the wealth taxation literature. For example, Garbinti et al. (2023) find that wealth taxpayers respond strongly to kinks associated with information discontinuities (e.g., exemption thresholds or reporting requirement cutoffs) but do not react to within-positive kinks that only affect tax rates. Similarly, Londoño-Vélez and Ávila Mahecha (2024) estimate a larger elasticity using an exemption-notch than a within-positive-notch.

Comparison with Past Literature. Where do our estimates stand in the literature? Table 2 summarizes studies estimating the effects of estate or inheritance tax changes on reported estates or inheritances. We categorize these studies based on the methodologies employed.

Panel A presents estimates using a diff-in-diff method with a repeal variation. Mas-Montserrat (2019) exploits the Catalan inheritance tax repeal on close heirs, comparing estates given to close heirs versus distant heirs, and reports an elasticity of 2.51 (s.e. 0.33), which is close to our repeal estimate of 2.89 (s.e. 0.26).

[Table 2 here]

Panel B reports our estimates using a within-positive rate variation in a diff-in-diff setting. To our knowledge, no paper has used a diff-in-diff estimation with a within-positive rate variation to identify the elasticity of estates with respect to the net-of-tax rate. Our contribution to the literature includes not only adding estimates using such an approach but also examining the direction of the rate change by splitting reforms into tax increases and decreases.

Panel C presents the estimates using bunching at the exemption threshold. The only existing study in this area is Escobar et al. (2019), who examines 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. Our bunching estimates from an exemption kink vary between 0.57 (s.e. 0.10) and 3.17 (s.e. 0.51). Although their estimate lies within the range of ours, the mixed results suggest that individuals do not respond proportionally to the magnitude of the tax change at exemption thresholds, as there are factors beyond the rate change itself that influence their behavior.

Panel D displays the estimates from within-positive kinks, the results are also 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 these 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.

Panel E includes one estimate using an RDD from Escobar (2017) at 0.88 (s.e. 0.36). Panel F presents the estimates from an older wave of literature using time-series variation and U.S. data. The results are mixed, with Joulfaian (2006) reporting an estimate of 0.14 (s.e. 0.05) that is statistically significant and Slemrod and Kopczuk (2001) having noisy estimates spanning from -0.11 to 0.09 that are statistically insignificant.

To summarize, our estimates contribute to the literature in the following ways. As shown, existing studies report a wide range of elasticity estimates, from statistically insignificant values (Slemrod and Kopczuk, 2001) to as high as 2.51 (Mas-Montserrat, 2019). A possible explanation for these differing findings lies in the types of variation and methods used. We show this is the case by estimating responses using both variations and methods within the same context, helping to reconcile the differences in elasticity estimates in the literature.

In addition, we offer estimates using new variations not previously available in the literature. Specifically, by distinguishing between responses to tax increases and cuts, we show asymmetric responses. We explore this novel aspect and investigate its underlying mechanism in Section 4.

3.5 Robustness Checks

Thus far, we have presented the main results of the behavioral responses in reported estates to estate tax changes. Here we present robustness checks of our main diff-in-diff design.

Stability of Algorithm. One of the identifying assumptions is that the performance of the prediction algorithm is similar before and after the reform. This cannot be tested directly, but we can study pre-reform features of the prediction algorithm in the spirit as we usually test parallel-trend assumptions using pre-reform data.

For the 2017 reform, Appendix Figure B.2 displays the top 25 important predictors used in the algorithm, split by period. In both panels, the same predictors emerge as most influential, with the top five being total wealth values four to six years prior and land values four years before death. Although the order of these predictors varies slightly, they consistently remain the top influential factors. To assess the stability of the algorithm's predictive performance, Appendix Table B.2 presents results by year. Columns (1) and (2) show the overall precision and recall rates, while Columns (3) through (8) provide performance by specific years. Overall, performance across groups shows little variation, supporting the algorithm's stability. Similarly, Appendix Figure B.3 and Table B.3 present the stability of the top predictors and prediction performance for the 2009 reform, respectively.

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 B.4 describes the procedures using 2012, 2013, 2014, and 2015 as placebo years. Appendix Table B.5 presents the results. We find no treatment effect in any of the placebo years.

Alternative Control Group. Since the 2009 reform affected the top 4%, in both our baseline estimations of the two reforms, we use those whose predicted estate is between the 90th and 96th percentiles as the control so that we can consistently compare the two reforms. The choice of the control group involves a tradeoff: selecting a control group whose reported estate evolves similarly to the treated group (closer to the top percentiles) while avoiding contamination by the reform (lower down in the percentiles).

In Appendix Table B.4, we show the elasticity estimates when using alternative control groups. For the 2017 reform, the elasticity estimates do not differ much when using P90-P93, P93-P96, and P96-P98. The estimate becomes insignificant when using the immediately below group P98-P99.5 as the control. This is because the group also decreased their reported estates after the tax increase, potentially because they behaved in a forward-looking manner and chose not to cross the threshold, which is consistent with the findings in 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 individuals expected the government to change the estate tax schedule, they could have adjusted their behavior prior to the policy change. If such anticipation exists, the naive treatment effect would be smaller than the true treatment effect. In Appendix B.5, we assumed the reform took place a year earlier than the true reform year (2016 for the 2017 tax increase and 2008 for the 2009 tax cut). Then, we implement the prediction algorithm and diff-in-diff estimation as described in Section 3.1. Appendix Table B.6 reports the elasticity estimates. In both reforms, the results are similar to the baseline estimates.

Alternative Specifications for Elasticities. In addition to using marginal tax rates to estimate the elasticity of reported estates with respect to the net-of-marginal-tax rate, we conduct a robustness check using average tax rates. This approach addresses the concern that taxpayers may respond to changes in their overall tax burden rather than only to marginal incentives. Significant tax reforms can substantially impact average tax rates and therefore influence estate planning decisions. By estimating elasticities with respect to the net-of-average-tax rate, we assess whether our key findings are robust to the choice of tax rate measure.

As shown in Appendix Table B.7, the estimated elasticities using the average tax rate are greater than those estimated with the marginal tax rate, as the first-stage effects are generally smaller. However, our two main findings (i) heterogeneous elasticities across types (repeal vs. within-positive) and (ii) directions (tax increase vs. cut), remain consistent, which reinforces the validity of our conclusions.

Macro Economic Conditions. 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.4, 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 (2) and (3) separately for each group. Appendix Table B.9 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.

Gifting. One possible explanation for the observed changes in reported estates following the reforms is a behavioral shift in gifting, as individuals may have increased or decreased their gifts, thus affecting their reported estates at death. As outlined in Section 2.1, the Taiwanese estate and gift tax systems are closely linked, with two key provisions designed to prevent tax avoidance through gifting. First, all gifts made within two years before death are included in the estate tax base. Second, the gift tax schedule moves with the estate tax schedule, i.e., gift and estate taxes are raised or lowered simultaneously. These rules are designed to lower potential estate tax avoidance through gifting near death.

The goals of this robustness check are twofold. First, we show that changes in reported 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 attribute to changes in estates. For the first goal, we estimate Equation 3 with gifts made within two years before death, scaled by pre-reform average wealth. Appendix Table B.10 presents the results: Column (1) shows that, after the 2017 tax increase, decedents reduced gifts by about 2.7%, 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 reported estates. Column (2) indicates no significant change in gifting after the 2009 tax cut, supporting that gifting does not explain the increased estates.

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.11. 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.11 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.12, using sudden death as a proxy for individuals' expectations of survival[6]. The results of the 2017 tax increase are in Columns (1) and (2). Nonsudden deaths (those likely expecting to die within two years) reduced

[6] 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.

gifting by about 2.6%, while sudden deaths showed no change. For the 2009 tax cut, shown in Columns (3) and (4), nonsudden deaths showed no change in gifting, while sudden deaths showed a 4% 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.

4 Mechanism

We have shown in the previous section that reported estates respond quickly and persistently following both reforms and that there is an asymmetrically stronger response to a tax increase than a tax cut. In this section, we aim to understand the sources of the responses and the driver of the asymmetry.

In Section 4.1, we break down estates by items to examine which components contribute to the responses. We calculate the elasticities of each estate item with respect to net-of-tax rates and quantify each item's contribution to the total change in reported estates.

Then, in Section 4.2, we examine whether the responses are tax avoidance or real behaviors such as adjusting labor supply or consumption by investigating decedents' pre-death outcomes and their heir children's labor supply after the reforms.

4.1 The Anatomy of Estate Items

What are the sources of the responses, and what drives the asymmetrical responses between a tax increase and a cut? To consistently compare the two reforms, we focus on the top 0.5% decedents and break estates into:

$$\begin{aligned} \text{Estates} = &\underbrace{(\text{Housing} + \text{Financial Asset} + \text{Deposit Savings})}_{\text{Asset}} \\ &- \underbrace{(\text{Other Deductible} + \text{Charitable \& Exemptions})}_{\text{Deduction}} \end{aligned}$$

Elasticities of Each Item. We estimate the elasticity of each item following the same procedure described in Section 3.1, replacing the dependent variable in Equation (3) with the value of each item. We then scale the coefficient by the mean of the item for the treated group in the pre-reform period. Figure 5 illustrates the elasticity estimates by item by reform. Appendix Table C.1 and C.2 present the detailed first-stage, reduced-form estimates used to calculate them. In Appendix Figure C.1 and C.2, we also show the event study graphs of the dynamic evolution of these items.

[Figure 5 here]

The blue estimates in Figure 5 refer to the results of the 2017 tax increase. We observe no response in housing and other deductibles, while financial asserts, deposit savings, and charity and exemptions shifted. The estimated elasticities of these items with respect to net-of-tax rate are 2.79 (se 0.35) for financial assets, 1.87 (se 0.43) for deposit savings, and 2.39 (se 0.67) for charity and exemption.

The orange estimates refer to the results of the 2009 tax cut. We find small responses in deposit savings and other deductibles and a greater response in charity and exemptions. The estimated elasticities are 0.38 (se 0.17) for deposit savings, 0.18 (se 0.09) for other deductibles, and 0.80 (se 0.15) for charity and exemption.

Comparing the sources of responses between the two reforms, we find that the 95% confidence intervals of the estimated elasticities differ significantly in financial assets, deposit savings, and charity and exemptions, suggesting they are the drivers of the asymmetrical responses.

Decomposition of Contribution to Total Elasticity. How does each elasticity of the item contribute to the elasticity of the total reported estate? We decompose the elasticity into a weighted sum of the elasticities of items.

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

The contribution of each item to the total elasticity of the reported estate depends on: (i) the magnitude of its own elasticity and (ii) the weight of the item in total estate. Table 3 presents the decomposition where we report the elasticity, weight, and the product of each item.

Panel A shows the numbers for asset items. Although liquid assets are more elastic, their smaller weight results in a lower contribution to the overall change in estate values. A similar pattern is observed for charitable deductions and exemptions in Panel B: while these items exhibit the highest elasticity, their small weight means they contribute relatively little to the total change.

For the 2017 tax increase, 66% of the change in reported estates is driven by assets, with the remaining 34% coming from deductions. In contrast, during the 2009 tax cut, only 4% of the change is driven by assets, while 96% comes from deductions. This suggests that most of the estate response during the tax increase is attributable to changes in assets, whereas the response during the tax cut is largely driven by changes in deductions.

[Table 3 here]

Summary. To summarize, first, we find that housing, an illiquid and third-party-reported item, is unresponsive in both reforms. This is consistent with the findings in the tax literature that link tax avoidance to whether items are third-party-repoted (Kleven et al., 2011; Waseem, 2020; Londoño-Vélez and Ávila Mahecha, 2024).

Second, the asymmetry between the two reforms is driven by more liquid and adjustable items: financial assets, deposit savings, and charity and exemptions. From a liquidity perspective, it is expected that these movable items would show greater responses in a shorter period. However, it remains unclear whether these shifts are avoidance behaviors, such as setting up trust funds or moving assets offshore, or real behaviors such as changes in consumption and labor supply. Given that the responses occurred quickly– within a year, we already see changes in reported estates, we argue these responses are more likely driven by avoidance. To explore this possibility further, the next section explores decedents' asset evolution before death, as well as labor supply behaviors by both decedents and their heirs.

4.2 Are the Responses "Real"?

Building on the findings above, this section aims to investigate whether the responses reflect tax avoidance or real behavioral changes. The changes in reported estates of decedents could be adjusted through several channels. Taking the 2017 tax increase as an example, the decrease in reported estates could be coming from decedents (i) consuming more; (ii) changing labor supply; (iii) using tax avoidance instruments such as setting up trust funds or exploiting offshore accounts to shift assets to get away from taxes etc.

Depending on what channels decedents adjust to, it will have different impacts on the heirs. If decedents change their consumption or labor supply, which in turn affects the actual estates left to their heirs, heirs might show corresponding behavioral responses. On the contrary, if decedents primarily engage in tax avoidance strategies that change reported but not true estate values, heirs' behavior may remain unaffected.

To examine this, Section 4.2.1 analyzes the pre-death wealth outcomes of decedents, focusing on variables such as housing, stock holdings, and labor income. Changes in the former two can indirectly signal changes in consumption or saving behavior, while the latter shows changes in labor supply.

In Section 4.2.2, we turn to heirs and examine the effect of the tax changes on their inheritance and labor supply behavior. *A priori*, if the changes in reported estates among decedents are primarily driven by tax avoidance, we would not expect to observe significant effects on the labor supply of their children.

4.2.1 The Effect of Reforms on Decedents' Outcomes Before Death

Housing and Stock holding. We use the overlapping items, housing and stock holding, which we observe both in the estate (at death) and wealth (pre-death) data to benchmark how much of the change in reported values at death can be attributed to changes in lifetime behavior after the reform. We focus on the outcomes one year and two years before death and estimate the same Equation 3 where we scale the outcome variables with the average pre-reform wealth. The results are shown in Table 4.

We first focus on the results of the 2017 tax increase, which are presented in Columns (1) to (3) in the table. Panel A Columns (1) to (3) are the housing values reported at death, one year before death, and two years before death, respectively for the 2017 tax increase. It shows that pre-death housing values did not change after the reform, which is consistent with the null response in reported housing at death.

[Table 4 here]

The relationship between financial assets reported at death and stock holding is trickier because financial assets include stock holding plus other self-reported items, and in the estate data, we only see its aggregate level. In Panel B, we report the effect of the reforms on financial assets reported at death. Column (1) shows that the 20.7% drop in financial assets (Column (2) in Panel B in Appendix Table C.1) translates into a 15.4% decrease when we measure it relative to the average pre-reform wealth and not to the average of its own pre-reform value.

In Panel C, Columns (2) and (3) report the stock values one year and two years before death, respectively. We find that the decline in reported financial assets at death during the 2017 tax increase is reflected in pre-death stock values. Stock values decreased by around 4% one and two years before death after the 2017 tax increase.

To investigate further, we split stock holding into privately- and publicly-listed stock, presented in Panel D and E, respectively. It shows that privately-listed stock decreased by around 2.5% before death. Publicly-listed stock decreased by around 1%. However, its decrease is only marginally statistically significant one year before death and statistically insignificant two years before death. In summary, the decline in stock values before death is mostly driven by privately-listed stock. This type of stock holding is assessed based on the net value of the firm, and these firms are often closely held by estate taxpayers who have significant control over firm valuations.

Benchmarking the extent to which the decrease in reported financial assets at death can be explained by the drop in pre-death stock requires additional assumptions, as the estate data only provides aggregate financial asset values without specifying the stock component. If we assume the extreme case that all financial assets are stock, this means $0.041/0.154 = 26.7\%$ of the decline in reported estate values can be attributed to changes in stock holdings one year before death. The rest would be a mixture of consumption/saving adjustments and avoidance manipulation made in the final year of life.

Columns (4) to (6) in Table 4 present the results of the 2009 reform. We do not find a change in one's lifetime housing and stock values, which is consistent with results in Section 4.1 where most of the increase in reported estates arises from deductions rather than assets.

Labor Income. Panel F in Table 4 displays the change in labor income. We find no effect on decedents' labor income one and two years before death following both reforms. This implies that labor supply was not a channel through which decedents adjusted their estates reported at death.

Summary. In both reforms, we can rule out the labor supply channel as a driver of changes in reported estates, as we find no effect on labor income. The remaining channels affecting reported estates are consumption/saving adjustments and tax avoidance.

During the 2017 tax increase, it is shown that individuals decreased their privately-listed stock holding, which resulted in a drop in the financial assets reported at death.

Disentangling whether the drop is a "real" change or tax avoidance is challenging and requires a more detailed analysis of firms' dividend payout behaviors; however, considering that many of these estate taxpayers closely hold these firms with substantial control over firm valuations, as well as the quickness of the response, we interpret this as more likely to reflect tax avoidance. In the following Section 4.2.2, we examine the heirs' outcomes to further investigate this hypothesis.

4.2.2 Heirs' Outcomes

We use decedents' heir children to examine the effect of the tax changes on their inheritance and their subsequent labor supply behavior. First, we establish a strong first stage by showing that the tax changes led to significant differences in inheritance levels. We then examine how these changes affect heirs' labor supply before and after reforms.

To construct the heir children samples, we start with the decedent samples and restrict them to the decedents who (i) did not have a living spouse at death and (ii) whose spouses did not die within three years before them.[7]

First stage: Inheritance. To examine how much their inheritances changed because of the tax changes, we estimate the following cross-sectional regression:

$$\begin{aligned} \text{Inheritance}_{j(i)} = \alpha_0 + \alpha_1 \text{Treated}_{j(i)} \times \text{PostReform}_i + \alpha_2 \text{Treated}_{j(i)} \\ + \alpha_3 \text{PostReform}_{j(i)} + \epsilon_{j(i)} \end{aligned} \tag{4}$$

where j is heir (child) and i is decedent (parent). $Inheritance_{j(i)} = Asset_i / NumHeir_i$[8], which is the average asset values of decedent i per heir. $Treated_{j(i)}$ is 1 if heir j's parent i is defined as treated in the decedent specification, which is when decedent i is predicted to be above the top 0.5% and 0 if between the 90th and 96th percentiles; $PostReform_{j(i)}$ is 1 if heir j's parent i died after the reform. The coefficient of interest is α_1, which is the treatment effect of the reforms on inheritance.

Table 5 presents the first-stage result. On average, the treated heir received 15 million TWD (about 480K USD) less after the 2017 tax increase and received 9 million TWD (about 340K USD) more after the 2009 tax cut. The difference between the 2017 and 2009 reforms reflects our earlier findings, where the 2017 tax increase induced a larger change in asset values compared to the 2009 tax cut.

[Table 5 here]

Labor Supply. The substantial first stage suggests that the reforms have a non-trivial impact on heirs. We now ask what the effect of a change in inheritances on their labor supply behavior is. To examine this, we track them three years before and four years

[7] The first restriction ensures that the inheritance does not go solely to the spouse, as we do not see how the estate is split among the heirs. The second restriction ensures that the heir children are not doubly shocked in a short period of time.

[8] $Asset_i$ is the reported assets of decedent i from the estate data. As mentioned earlier, gifts made within two years before death are also included, when constructing this number, we net out the gifts made within two years. $NumHeir_i$ is the number of children heirs of decedent i.

after their parent's death. We estimate the following individual fixed-effects regression separately for heirs whose parents died before and after the reform:

$$\begin{aligned} y_{j(i),t} = &\sum_{k\neq -1} \beta_k \cdot \text{Treated}_{j(i)} \times \mathbb{1}[t - \text{DeathYear}_i = k] \\ &+ \sum_{k\neq -1} \mathbb{1}[t - \text{DeathYear}_i = k] + \gamma' X_{j(i),t} + \alpha_{j(i)} + u_{j(i),t} \end{aligned} \tag{5}$$

where $y_{j(i),t}$ is the outcome of heir j in time t. $Treated_{j(i)}$ is defined as before, which is 1 if heir j's parent is predicted to be the top 0.5% and 0 if between the 90th and 96th percentiles. $\mathbb{1}[t - ParentDeathYear_i = k]$ are year-to-parental-death time dummies. $X_{j(i),t}$ contains age-specific fixed effects and marriage status that vary within an heir across years. $\alpha_{j(i)}$ is heir fixed effect. We omit event time $k = -1$ and cluster the standard error at the heir level.

Panel I of Figure 6 presents the results of the 2017 tax increase. Panel I(A) plots the evolution of the probability of being employed after the parent's death. The estimates for those whose parents died before the 2017 tax increase are in grey and those after are in blue. For those whose parents died before the tax increase, the probability of being employed dropped by around 2.5% one year after the parent's death and decreased to around 2% after four years. In other words, when exploiting the variation in the amount of inheritance between the treated (children of the top 0.5%) and control (children of those between P90-P96th percentiles), we find that the treated heirs reduced their labor supply more on the extensive margin. This also applies to those whose parents died after the 2017 tax increase.

When comparing these two samples, which is to exploit the tax variation where those who died after the tax increase now faced a higher tax liability, we show that the 95% confidence intervals of the two samples overlap and exhibit no statistically significant difference. This indicates that there is no difference in the extensive margin before and after the tax increase.

[Figure 6 here]

Panel I(B) plots the results of log labor income, which is the intensive margin. For both samples, treated heirs do not exhibit a different trend on their intensive margin compared to the control group. When comparing the two samples before and after the reform, we again find no difference between those whose parents died before versus after the reform.

For the 2009 tax cut, Panel II(C) demonstrates the results for the extensive margin. The probability of being employed of those whose parents died before the 2009 tax cut was reduced by around 0.5% within the first year after parent's death and further decreased to around 2% after four years. A similar pattern is observed for heirs whose parents died after the tax cut, suggesting that treated heirs respond more on the extensive margin than the control because of the levels of inheritances they receive. However, comparing the pre- and post-reform samples, we again do not see a statistically significant difference in whether the parents died before or after the reform. Similarly, Panel II(D) aligns with the findings of the 2017 tax increase, showing no differential behavior on the intensive margin between treated and control groups, nor between heirs whose parents died before and after the reform.

Summary and Comparison to Past Literature. Our specification Equation (5) exploits two variations: (i) the level of inheritance between the treated (children of the top 0.5%) and control (children of the P90-P96th), and (ii) the level of inheritance induced by the estate tax reforms.

For the first variation, we find that treated heirs reduced their labor supply on the extensive margin but not on the intensive margin. This result aligns with the existing literature, where most studies report a negative effect on the extensive margin following a parent's death (Holtz-Eakin et al., 1993; Nekoei and Seim, 2023). However, results on the intensive margin are mixed. Nekoei and Seim (2023) report around a 1% drop in earnings, while Holtz-Eakin et al. (1993) find no effect for single filers and only a small negative effect for joint filers,[9] with earnings decreasing by less than $1,000 for a $1 million USD inheritance.

The second source of variation, derived from estate tax changes, is particularly relevant because it is likely more exogenous than inheritance received following a parent's death and has not been extensively explored in previous literature. Using this exogenous variation, we find that no effect on heirs' labor supply before and after the estate tax changes. This null effect is striking given the substantial first-stage response observed in the change in the level of the inheritance receipt. The lack of an effect on heirs' labor supply, despite a significant first stage, combined with the rapid changes in decedents' reported estates and evidence from Section 4.2.1 supports a tax avoidance narrative. If changes in decedents' estates mainly reflect tax avoidance rather than true adjustments in estate values, heirs would have little reason to alter their behavior before and after the reforms.

4.3 Discussion: Asymmetrical Tax Avoidance Responses in the Presence of Sunk Costs

These findings, along with the quick changes in the reported estate that responded within a year after the reforms, suggest that the decedents' responses we capture are likely to be driven by tax avoidance rather than real changes. The asymmetrical responses between the tax increase and the tax cut also fit this interpretation. As individuals engage in tax planning during their lifetime, they incur avoidance costs such as setting up offshore accounts, establishing trust funds, or paying professional tax planners. When taxes increase, they may increase their avoidance even more. However, when taxes decrease, they may not reduce their avoidance by as much since they have already invested in these avoidance instruments. This also explains the faster response observed during the 2017 tax increase compared to the gradual adjustment seen in the 2009 tax cut.

[9] Holtz-Eakin et al. (1993) report that a $1 million USD increase in inheritance reduces joint filers' earnings by less than $1 thousand USD.

Although we do not have data on individuals' behavior about setting up trust funds or transferring wealth offshore, we show aggregate-level evidence that highlights the prevalence of these avoidance strategies among wealthy Taiwanese individuals. Appendix Figure C.4 shows the growth of aggregate trust fund sizes in Taiwan over time, as reported by the Taiwanese Trust Association. Interestingly, there was a sharp increase after the 2017 estate tax increase, with trust fund sizes growing by 43% between 2017 and 2020.

Offshore wealth plays a particularly significant role in Taiwan. According to Alstadsæter et al. (2018), the fraction of offshore wealth relative to GDP in Taiwan ranked 7th globally. Appendix Figure C.5 shows that offshore wealth in Taiwan accounted for 22% of GDP in 2007, substantially higher than the global average of 9.8%. Even more striking, Appendix Figure C.6 shows Taiwan topping the list for the number of unique shell company owners identified in the Panama Papers Leak.

These pieces of evidence suggest that wealthy individuals in Taiwan have access to means for avoiding/evading estate taxes. These avoidance measures often come with setup costs such as legal fees or administrative expenses that become "sunk" once investments are made. Reversing these arrangements by bringing money back onshore or dissolving trusts can also be costly or carry penalty fees, making individuals less inclined to adjust avoidance efforts when tax rates decrease, which is why there is an asymmetrical response to tax increases versus decreases.

5 Welfare Analysis

In this section, we propose an estate tax planning framework built on Chetty (2009). We first start with a static framework, then explain the observed asymmetry in the empirical section with a two-period scenario, and analyze the welfare effect when the tax rate changes.

5.1 Model Setup

Consider an individual i who has an initial wealth w. She chooses to consume c, shelter s units of wealth by paying a resource cost $\Gamma(s)$, and leave e as estates to her heirs, which are the after-tax reported estates plus the amount she sheltered. Her reported estates are $ReportedEstate \equiv w - c - s$. Her true estates left are the after-tax reported estates plus the sheltered amount, $e \equiv (1-\tau) \cdot ReportedEstate + s$ where $\tau \in [0,1]$. $\Phi(e)$ is the utility of bequeathing estates to her heirs. We assume agents have quasi-linear utility.

She solves the following maximization problem

$$\max_{c,s} \quad c + \Phi(e) - \Gamma(s)$$
$$\text{s.t.} \quad \underbrace{e}_{\text{True Estates}} = \underbrace{(1-\tau)\cdot(w-c-s)}_{\text{After-tax Reported Estates}} + \underbrace{s}_{\text{Sheltered amount}}$$

The F.O.C. are

$$\partial/\partial c : (1-\tau)\cdot\Phi'(e) = 1$$
$$\partial/\partial s : \tau\cdot\Phi'(e) = \Gamma'(s)$$

The first F.O.C. implies that the individual will choose consumption such that the after-tax marginal utility of leaving estates is equal to the marginal utility of consumption. The second F.O.C. indicates that the individual will shelter up to the point where the marginal benefit of sheltering is equal to the marginal cost of sheltering.

Explanation for the Asymmetry. In our empirical results, we find that individuals respond more strongly to tax increases than to tax cuts. This phenomenon can be explained by a two-period model in which the government unexpectedly changes the tax rate in the second period.

In the second period, individuals can adjust their sheltering decisions by either increasing or decreasing the amount of sheltered wealth. However, the marginal cost of decreasing what was already sheltered would be higher than increasing it. For example, reducing sheltered assets may involve paying penalty fees for dissolving trust funds or shifting money back onshore.

As a result, the model would predict that when there is a tax increase, individuals will choose to shelter more wealth in the second period. In contrast, when there is a tax cut, individuals are less likely to reverse their initial sheltering decisions, leading to a smaller response. This asymmetry explains why we observe a larger behavioral response to tax increases compared to tax cuts.

Welfare. How does welfare change in response to tax rate changes? Suppose welfare is the sum of agents' utilities and tax revenues:

$$W(\tau) = \underbrace{c + \Phi(e) - \Gamma(s)}_{\text{Utility}} + \underbrace{\tau\cdot(w - c - s)}_{\text{Tax revenues}}$$

$$dW(\tau) = \underbrace{\frac{-RE\cdot d\tau}{1-\tau}}_{dU} + \Big[\underbrace{RE\cdot d\tau}_{dM} - \underbrace{\frac{\tau\cdot\varepsilon\cdot RE\cdot d\tau}{1-\tau}}_{dB}\Big]$$

where the first term dU is the change in utilities, which is the direct effect of leaving less estate to heirs. The second term is the change in tax revenues, which could be further split into a mechanical change (dM) and behavioral change (dB).

5.2 Policy Analysis

Using our empirical estimates, we explore the policy implications of estate tax reforms by focusing on the top 0.5%. This allows us to compare outcomes under different scenarios by applying the estimated elasticities from the 2017 tax increase and the 2009 tax cut and compare the scenarios under whether accounting for the presence of sunk costs.

In our empirical findings, we find that responses to tax increases and cuts differ in both magnitude and adjustment speed: the response to the 2017 tax increase is immediate and stable, while the response to the 2009 tax cut evolves gradually, beginning smaller and increasing as individuals recover from sunk costs. This implies that during a tax increase, the elasticity without sunk costs will accurately inform the welfare effect. In contrast, during a tax cut, both elasticities, the short-run and the long-run, will inform welfare, with the rate at which individuals recover from sunk costs impacting the cumulative welfare effect.

2017 Tax Increase. Columns (1) and (2) of Table 6 present the parameters and results for the 2017 reform, utilizing the estimated elasticities from the 2009 tax cut and the 2017 tax increase, respectively. Panel A includes the average marginal tax rate, the average tax rate change, and the total sum of reported estates of the top 0.5% in 2016 observed in our data.

Panel B presents the results of tax revenue changes, splitting by the mechanical and behavioral effects. dB/dM could be interpreted as, with \$1 TWD mechanically raised from the tax increase, how much would be lost due to behavioral effect. Panel C illustrates the welfare changes.

In Column (1), using the 2009 tax cut elasticity suggests that for each 1 TWD mechanically raised, there is only a 0.05 TWD loss from behavioral effects, resulting in an increase in tax revenues by 10.6 billion TWD and a welfare decrease 1.8 billion TWD. In contrast, Column (2), which uses the 2017 tax increase elasticity, indicates a welfare loss of 4.6 billion TWD due to a more substantial behavioral response.

The discrepancy between the two scenarios highlights that the welfare change would be overestimated by 61% if the government naively implements the underestimated elasticity where sunk costs are present.

2009 Tax Cut. Columns (3) and (4) of Table 6 summarize the parameters and results of the 2009 tax cut. Panel A shows the data moments of the top 0.5% in 2008 observed in our data.

In Column (3), applying the estimated elasticity of the tax cut (0.465) represents the short-term welfare impact. This results in a decrease in tax revenues by 12.53 billion TWD and an increase in welfare by 12 billion TWD. In contrast, Column (4) uses the higher elasticity estimate from the 2017 tax increase (2.757), which represents the long-term effect. This leads to a much larger behavioral effect that offsets the mechanical decrease in tax revenues, resulting in a welfare increase of 31 billion TWD.

This suggests that during a tax cut, in the short run, as individuals face sunk costs, tax revenues will decrease; however, as they recover from the sunk costs, tax revenues will ultimately flip sign and become positive. The cumulative tax revenue change will depend on the pace at which individuals recover from their sunk costs.

6 Conclusion

This study leverages two large estate tax reforms in Taiwan to analyze behavioral responses to estate taxation. We find that responses are both quick and persistent, and differ substantially based on the nature of the tax change: full repeals elicit larger elasticities than within-positive tax variation, and responses to a tax increase are six times stronger than those to a tax cut.

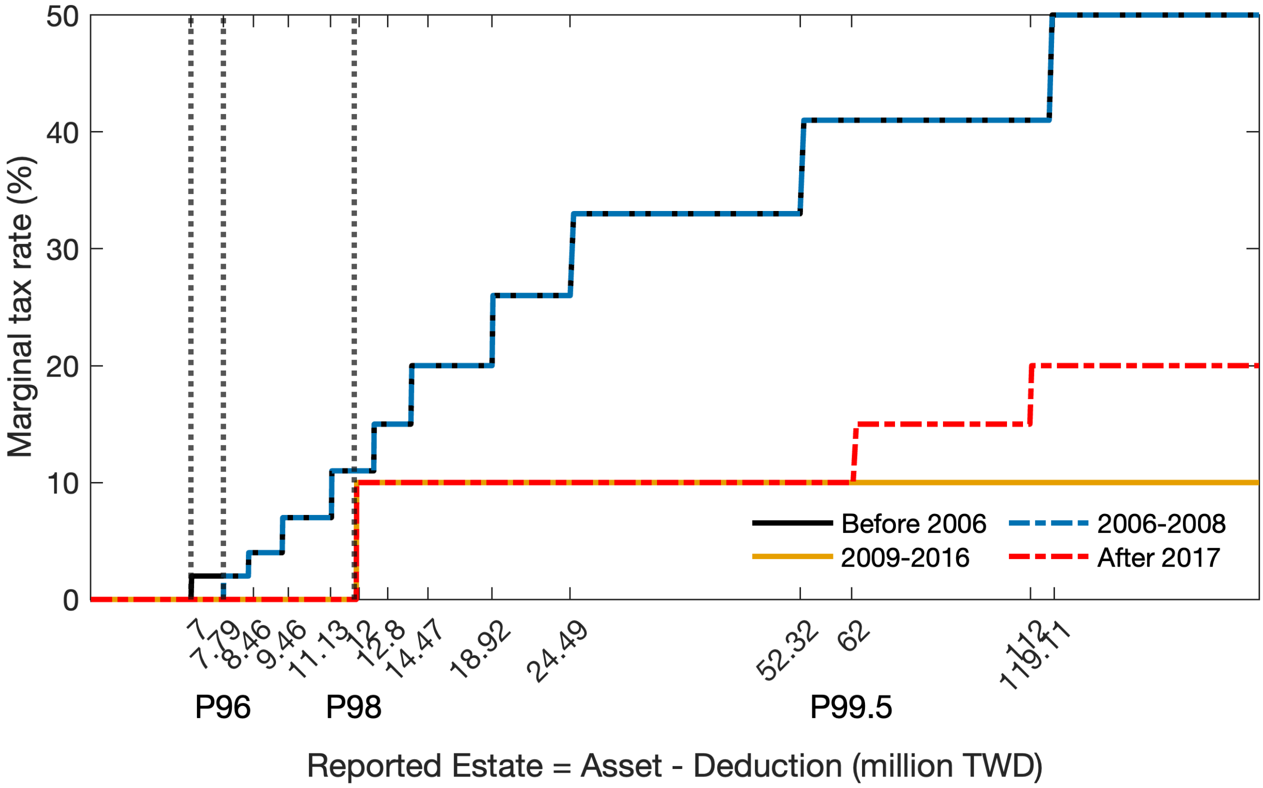
Our analysis suggests that these adjustments are mainly driven by tax avoidance, particularly through liquid assets like financial holdings and charitable exemptions, which respond more strongly to tax increases. The lack of labor supply changes among decedents and heirs supports the interpretation that these responses reflect avoidance strategies rather than real behavioral adjustments.

The observed asymmetry in responses aligns with the tax avoidance framework: initial investments in avoidance strategies create sunk costs, leading individuals to adjust more to tax increases than to tax cuts. We model these dynamics and show that using elasticities from scenarios with sunk costs could misestimate welfare impacts. If the government applies the tax cut elasticity, which is dampened by sunk costs, it would underestimate the welfare cost and overestimate the net welfare effect of a tax increase by 61%.

Our findings have important policy implications. First, the substantial tax avoidance in response to estate tax changes underscores the need for stronger enforcement measures, such as expanding third-party reporting and enhancing international cooperation to detect offshore assets. Second, the observed asymmetry in taxpayer responses to estate tax increases and cuts has significant welfare implications and should be carefully considered by policymakers when evaluating estate tax reforms. Moreover, this asymmetry due to sunk costs may extend beyond estate taxes to other areas where tax avoidance involves similar fixed costs, such as corporate income and wealth taxes.

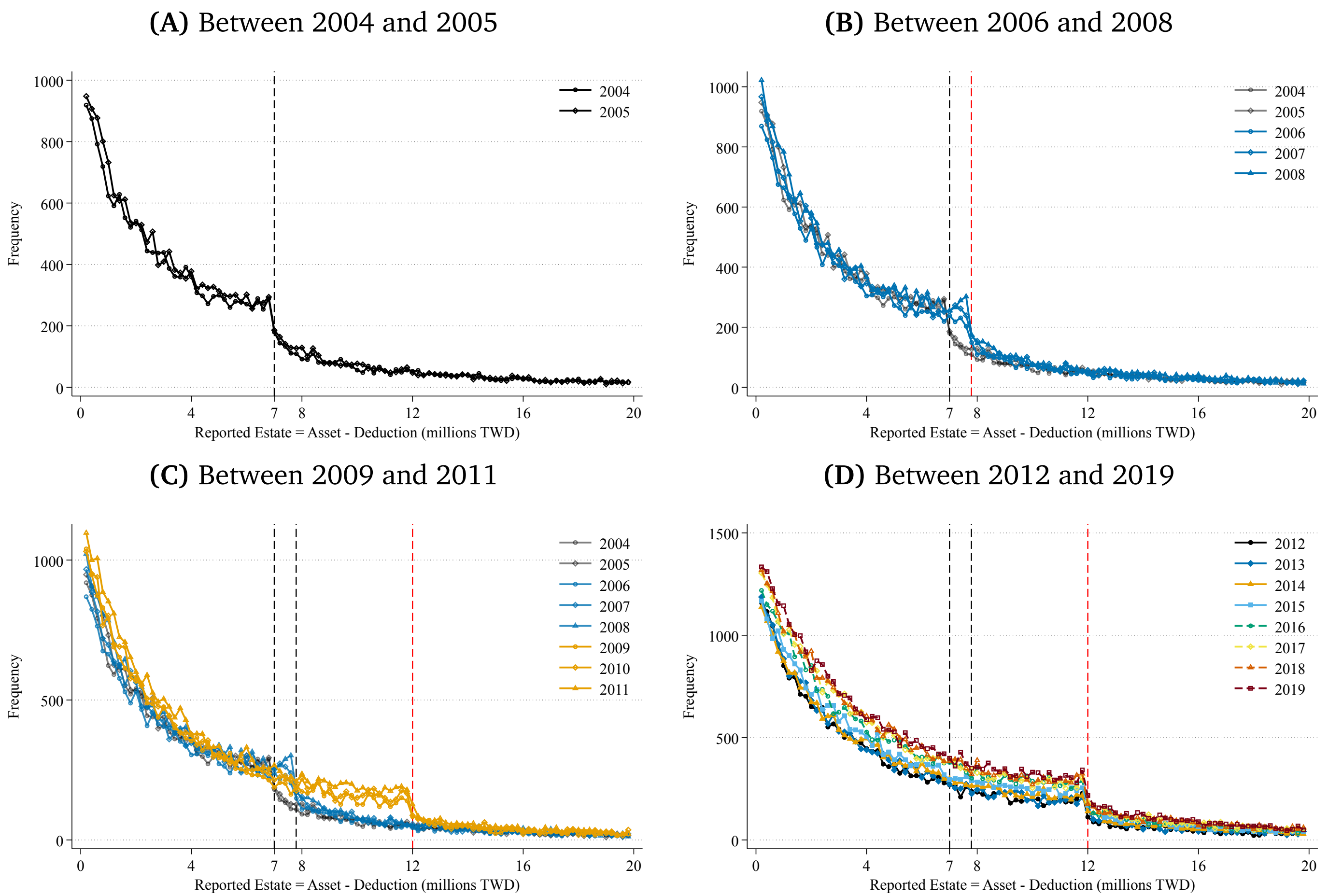
Figures

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 233K 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 MTRs 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. Click here to return to the text.

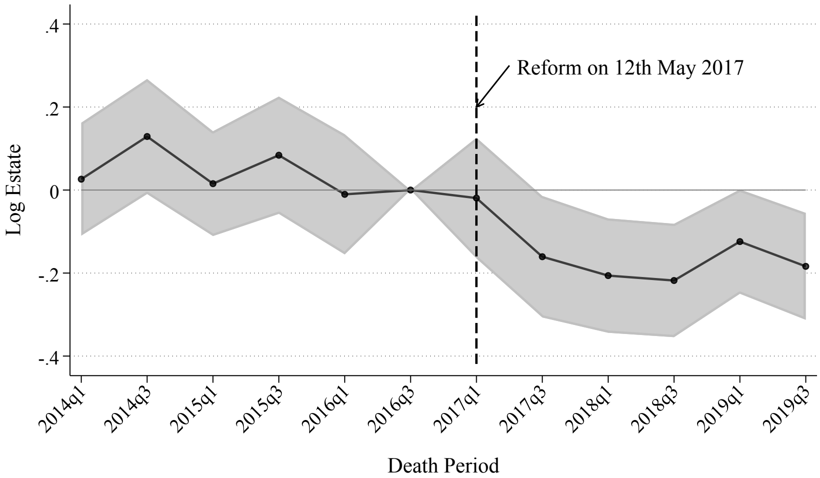
Figure 2: Density of Reported Estate Around Thresholds Over Time



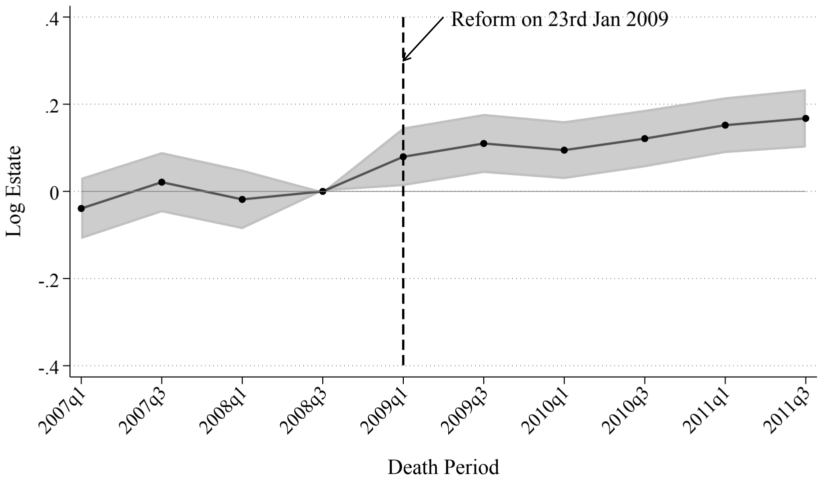
Notes: This figure presents the distribution of individuals around the exemption thresholds over time. Individuals are binned every 200K TWD (approximately 6K USD) interval. Panel A shows the distribution between 2004 and 2005 and the black dashed line corresponds to the 7 million TWD threshold. Panel 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 2009 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 plots all the distribution of the later years, showing that the excess mass at the 2009-induced new threshold remains even ten years later. Click here to return to the text.

Figure 3: The Effects of Estate Tax Reforms on Reported Estate

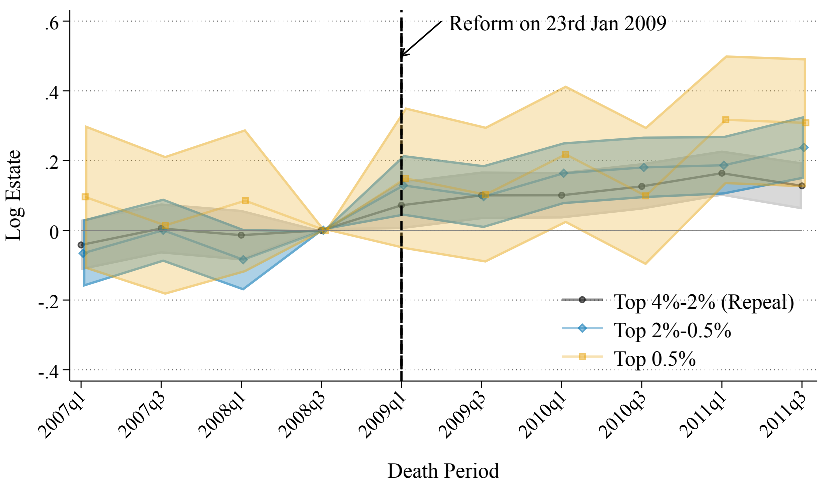
(A) 2017 Tax Increase: Top 0.5%



(B) 2009 Tax Cut: Top 4%



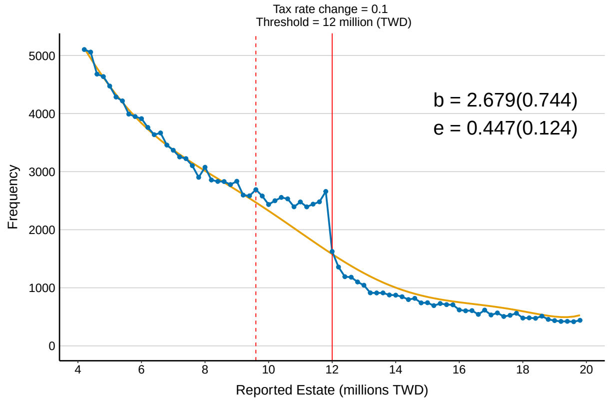
(C) 2009 Tax Cut: Splitting Top 4%-2% (Repeal), Top 2%-0.5%, Top 0.5%



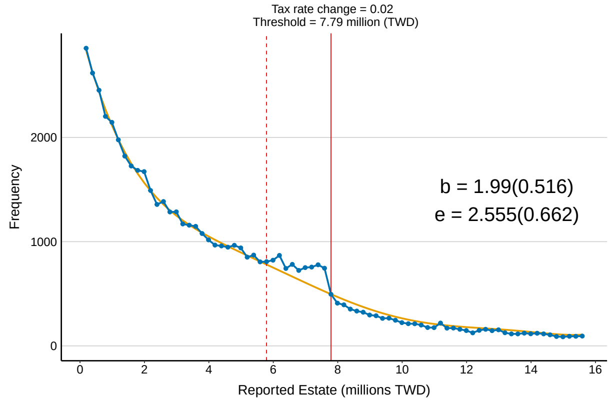
Notes: This figure presents the evolution of reported estates before and after the tax reforms. Panel A shows the result of the 2017 tax increase where the treated are those predicted to be the top 0.5% and the control are those predicted to be between the 90th and 96th percentiles. Panel B and C refers to the results of the 2009 tax cut where the reform 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 three subgroups: top 4%-2% (repeal), top 2%-0.5%, and top 0.5%. The control remains the same. These graphs plot the estimated coefficients of the interaction term and their 95% confidence intervals from Specification (1). Click here to return to the text.

Figure 4: Bunching Estimates Using Exemption kinks v.s. Within-positive kinks

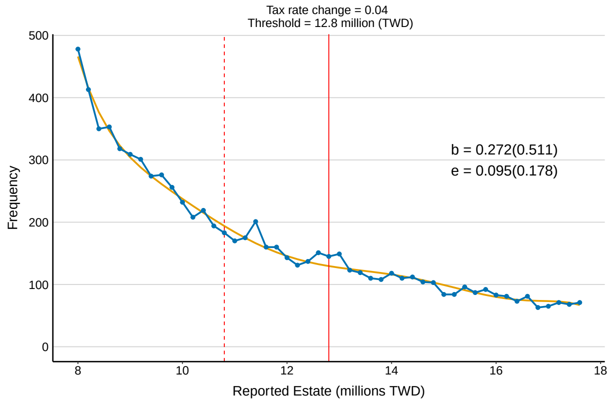
(A) Exemption: 12M TWD, 2009-2019



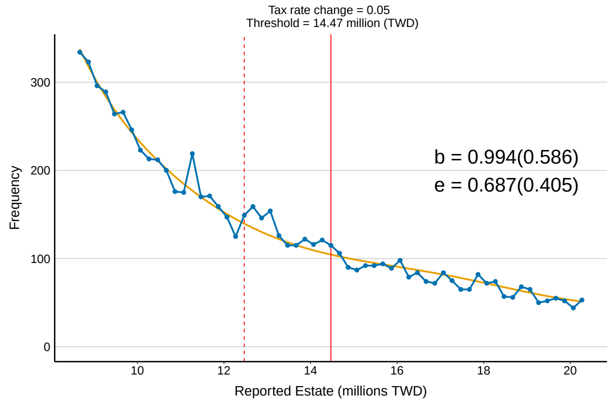
(B) Exemption: 7.79M TWD, 2006-2008



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

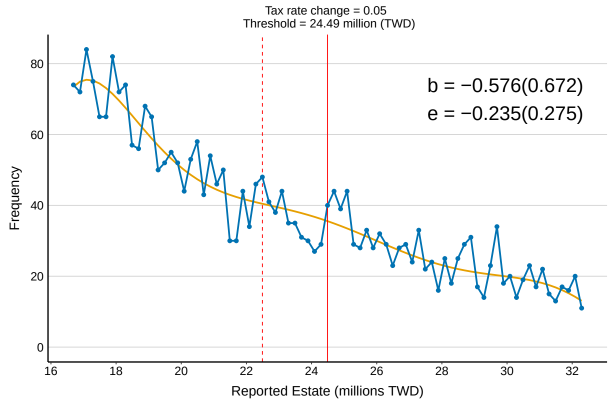


(D) Within-positive: 14.47M TWD, 2006-2008



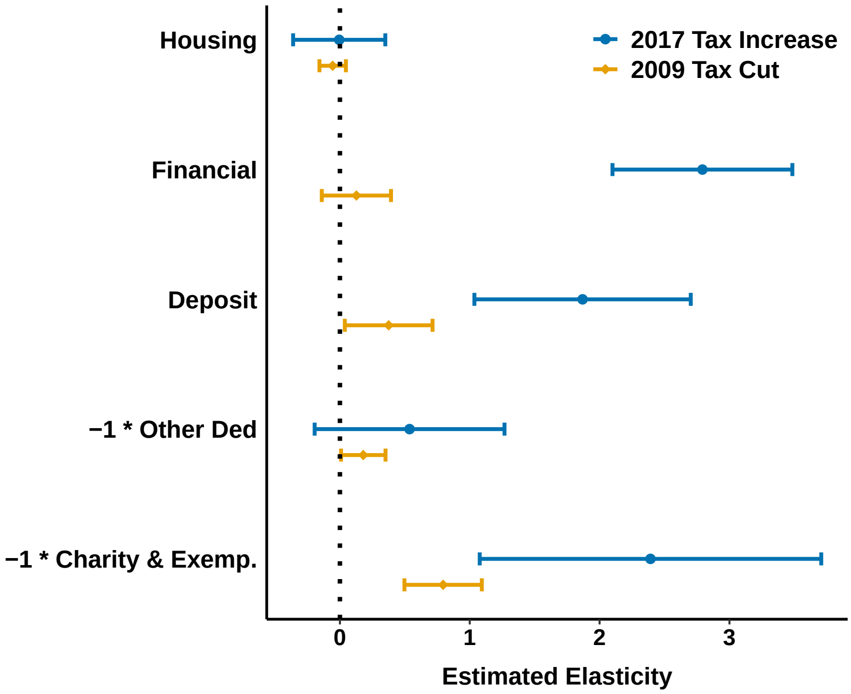
(E) Within-positive: 18.92M TWD, 2006-2008

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



Notes: This figure groups individuals into bins of 200,000 TWD of reported estates and plots the frequency around different kinks, categorized by whether the kink is around an exemption threshold (exemption kink) or adjusts tax rates 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. Click here to return to the text.

Figure 5: Elasticity Estimates of Each Item

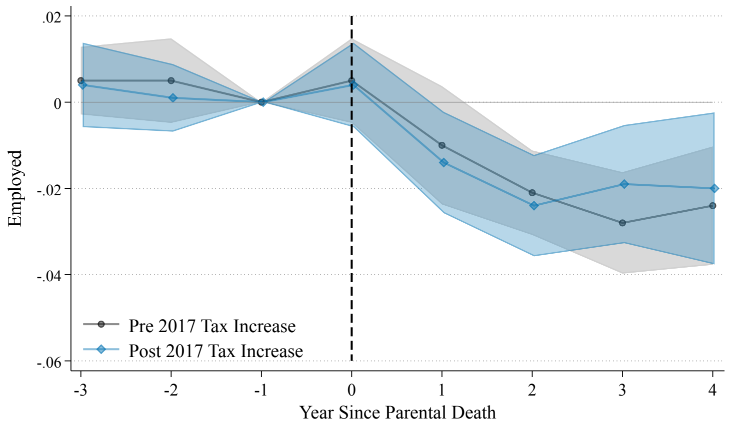


Notes: This figure presents elasticity estimates by estate items, categorized into housing, financial assets, deposit savings, other deductibles, and charity and exemptions. The estimates for the 2017 tax increase are shown in blue, while those for the 2009 tax cut are shown in orange. Detailed first-stage and reduced-form estimates used for the calculations are presented in Appendix Table C.1 and Table C.2. The treatment group in the estimation are individuals predicted to be above the 0.5th percentile, while the control group are those predicted to be between the 90th and 96th percentiles. Click here to return to the text.

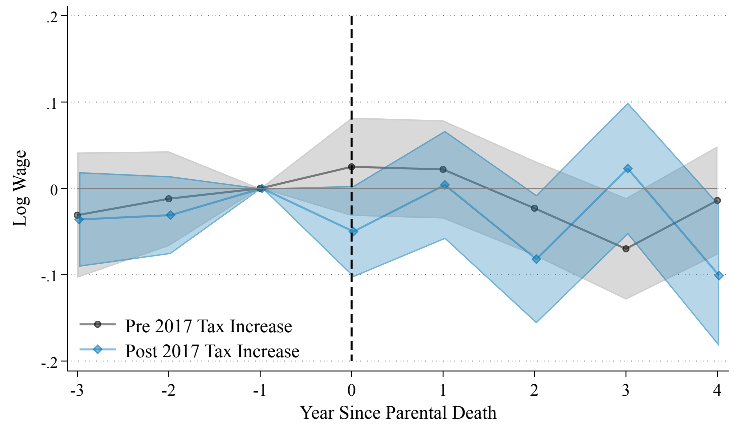
Figure 6: The Effects of Estate Tax Reforms on Heirs' Outcomes

Panel I. 2017 Tax Increase

(A) Pr(employed)

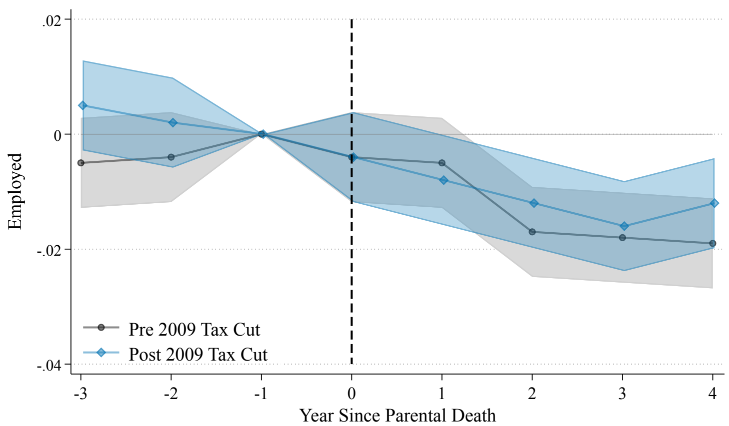


(B) Log(labor income)

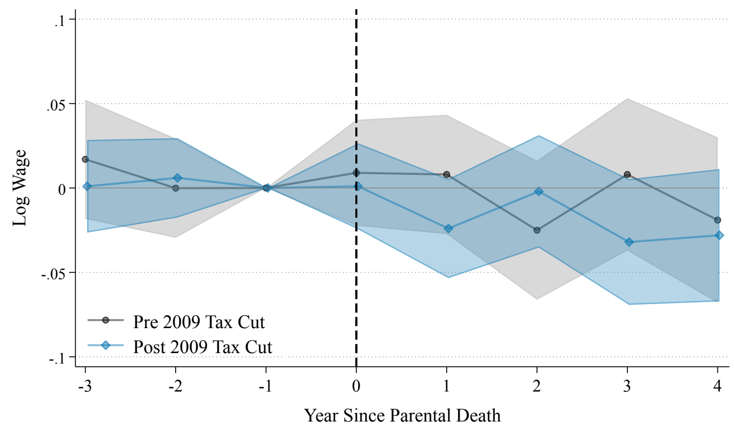


Panel II. 2009 Tax Cut

(C) Pr(employed)



(D) Log(labor income)



Notes: This figure presents the event study estimates from Specification 5. In the regressions, the treated group consists of heirs of donors predicted to be above the top 0.5%, while the control group consists of heirs whose parents are predicted to be between the 90th and 96th percentiles. Panel I shows the results for the 2017 tax increase, with outcomes on the extensive margin and intensive margin displayed in Panels A and B, respectively. Similarly, Panel II shows the results for the 2009 tax cut. Click here to return to the text.

Tables

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

	(1)	(2)	(3)	(4)	(5)
	2017 Tax Increase	**2009 Tax Cut**			
	All	All	Repeal	Pure Decrease	
	Top 0.5%	Top 4%	Top 4%-2%	Top 2%-0.5%	Top 0.5%
First stage	-0.074	0.100	0.045	0.128	0.331
	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)
	[29,576]	[25,569]	[20,918]	[19,167]	[16,634]
Reduced form	-0.204	0.131	0.130	0.204	0.154
	(0.029)	(0.016)	(0.016)	(0.021)	(0.044)
	[63,627]	[71,234]	[59,045]	[52,925]	[46,410]
Elasticity	2.757	1.310	2.889	1.594	0.465
	(0.394)	(0.161)	(0.261)	(0.166)	(0.133)

Notes: This table shows the treatment effect on log net-of-tax-rate change (first-stage) and on log reported estates (reduced form), and the implied elasticities of reported estates. Column (1) shows the result of the 2017 tax increase. Columns (2)-(5) are the results of the 2009 tax cut where (2) uses the treated group top 4%. Column (3)-(5) split the top 4% into top 4%-2% (repeal), top 2%-0.5%, and top 0.5%, respectively. Standard errors are in parentheses and the number of observations are in brackets. Click here to return to the text.

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

Paper	Variation	Context	Outcome variable	Est. elasticity w.r.t. $1-\tau$
Panel A: DiD (repeal)				
Mas-Montserrat (2019)	Repeal (top 5%-1%)	Spain	Inheritance	2.51^{***} (s.e. 0.33)
***Our study	Repeal (top 4%-2%)	Taiwan	Estate	2.89^{***} (s.e. 0.36)
Panel B: DiD (within-positive rate change)				
***Our study	Decrease (top 2%-0.5%)	Taiwan	Estate	1.59^{***} (s.e. 0.17)
***Our study	Decrease (top 0.5%)	Taiwan	Estate	0.47^{***} (s.e. 0.13)
***Our study	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	Bunching (0% to 2%)	Taiwan	Estate	3.17^{***} (s.e. 0.51)
***Our study	Bunching (0% to 10%)	Taiwan	Estate	0.57^{***} (s.e. 0.10)
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	Bunching (11% to 15%)	Taiwan	Estate	0.10 (s.e. 0.19)
***Our study	Bunching (15% to 20%)	Taiwan	Estate	0.27 (s.e. 0.19)
***Our study	Bunching (20% to 25%)	Taiwan	Estate	0.28 (s.e. 0.19)
***Our study	Bunching (25% to 33%)	Taiwan	Estate	0.04 (s.e. 0.26)
Panel E: RDD				
Escobar (2017)	Tax decrease	Sweden	Inheritance	0.88^{***} (s.e. 0.36)
Panel F: OLS				
Slemrod and Kopczuk (2001)	Time-series	U.S.	Estate	Noisy, [-0.11, 0.09]
Joulfaian (2006)	Time-series	U.S.	Tax rev/Avg tax rate	0.14^{***} (s.e. 0.05)

Notes: Mas-Montserrat (2019)'s number is from Table 4 Column 2. We use the version where they converted the heir-level MTR to an aggregate donor-level MTR as that is more similar to our estate tax setting. Escobar et al. (2019)'s number is from Figure C1. Glogowsky (2021)'s number is from Column 5 in Table 1. Slemrod and Kopczuk (2001)'s number is from Table 3 Column III. Joulfaian (2006) 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 τ. Click here to return to the text.

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	Product	Elasticity	Weight	Product
Panel A: Asset						
Housing	-0.005	44.5%	-0.008	-0.056	47.6%	-0.117
	(0.181)			(0.052)		
Financial Asset	2.792	14.7%	1.367	0.127	10.1%	0.055
	(0.353)			(0.136)		
Deposit Saving	1.869	5.8%	0.361	0.376	3.8%	0.062
	(0.425)			(0.172)		
Subtotal Asset			1.720			0.001
Panel B: Deduction						
-1*Other Deductible	0.537	29.4%	0.524	0.180	31.6%	0.250
	(0.373)			(0.087)		
-1*Charity & Exemp.	2.392	5.6%	0.448	0.795	6.9%	0.240
	(0.671)			(0.152)		
Subtotal Deduction			0.972			-0.490
Estate (= Asset-Deduction)			2.692			0.491
% from Asset			**63.9%**			**0.3%**
% from Ded			**36.1%**			**99.7%**

Notes: This table presents the decomposition computation from the analysis in Section 4.1 that focuses on the top 0.5%. Panel A shows the asset breakdown, divided into housing, financial assets, and deposit savings, while Panel B shows the deduction breakdown, divided into other deductibles and charity and exemptions. Columns (1) through (3) display the results for the 2017 tax increase: Column (1) contains the elasticity estimates for each item, Column (2) shows each item's weight relative to the total estate, and Column (3) presents the product of the elasticity and weight. Similarly, Columns (4) through (6) present the results for the 2009 tax cut. The last two rows calculate the contributions of assets and deductions to total estates. Click here to return to the text.

Table 4: The Effect of Estate Tax Changes on Donors' Pre-Death Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	2017 Tax Increase			**2009 Tax Cut**		
	At Death	1 Year Before	2 Years Before	At Death	1 Year Before	2 Years Before
All dependent variables are scaled by one's average pre-reform wealth						
Panel A: Housing						
Treated#Post	-0.094	-0.066	-0.027	0.143	0.188	0.120
	(0.053)	(0.060)	(0.049)	(0.087)	(0.176)	(0.099)
Panel B: Financial Asset						
Treated#Post	-0.154^{***}			0.020		
	(0.018)			(0.018)		
Panel C: Stock						
Treated#Post		-0.041^{***}	-0.039^{***}		0.012	0.016
		(0.009)	(0.009)		(0.012)	(0.012)
Panel D: Private stock						
Treated#Post		-0.023^{***}	-0.025^{***}		0.006	0.008
		(0.005)	(0.006)		(0.007)	(0.007)
Panel E: Public stock						
Treated#Post		-0.011^{*}	-0.008		0.004	0.006
		(0.005)	(0.005)		(0.008)	(0.008)
Panel F: Labor Income						
Treated#Post		-0.0002	0.0001		0.0005	0.0007
		(0.0002)	(0.0002)		(0.0003)	(0.0004)
Number of obs	63,627	63,627	63,627	46,410	46,410	46,410

Notes: This table presents the estimated coefficients of the interaction term from Specification (3). The dependent variables are all scaled by the donor's average pre-reform wealth to account for zeros. The treated and control groups are defined as previously: the treated group includes those predicted to be in the top 0.5%, while the control group includes those predicted to be between the 90th and 96th percentiles. Columns (1) to (3) display the outcomes at death, one year before death, and two years before death, respectively, for the 2017 reform. Columns (4) to (6) present the outcomes at death, one year before death, and two years before death, respectively, for the 2009 reform. Click here to return to the text.

Table 5: The Effect of Estate Tax Changes on Heirs' Inheritance (thousands TWD)

	(1) **2017 Tax Increase**	(2) **2009 Tax Cut**
Treated#PostReform	-15,164.081***	9,794.326***
	(1,920.561)	(1,262.188)
Num of heirs	72,704	53,597

Notes: This table presents the estimated coefficient on the interaction term in Specification 4, indicating the treatment effect of the reforms on inheritance. 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) shows the result for the 2017 tax increase, and Column (2) shows the result for the 2009 tax cut. Click here to return to the text.

Table 6: Data Moments and Results

	(1)	(2)	(3)	(4)
	2017 Tax Increase		**2009 Tax Cut**	
	Estimated Elasticity		Estimated Elasticity	
	$\hat{\varepsilon}^{TaxCut} = 0.465$	$\hat{\varepsilon}^{TaxInc} = 2.757$	$\hat{\varepsilon}^{TaxCut} = 0.465$	$\hat{\varepsilon}^{TaxInc} = 2.757$
Panel A: Parameters				
Mean τ_{t-1}	0.098	0.098	0.340	0.340
Mean $d\tau$	0.064	0.064	-0.250	-0.250
Aggregate RE_{t-1}	175125.783	175125.783	35000.610	35000.610
Panel B: Tax Revenues				
Mechanical (dM)	11208.050	11208.050	-16474.388	-16474.388
Behavioral (dB)	-566.243	-3357.271	3946.365	23482.991
$-dB/dM$	0.051	0.300	0.240	1.425
Total ($dM + dB$)	10641.807	7850.779	-12528.022	7008.603
Panel C: Welfare				
dW	-1783.968	-4574.997	12433.170	31884.929

Notes: All values in millions 2016 TWD except for tax rates or ratios. τ^{t-1} is the tax rate in 2016 for the 2017 tax increase and in 2008 for the 2009 tax cut. Aggregate RE_{t-1} is the total sum of reported estates of the top 0.5% in 2016 for the 2017 tax increase and in 2008 for the 2009 tax cut. Click here to return to the text.

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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 biannually and lower than the market prices.
Financial Assets	Include publicly-listed and privately-listed 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-listed 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	
Charitable/Exemptions	Charitable contributions are donations to nonprofit organizations. Exemptions 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

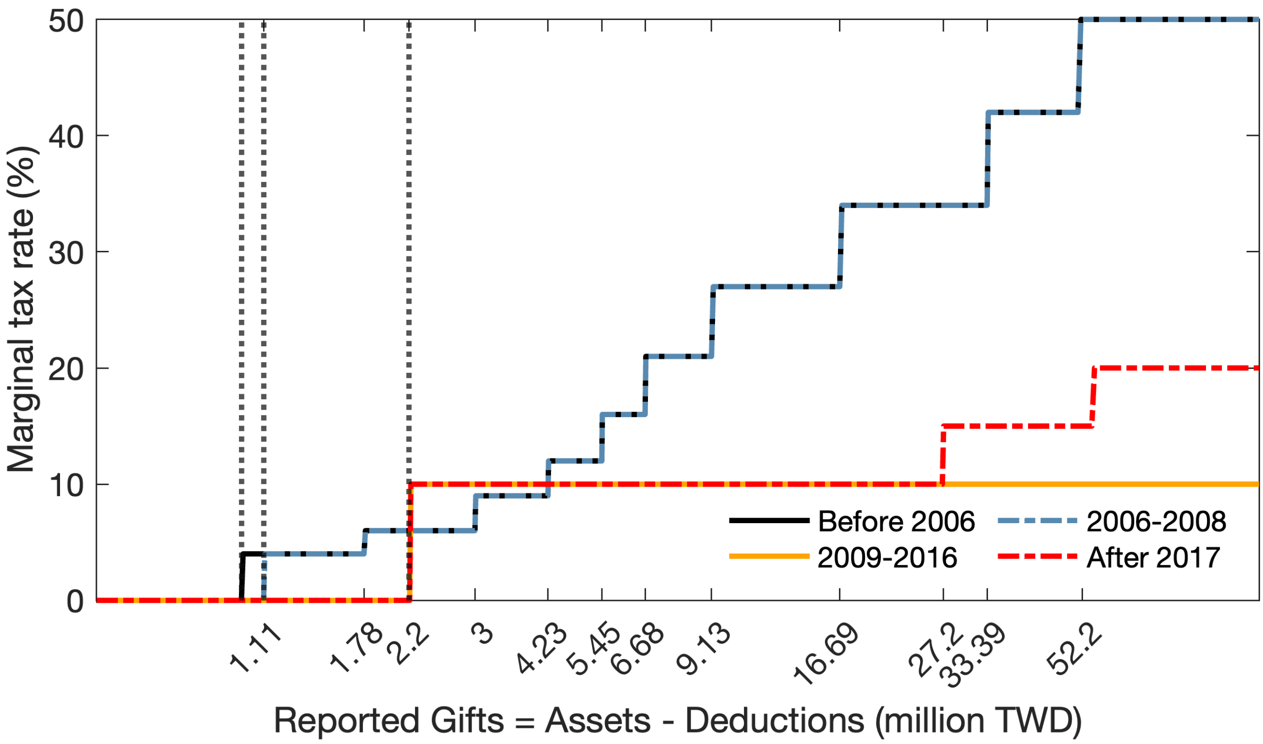
Table A.1: (Continued)

Item	Description
Other Deductibles	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: Click here to return to the text.

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 MTRs brackets are introduced for those above 27.2 million TWD. Click here to return to the text.

B Appendix to Section 3: Empirical Analysis

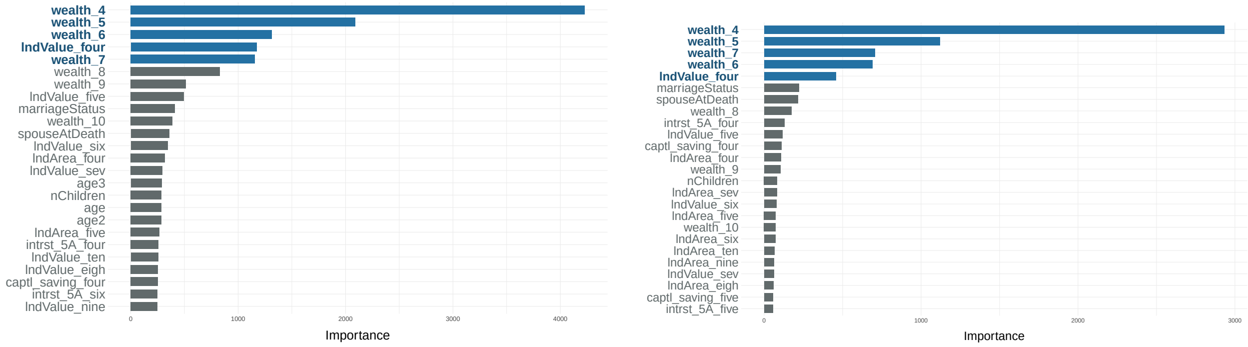
B.1 2017 Prediction Algorithm Details

Procedures. To construct donors' counterfactual estates in the absence of the reform, we use a random-forest algorithm to predict a donor's reported estates at death based on her past wealth 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 testing 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, stocks, capitalized interest income, earned income, unearned income, whether a business owner [4 years, 5 years,.., 10 years before death], and gender, age, number of kids, marriage status, born place, parents 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 held-out testing 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 and demographic variables, which serve as the predictors in the algorithm, we can back out everyone's counterfactual estate percentile group had the reform not happened

Important Predictors. We report the most important top 25 predictors in our algorithm. Panel A in Figure B.2 presents the overall top 25 most important predictors in our algorithm (2014-2016). Panel B shows those when using 2014-2015. In both periods, the top 5 are the same: 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, with just the difference of the top 3-5 being reorderd.

Figure B.2: Top 25 Important Predictors



(A) Overall: 2014-2016 **(B)** 2014-2015

Performance. Table B.2 presents the prediction performance, measured in precision and recall rates using the held-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 the overall performance of the 2017 prediction algorithm, which uses 2014-2016 as the training samples. Then, to show the stability over time, in Column (3) to (8) we split them into 2014, 2015, and 2016, respectively.

Overall, the precision rates range from 61% to 82% by group, and the recall rates range from 60% to 88%. When splitting them by year, the results are similar, landing support on the stability of the algorithm.

Table B.2: Performance of 2017 Prediction Algorithm

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Overall		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%

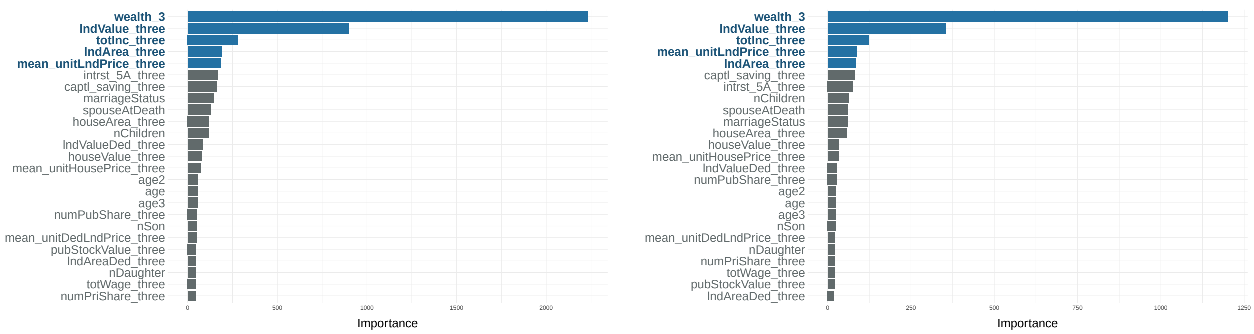
B.2 2009 Prediction Algorithm Details

Procedures. To construct donors' counterfactual estates in the absence of the reform, we use a random-forest algorithm to predict a donor's reported 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 testing sample
- 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-P8, iv) P98-P99.5, v) above P99.5
- Predictors are the values and quantities of land, houses, stocks, capitalized interest income, cars, earned income, unearned income, whether a business owner [3 years before death] and gender, age, number of kids, marriage status, born place, parents alive etc
- In the above step, 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 held-out testing 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. Panel A in Figure B.3 presents the overall top 25 most important predictors in our algorithm (207-2008). Panel B shows those when using 2008 only. In both periods, the top 5 are the same: wealth three years before death, land values three years before death, land value three years before death, land area three years before death, and average land prices per squared meter three years before death.

Figure B.3: Top 25 Important Predictors



(A) Overall: 2007-2008 **(B)** 2007

Performance. We report the performance of the prediction algorithm of the 2009 reform where the 2006-2007 years are used as the training samples.

Table B.3: Performance of 2009 Prediction Algorithm

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall		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%

B.3 Robustness check: Alternative Control Groups

Table B.4: Elasticity Estimates Using Alternative Control Groups

	2017 Tax	2009 Tax Cut	
	Top 0.5%	Top 4%	Top 0.5%
P90-P93	2.991	1.441	0.507
	(0.536)	(0.238)	(0.211)
P93-P96	3.277	0.266	0.131
	(0.534)	(0.179)	(0.142)
P96-P98	2.773		
	(0.531)		
P98-P99.5	0.688		
	(0.550)		
P90-96 (baseline)	**2.745**	**1.317**	**0.465**
	(0.441)	**(0.204)**	**(0.203)**

B.4 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 groups 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 3 and present the estimates in Table B.5. In all three scenarios, the estimated coefficients are statistically insignificant, indicating that no effect on reported estate is detected.

- Take a given period and assume a placebo reform year
- Define treatment using the pre-period
- Train an algorithm and apply to all periods

Table B.5: Treatment Effect of Placebo Tests

Period Placebo Reform	2009-2013 2012	2010-2014 2013	2011-2015 2014	2012-2016 2015
Treated#Post	-0.0548 (0.0361)	-0.0463 (0.0404)	0.0227 (0.0398)	-0.0265 (0.0321)
Num of obs	44,901	42,701	45,569	46,322

Notes: Click here to return to the text.

B.5 Robustness check: Anticipation

Table B.6 presents the estimated elasticities assuming that there is an anticipatory behavior from donors. We..

- Define reform year 2016 (or 2008)
- Retrain the algorithm and re-predict
- Estimate diff-in-diff redefining the post dummy

Table B.6: Elasticity Estimates with Anticipatory Behavior

	2017 Tax Increase		2009 Tax Cut	
	Baseline	Anticipated	Baseline	Anticipated
First stage	-0.074	-0.067	0.331	0.332
	(0.001)	(0.001)	(0.005)	(0.005)
Reduced form	-0.204	-0.166	0.154	0.190
	(0.029)	(0.036)	(0.044)	(0.048)
Elasticity	2.757	2.478	0.465	0.572
	(0.394)	(0.539)	(0.133)	(0.145)

Notes: Click here to return to the text.

B.6 Robustness check: Alternative Specification for Elasticities

Average tax rates. Instead of using marginal tax rates in the first stage to compute the elasticity of reported estates with respect to the net-of-marginal-tax rate, we use average tax rates to estimate the elasticity of reported estates with respect to the net-of-average-tax rate. We estimate the same first-stage and reduced-form specifications as in Equations 2 and 3, but replace τ_i with the average tax rate.

Table B.7: Diff-in-diff Estimates and Implied Elasticities Using Average Tax Rates

	(1)	(2)	(3)	(4)	(5)
	2017 Tax Increase	**2009 Tax Cut**			
	All	All	Repeal	Pure Decrease	
	Top 0.5%	Top 4%	Top 4%-2%	Top 2%-0.5%	Top 0.5%
First stage	-0.035	0.046	0.015	0.045	0.211
	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
	[29,576]	[25,569]	[20,918]	[19,167]	[16,634]
Reduced form	-0.204	0.131	0.130	0.204	0.154
	(0.029)	(0.016)	(0.016)	(0.021)	(0.044)
	[63,627]	[71,234]	[59,045]	[52,925]	[46,410]
Elasticity	5.829	2.848	8.667	4.533	0.730
(0.845)	(0.353)	(1.213)	(0.477)	(0.209)	

Notes: This table shows the treatment effect on log net-of-average-tax-rate change (first-stage) and on log reported estates (reduced form), and the implied elasticities of reported estates. Column (1) shows the result of the 2017 tax increase. Columns (2)-(5) are the results of the 2009 tax cut where (2) uses the treated group top 4%. Column (3)-(5) split the top 4% into top 4%-2% (repeal), top 2%-0.5%, and top 0.5%, respectively. Standard errors are in parentheses and the number of observations are in brackets. Click here to return to the text.

IV-2SLS. Instead of using only the pre-period donor samples to run the first-stage as in Sepcificaiton X. We estimate the following IV-2SLS regressions:

$$\log(1-\tau_i) = \gamma_0 Treated_i \times Post_i + \gamma_1 Treated_i + \gamma_2 Post_i + \mu_i$$
$$\log Estate_i = \varepsilon \widehat{\log(1-\tau_i)} + \alpha_0 Treated_i + \alpha_1 Post_i + v_i$$

where τ_i is the marginal tax rate faced by donor i. $Treated_i$ is 1 if i' estate is predicted to be above the top 0.5% and 0 if between the 90th and 96th percentiles. $Post_i$ is 1 if i died after the reform. We use $Treated_i \times Post_i$ to instrument the net-of-tax rate change. Our parameter of interest is ε, which indicates the elasticity of reported estates with respect to net-of-tax rate.

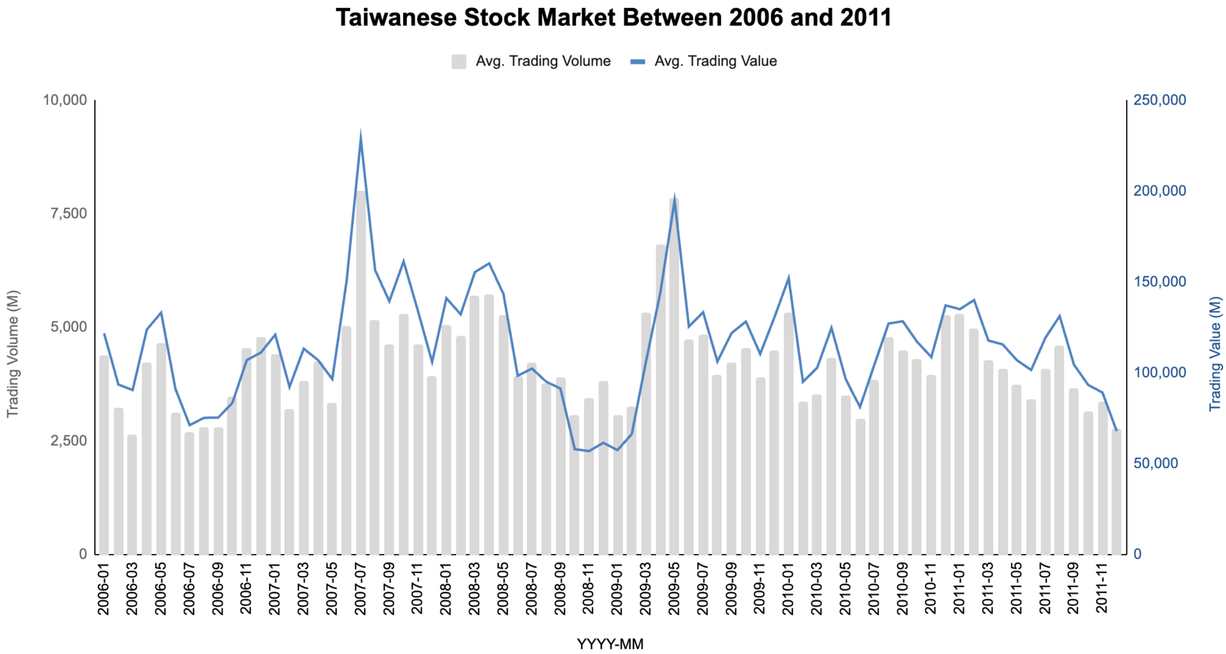
Table B.8: IV-2SLS Elasticity Estimates

	(1)	(2)	(3)	(4)
	2017 Tax Increase	**2009 Tax Cut**		
	All	All	Pure Decrease	
	Top 0.5%	Top 4%	Top 4%-0.5%	Top 0.5%
First stage	-0.043	0.097	0.069	0.328
	(0.001)	(0.001)	(0.001)	(0.002)
F test	2070.18	6542.75	4521.91	38606.29
Elasticity	4.712	1.357	2.007	0.468
	(0.543)	(0.168)	(0.232)	(0.116)
Num of obs	63,627	71,234	68,397	46,410

Notes: Click here to return to the text.

B.7 Robustness: Macro Economic Condition

Figure B.4: Taiwanese Stock Exchange Trading Volume and Values Between 2006 and 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/`.

Table B.9: Elasticity Estimates Split by Pre-reform Fraction of Stockholding, Top 0.5%

	Low Stock	High Stock
First stage	0.312	0.376
	(0.006)	(0.008)
	[13,426]	[3,208]
Reduced form	0.142	0.171
	(0.050)	(0.088)
	[36,937]	[9,473]
Implied Elasticity	0.455	0.455
	(0.160)	(0.234)

Notes: Click here to return to the text.

B.8 Robustness check: Gifting

Table B.10: The Effect of Reforms on Gifts Within Two Years Before Death

	(1) 2017 Tax Increase	(2) 2009 Tax Cut
Treated#Post	-0.0267***	-0.0002
	(0.0073)	(0.0073)

Notes: Click here to return to the text.

Table B.11: 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 $\Rightarrow$ **No change**	Gifting can reduce estate, but gifting is now more costly $\Rightarrow$ **Ambiguous**. Could gift more or less
Gifting vs. Other Avoidance	Want to switch gifting to other avoidance due to higher cost of gifting $\Rightarrow$ **Gift less**	Want to switch gifting to other avoidance as gifting is now more costly $\Rightarrow$ **Gift less**
Overall Prediction	**Gift less**	**Ambiguous**
Panel B: 2009 Both Taxes Decrease		
Estate vs. Gifting	Gifting does not reduce estate $\Rightarrow$ **No change**	Gifting reduces estate, but estate is now cheaper $\Rightarrow$ **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 $\Rightarrow$ **No change**	May switch other avoidance to gifting due to reduced cost of gifting $\Rightarrow$ **Gift more**
Overall Prediction	**No change**	**Ambiguous**

Notes: Click here to return to the text.

Table B.12: The Effect of Reforms on Gifts Within Two Years Before Death

	(1)	(2)	(3)	(4)
	2017 Tax Increase		2009 Tax Cut	
	Non-sudden	Sudden	Non-sudden	Sudden
Treated#Post	-0.0264***	-0.0720	-0.0001	0.0404*
	(0.0073)	(0.0513)	(0.0073)	(0.0191)

Notes: Click here to return to the text.

C Appendix to Section 4: Mechanism

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

Table C.1: Elasticity by Items Computation, Top 0.5% 2017 Tax Increase

	(1)	(2)	(3)	(4)	(5)
	2017 Tax Increase				
	Housing	Financial	Deposit	OthDed	Charit/Exemp
Panel A: First stage (Dependent variable: $\log(1-\tau)$)					
Treated#Post	-0.074	-0.074	-0.074	-0.074	-0.074
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Reduced form (Dependent variable: item values)					
Treated#Post	32.262	-4256.752	-1124.073	1108.972	1329.51
	(973.312)	(535.961)	(254.957)	(770.42)	(372.742)
Pre-period mean	72698.56	20603.28	8130.71	27942.14	7512.00
Rel. change	0.0005	-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: Click here to return to the text.

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

	(1)	(2)	(3)	(4)	(5)
			2009 Tax Cut		
	Housing	Financial	Deposit	OthDed	Charit/Exemp
Panel A: First stage (Dependent variable: $\log(1-\tau)$)					
Treated#Post	0.331	0.331	0.331	0.331	0.331
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Panel B: Reduced form (Dependent variable: item values)					
Treated#Post	-1133.037	480.778	551.258	-1892.939	-1967.525
	(1049.019)	(480.778)	(551.258)	(915.876)	(375.244)
Pre-period mean	61155.57	11447.78	4434.14	31734.51	7479.61
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)

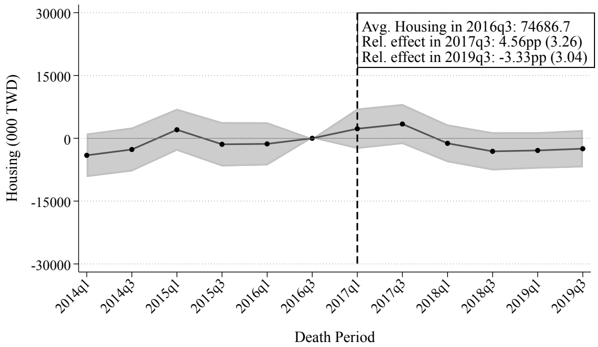
Notes: Click here to return to the text.

C.2 Sources of Responses: Event Study Graphs by Items

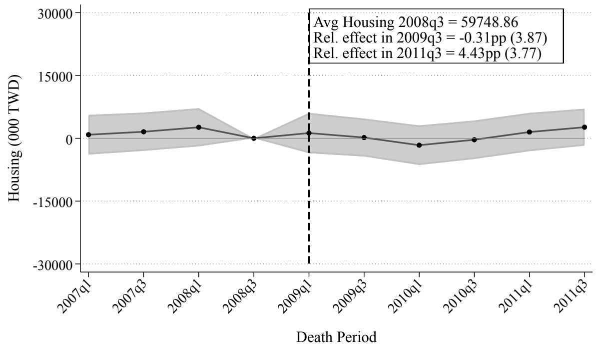
Figure C.1: Margins of Adjustment in Asset (Treated = Top 0.5% Donors)

I. Housing

(A) 2017 Tax Increase

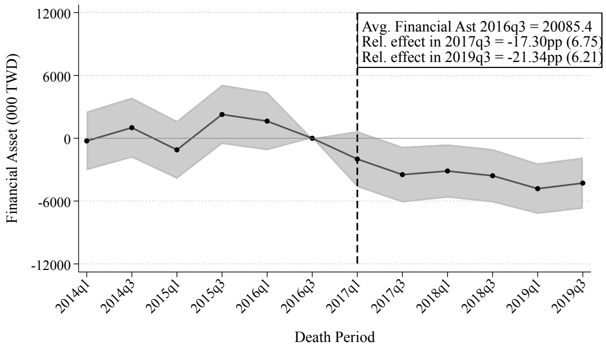


(B) 2009 Tax Cut

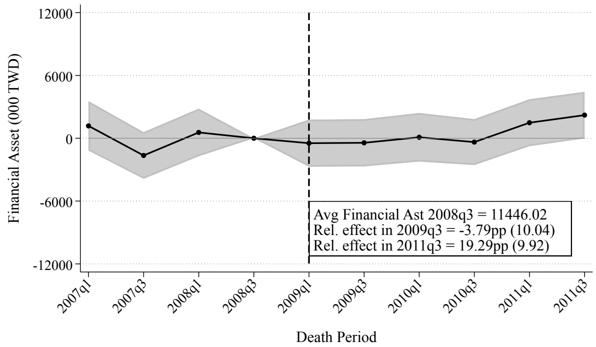


II. Financial Assets & Others

(C) 2017 Tax Increase

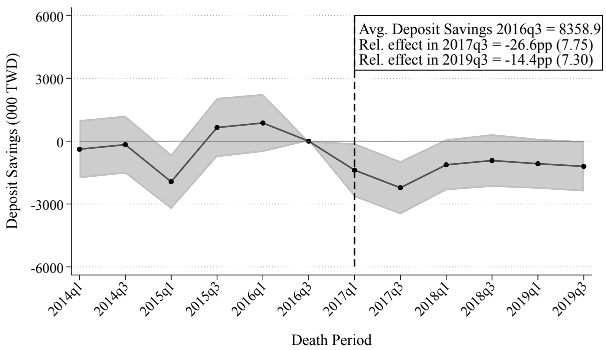


(D) 2009 Tax Cut



III. Deposit Savings

(E) 2017 Tax Increase



(F) 2009 Tax Cut

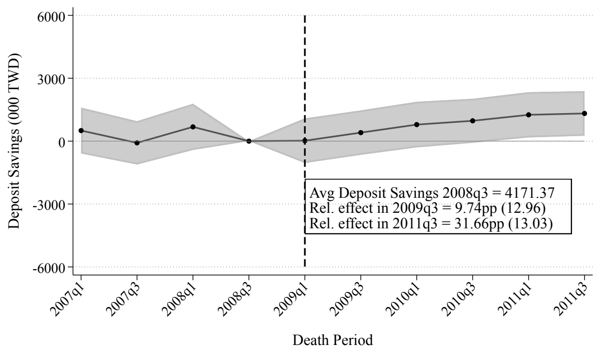
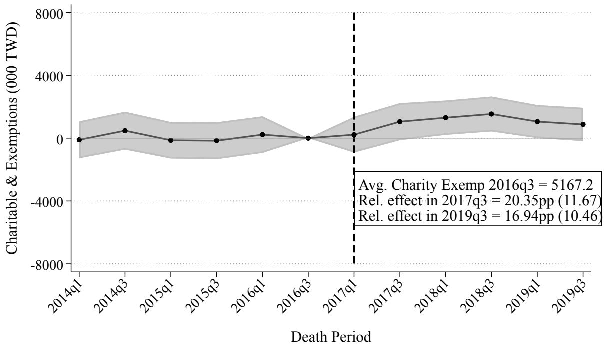


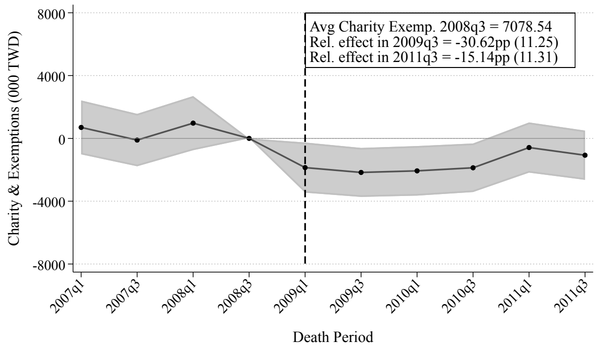
Figure C.2: Margins of Adjustment in Deduction (Treated = Top 0.5% Donors)

I. Charitable & Exemptions

(A) 2017 Tax Increase

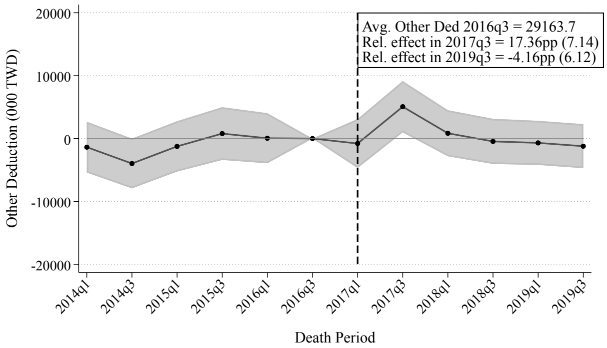


(B) 2009 Tax Cut

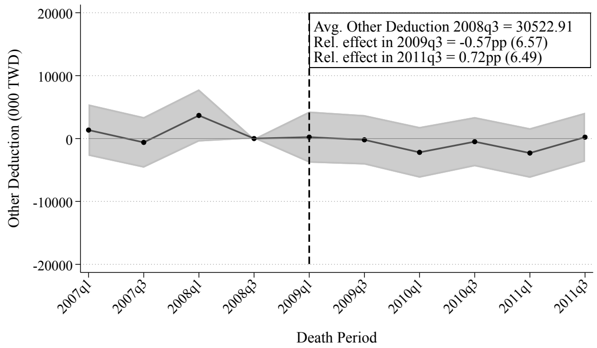


II. Other Deductions

(C) 2017 Tax Increase

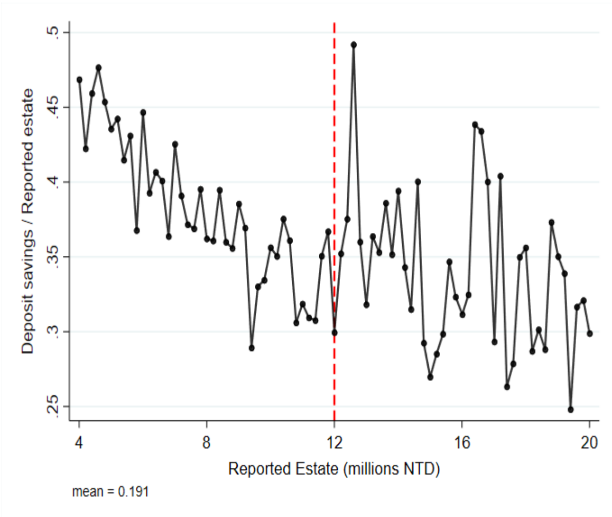


(D) 2009 Tax Cut

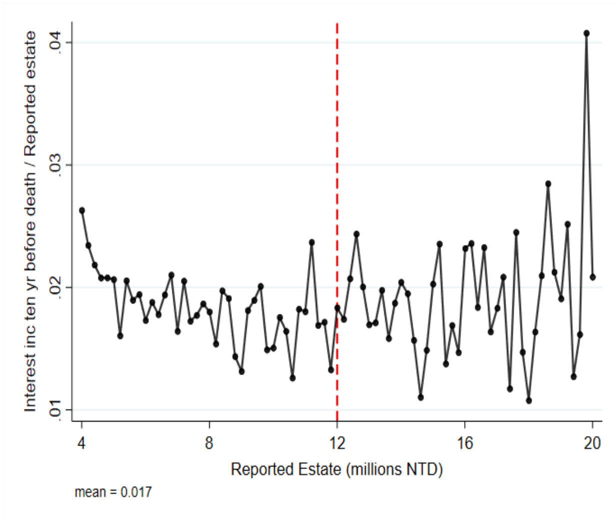


C.3 Source of Responses: Discontinuity at Reported Deposit Savings

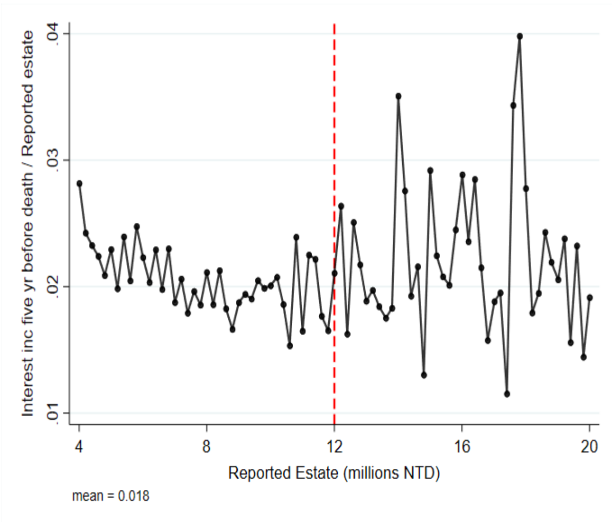
Figure C.3: Bunchers Reported Savings At Death and Interest Income Before Death (Below Death Age 60)



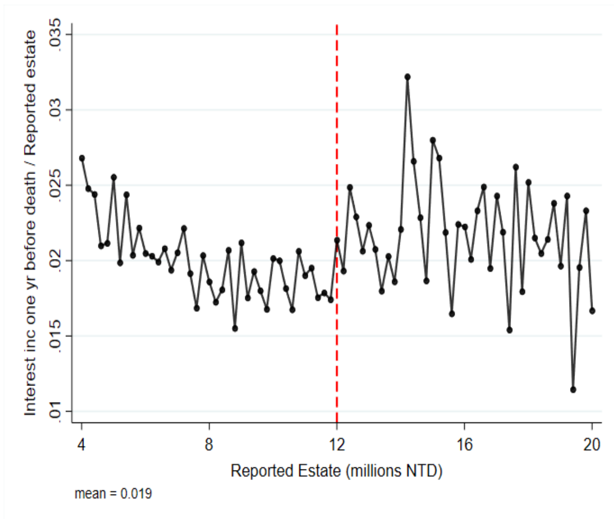
(A) Savings Reported At Death



(B) Interest Income One Year Before Death



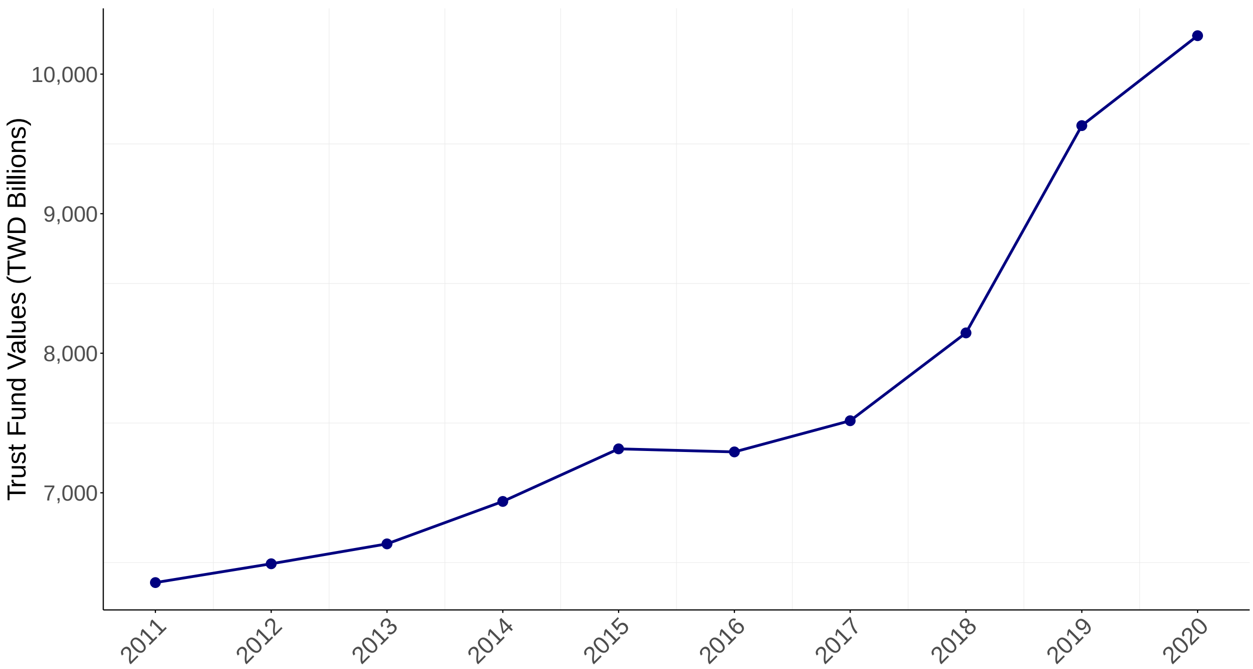
(C) Interest Income Five Years Before Death



(D) Interest Income Ten Years Before Death

C.4 Aggregate Trust Fund Sizes Over Time

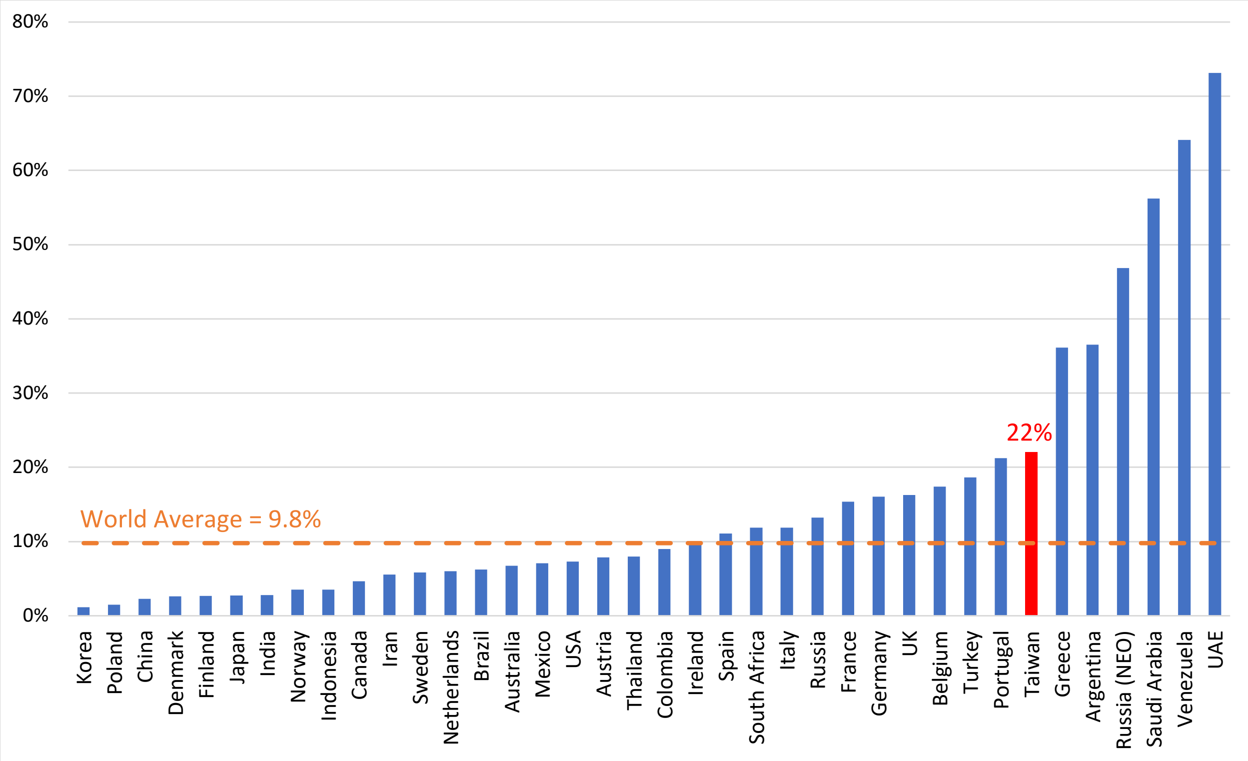
Figure C.4: Trust Fund Sizes in Taiwan Over Time



Notes: Data source: https://www.trust.org.tw/tw/about/annual-reports. Click here to return to the text.

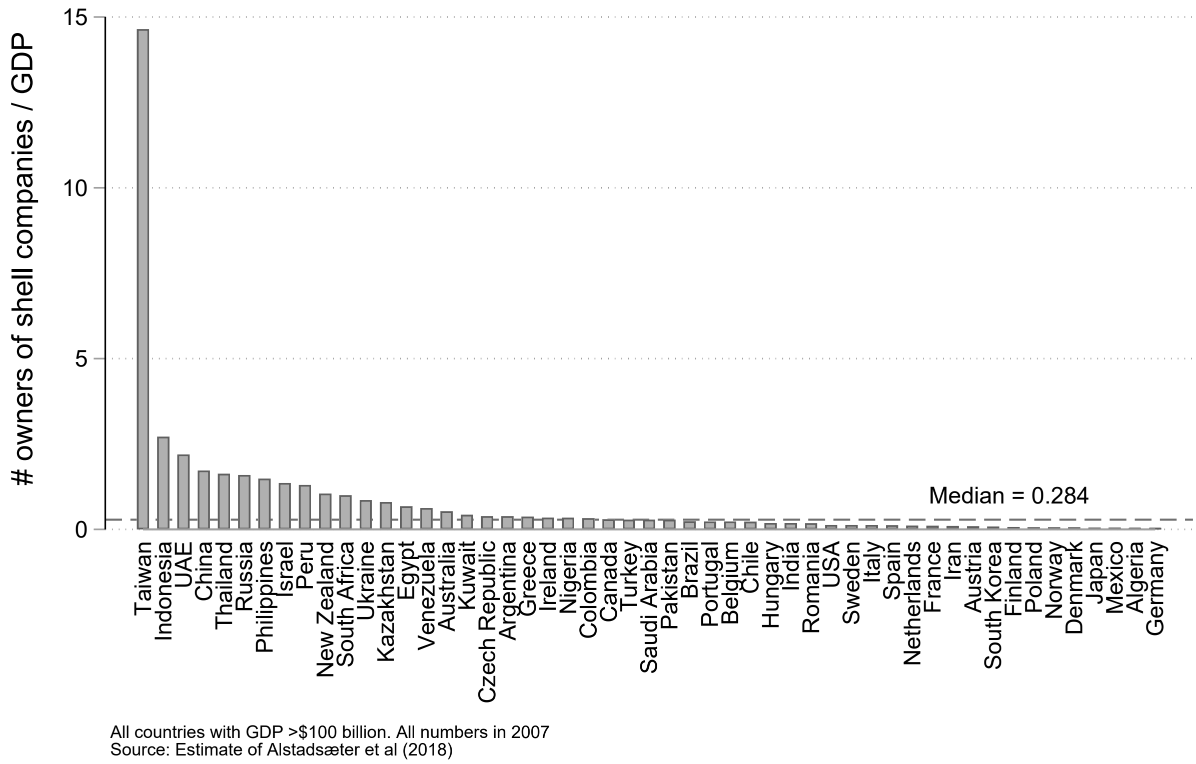
C.5 Taiwanese Fraction of Offshore Wealth

Figure C.5: Taiwanese Fraction of Offshore Wealth



Notes: Data source Alstadsæter et al. (2018). Click here to return to the text.

Figure C.6: Taiwanese Fraction of Offshore Wealth



Notes: Data source Alstadsæter et al. (2018). Click here to return to the text.