The Impact of Fiscal Policy on Financial Institutions, Asset Prices, and Household Behavior

ABSTRACT

Tax incentives on financial activities have a major impact on the U.S. government budget and currently amount to $1 trillion in foregone tax revenue per year. Motivated by this fact, this paper uses a novel empirical framework to study the role of fiscal policy as a driver of capital flows in financial markets, the cost and supply of financial products, and asset prices. Using fiscal reforms that shifted household demand for financial products, I document a large and persistent response of capital flows to tax incentives, accounting for up to 53% of the long-term growth of aggregate financial sectors. These shifts in capital allocation have persistent effects on the cross-section of asset prices, with securities that are held relatively more by subsidized institutions outperforming by 13 percentage points in the three years post-reform. The tax savings introduced by fiscal reforms, however, do not entirely accrue to households, as financial institutions retain up to 21% of these savings by increasing the cost of their products when market entry is limited. Given the large response of capital flows to tax incentives, I then investigate whether this response differs across individual U.S. households. I find that fiscal policy carries substantial distributional effects, which tend to benefit wealthier households. Having access to sophisticated advisors, ultra-high-net-worth households are significantly more responsive to tax incentives and earn a tax alpha of 50 bps annually relative to less-wealthy households. Overall, these findings highlight the importance of fiscal policy in shaping financial markets, with wealthier households benefiting from tax incentives more than less-wealthy households.

1 Introduction

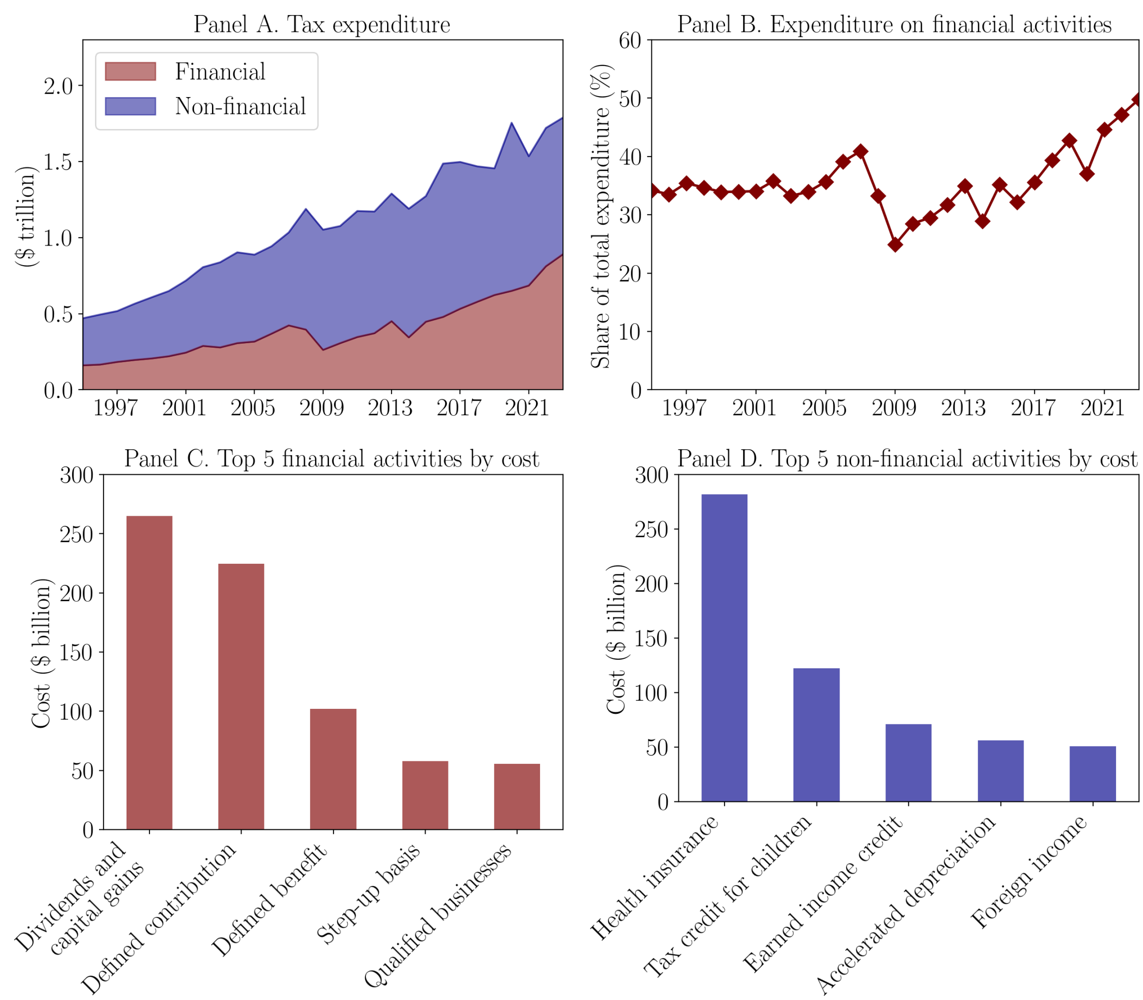
Tax incentives on financial activities have a major impact on the U.S. government budget and changed substantially over time. Federal tax expenditures – defined as revenue losses due to special exclusions, exemptions, or deductions – have increased from $500 billion per year in 1995 to almost $2 trillion per year in 2023 (Figure 1, Panel A). Even more striking is the share of total tax expenditures allocated to financial activities, which has risen from 34% to 50% over the same period (Figure 1, Panel B). Despite the magnitude and growing importance of tax expenditures on financial activities, there is limited evidence on how fiscal policy affects the structure of the financial system, asset prices, and whether the response to tax incentives differs across households.

The objective of this paper is to fill this gap and provide answers to three connected questions. How does fiscal policy shape the structure of the financial system, including capital flows across financial institutions, competition, and fees charged? As institutions hold different portfolios of assets and the products they offer are taxed at different rates, what is the impact of fiscal policy on asset prices? If capital flows respond strongly to tax incentives, are there large differences in the response across households? Answers to these questions are key to understand the impact of tax incentives on the government budget, financial stability, and ultimately household welfare. I show that fiscal policy is a key driver of the structure of the U.S. financial system, has persistent effects on asset prices, and carries substantial distributional effects, which tend to benefit wealthier households.

Given the importance for the government budget, I focus throughout on the two largest sources of tax expenditures on financial activities – defined contribution plans and dividend/capital gain taxes. These two expenditures amounted to $489 billion in foregone tax revenue in 2023 alone (Figure 1, Panel C). To put this number in perspective, it was two times higher than the revenue loss due to subsidies for health insurance and four times higher than the revenue loss due to tax credits for children (Figure 1, Panel D), two of the most extensively studied federal expenditures (Poterba, 2011).

This paper makes three main contributions. First, using a novel empirical design, I show that fiscal reforms that shift household demand for financial products have large and persistent effects on capital flows, accounting for up to 53% of the long-term growth of aggregate financial sectors. However, the tax savings introduced by fiscal reforms do not entirely accrue to households: for every dollar of tax savings, financial institutions retain up to 21 cents by increasing the cost of their products when market entry is limited. Second, as financial institutions hold different portfolios of assets, the capital reallocation induced by fiscal reforms has large and persistent effects on asset prices: securities that are held relatively more by subsidized institutions outperform by 13 percentage points over three years after the reform is passed. Third, using unique household-level data with broad coverage of ultra-high-net-worth (UHNW) households, I show that wealthier households are significantly more responsive to tax incentives and earn a 50 bps tax alpha compared to less-wealthy households. Taken together, these analyses provide a comprehensive understanding of how fiscal policy shapes the structure of the U.S. financial system and which households stand to benefit from tax incentives on financial activities.

Figure 1: **Tax expenditures on financial and non-financial activities**
All information is from the annual reports published by the Joint Committee on Taxation and the Department of the Treasury.



In the first part of the paper, I use all fiscal reforms passed by the U.S. government over the last 25 years and study their effects on multiple sectors for which I observe detailed institutional data: (i) the market for retirement savings; (ii) real estate markets; (iii) the life insurance industry; (iv) the asset management industry. The fiscal reforms include the 2001 Economic Growth and Tax Relief Reconciliation Act, which increased contribution limits on defined contribution plans and decreased personal income tax rates; the 2003 Jobs and Growth Tax Relief Reconciliation Act, which reduced dividend and capital gain tax rates; the 2012 American Taxpayer Relief Act, which increased capital gain tax rates; and the 2017 Tax Cuts and Jobs Act, which reduced personal and corporate income tax rates. The study of multiple reforms and multiple institutional sectors allows me to estimate the response of capital flows, market entry, and fees to four key policy parameters: contribution limits on defined contribution plans as well as income, dividend, and capital gain tax rates. I assemble data from several sources and, within each broad institutional sector, I identify the financial products affected by each reform. These products include retirement share classes offered in defined contribution plans, real estate trusts, ETFs, and variable annuities offered by life insurance companies.[1]

Using difference-in-differences designs around the passage of the reforms, I find that fiscal policy has large effects on capital flows across financial institutions. Quantitatively, fiscal reforms increased the annual growth rate of assets under management by 22.56 percentage points for retirement share classes, 4.15 percentage points for real estate trusts, 9.37 percentage points for ETFs, and 5.54 percentage points for variable annuities over two to four years. To make these estimates comparable, I convert them into elasticities and I rescale them by the percentage change in the relevant policy parameter. I find that a 1% increase in contribution limits increases aggregate demand for retirement share classes by 0.83%. A 1% decrease in income and dividend tax rates increases aggregate demand for variable annuities and real estate trusts by 1.60% and 1.59% respectively. A 1% increase in capital gain tax rates increases aggregate demand for ETFs by 1.56%.

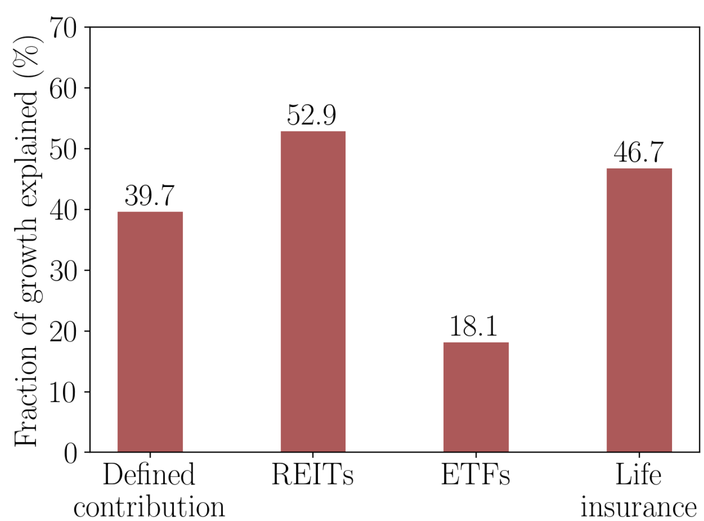
While these elasticities have not been estimated in previous work, it is useful to compare them with other elasticities to tax rates estimated in the literature. I find them to be three times higher than the elasticity of realized capital gains to changes in the capital gain tax rate (Agersnap and Zidar, 2021) and up to ten times higher than the elasticity of taxable wealth to changes in the wealth tax (Jakobsen et al., 2020). These differences have important implications for the design of fiscal policy: while households may be reluctant to reduce trading in financial markets or to reduce wealth accumulation in response to an increase in taxes, they actively re-optimize their portfolio allocation across financial products to save taxes going forward.

To evaluate the impact of fiscal policy on the long-term structure of the U.S. finan-

[1] As discussed in Section 3, the institutional details of these products make them ideal candidates for studying the effect of changes in contribution limits on defined contribution plans as well as dividend, capital gain, and income tax rates.

cial system, I then compare the effects of fiscal reforms with the cumulative capital flow observed for each aggregate institutional sector over two decades, between 2000Q1 and 2020Q4. Figure 2 provides a preview of the result: the response of capital flows to fiscal reforms explains up to 53% of institutions' long-term growth. This result shows that fiscal reforms have not only a large but also a persistent effect on capital flows, making fiscal policy a key driver behind the long-term structure of the U.S. financial system. Because different institutional sectors face different regulations, funding structures, and capital requirements, this result highlights a strong interaction between fiscal policy, financial development, and financial stability.

Figure 2: **Fraction of long-term growth explained by fiscal policy**
This figure reports the fraction of long-term growth of aggregate financial sectors explained by fiscal policy, where the long-term growth of each sector is computed as the cumulative value-weighted flow observed between 2000Q1 and 2020Q4. Further details on these calculations are provided in Section 4.



Given these large shifts in demand, I then examine whether financial institutions respond to fiscal reforms by adjusting their fee structures. This analysis is important to understand whether part of household tax savings are captured by financial institutions. I find that, depending on the cost of entry, the margin of adjustment is either on the supply or on the cost of financial products. Specifically, when the effect on market entry is limited, institutions increase their fees by 11 basis points on average following favorable reforms. This effect is driven by institutions that enter the market after the reform is passed which increase fees by 19 basis points. To put these numbers in perspective, for every dollar of tax savings introduced by fiscal reforms, financial institutions retain 12 cents on average, and new institutions retain up to 21 cents. However, when reforms trigger significant entry of new institutions, the increase in fees is not significant, consistent with a rise in competition. Hence, imperfect competition due to entry costs is key to understand how tax savings introduced by fiscal reforms are shared between households and the financial sector.

In the second part of the paper, I investigate how the capital reallocation driven by fiscal reforms affects asset prices. This analysis is motivated by a growing literature that highlights the importance of financial institutions for asset prices because of differences in capital constraints, investment mandates, or funding structures.[2] I focus on the 2012 increase in capital gain tax rates, which provides the cleanest identification strategy.

Specifically, I use insights from the benchmark inclusion (Shleifer, 1986) and Bartik instrument (Goldsmith-Pinkham et al., 2020) literatures to measure how much the demand of individual assets is expected to increase as a consequence of fiscal reforms. Intuitively, the expected increase in demand is higher for assets that have a higher weight in the portfolio of financial institutions that benefit from fiscal reforms. I find that when demand increases by 1% on impact of a fiscal reform then returns increase cumulatively by 12.71 percentage points over three years after the reform is passed. While the implied multiplier of 1.06 is within the range of estimates proposed by a growing literature on demand estimation in financial markets (Gabaix and Koijen, 2022), the time horizon that I consider is significantly longer, suggesting that fiscal reforms have not only a very persistent effect on capital allocation but also on the cross-section of asset prices. Moreover, I find that the shape of the response is hard to explain by existing macro-finance models which would generally imply a discontinuous jump of prices on impact of the reform. On the contrary, I find that prices keep increasing at relatively constant rates for several years before flattening out.

Having documented substantial effects of fiscal reforms on capital flows, fee structures, and asset prices, the third part of the paper studies heterogeneity across U.S. households in the response to tax incentives. This analysis is key to understand the distributional implications of fiscal policy, but it has been generally limited by data availability. To overcome this limitation, I use detailed household-level data from Addepar, a wealth management platform that currently covers around $6 trillion in assets advised by over 1,000 different financial advisors.

The Addepar dataset has several ideal features. It includes holdings, flows, returns, realized and unrealized gains at high frequency, for virtually all asset classes, and across the wealth distribution, including importantly UHNW households with wealth above $100

[2] See for instance He and Krishnamurthy (2013) and Brunnermeier and Sannikov (2014) for the importance of capital constraints, Koijen and Yogo (2019) for investment mandates, and Chodorow-Reich et al. (2021) for the importance of funding structures.

million. I leverage the high-frequency nature of the data and estimate the propensity of different households to optimize capital gain taxes via tax-loss harvesting. The focus on capital gain taxes and tax-loss harvesting is motivated by two reasons. As discussed in Figure 1, capital gain taxes are part of the largest tax expenditure and therefore of first-order importance for the U.S. federal budget. Second, recent years have witnessed an unprecedented growth in assets managed through tax-aware accounts which have surpassed $2 trillion. This growth has been strongly driven by private banks, which are increasingly specializing in the optimization of capital gain taxes for wealthy households and aggressively entering this market through acquisitions of tax-oriented financial advisors.[3]

I estimate that households in the top wealth quintile realize three times more losses than households in the bottom quintile, indicating greater tax awareness. This behavior is particularly visible in small interval around zero unrealized gains where, differently from households in the bottom quintile, wealthier households realize discontinuously significantly more losses than gains. This difference in the response to tax incentives across the wealth distribution is not explained either by differences in marginal tax rates or by differences in the share of assets allocated to retirement plans.

I then examine the mechanism behind this heterogeneity in the response to tax incentives. I find that access to tax-sophisticated advisors (private banks and family offices) is strongly correlated with the higher tax awareness of wealthier households. Moreover, consistently with recent industry trends, private banks are significantly more effective than family offices in optimizing capital gain taxes, although both demonstrate higher tax sophistication compared to other types of advisors (e.g. independent advisors and broker-dealers).

Finally, I investigate whether this difference in the response to tax incentives has meaningful implications for heterogeneity in after-tax returns across the wealth distribution. I estimate that UHNW households deduct, on average, 60% of their realized gains using realized losses, compared to 30% for less-wealthy households. In terms of performance, households at the low end of the wealth distribution pay about 1% annually in capital gain taxes relative to their wealth, compared to 0.50% for UHNW households. This tax alpha of 50 basis points per year is economically meaningful, especially considering that it can be achieved for virtually any desired risk exposure and with minimal incremental risk.

Taken together, the three parts of this paper have the objective to paint a comprehensive picture of how fiscal policy shapes the financial system. Fiscal reforms are followed by significant changes in the structure of the financial industry, which in turn affects fee struc-

[3] See, for instance, JPMorgan's acquisition of 55ip in 2020 and Morgan Stanley's acquisition of Parametric in 2021.

tures and asset prices. However, the benefits of these changes are not evenly distributed, with wealthier households better positioned to take advantage of tax incentives. These findings have important implications for the design of fiscal policy and its role in shaping both the financial system and wealth inequality.

Related literature. This paper is related to a large literature that has studied the importance of financial institutions for saving decisions (Madrian and Shea, 2001; Chetty et al., 2014), for the dynamics of risk exposure over the life cycle (Parker et al., 2022), for the development (Scharfstein, 2018) and the stability (He and Krishnamurthy, 2013; Brunnermeier and Sannikov, 2014) of the broader financial system, for asset prices (Nagel, 2005; Koijen and Yogo, 2019; Chodorow-Reich et al., 2021; Haddad and Muir, 2021; Gabaix and Koijen, 2022; Parker et al., 2023; Kekre et al., 2024), and for real growth (Huang and Xu, 1999; Carlin and Mayer, 2003). Compared to these papers, I link the growth of heterogeneous financial institutions to fiscal policy and show that it is a key driver behind the long-term structure of the U.S. financial system. In this sense, this paper is also related to a smaller literature that has looked at the tax efficiency of mutual funds and ETFs (Sialm and Starks, 2012; Sialm and Zhang, 2020; Moussawi et al., 2022), although I focus more broadly on the impact of fiscal policy on the structure of the U.S. financial system, including the growth of multiple institutional sectors, competition, fees, and asset prices.

The paper is also related to a literature that has studied drivers of financial development other than fiscal policy. These include the legal environment (La Porta et al., 1998), the regulation of the banking industry (Franks et al., 1997), and household demographics (Khorana et al., 2005; Mian et al., 2021). However, no previous paper has quantified the impact of fiscal policy on the long-term growth of aggregate financial sectors and, therefore, on financial stability.

The paper also connects to the literature that has studied how taxes affect household portfolio allocation (Poterba and Samwick, 2003; Bergstresser and Poterba, 2004) and whether dividend and capital gain taxes are priced both in the aggregate and in the cross-section (Sialm, 2009). Compared to these papers, I study the effect of fiscal policy on aggregate capital flows and connect the response of capital flows to movements in asset prices, both in the short term and in the long term. Moreover, early papers find that tax-loss harvesting strategies have limited effect on performance (Auerbach et al., 1998), although Ivković et al. (2005) document a strong lock-in effect of capital gain taxes. I find that tax-loss harvesting can reduce the effective tax rate by up to 60% for UHNW households relative to the statutory tax rate.

The individual-level analysis in the paper contributes to a growing literature that has studied heterogeneity in portfolio allocation (Calvet et al., 2007; Fagereng et al., 2017; Balloch and Richers, 2023), rebalancing (Barber and Odean, 2000; Hoopes et al., 2016; Calvet et al., 2009a; Fagereng et al., 2019; Gabaix et al., 2024), inertia (Calvet et al., 2009b; Andersen et al., 2020), and returns (Fagereng et al., 2020) across households, or that has used heterogeneity as a key determinant for the transmission of policy shocks to macroeconomic and financial outcomes (Auclert, 2019; Kekre and Lenel, 2022, 2024). Compared to these papers, I show that substantial heterogeneity exists in the response to tax incentives across the wealth distribution, that wealthier households are substantially more responsive, and that this has quantitatively meaningful implications for heterogeneity in individual after-tax returns.

Finally, the paper is related to a more recent literature that has used micro-level data to draw implications for optimal fiscal policy, with focus on inequality (Saez and Zucman, 2016; Piketty et al., 2018; Smith et al., 2019; Mian et al., 2020; Smith et al., 2023), on the elasticity of taxable wealth to wealth taxes (Jakobsen et al., 2020), or on the corporate sector (Zwick and Mahon, 2017; Chodorow-Reich et al., 2024). Compared to these papers, I use the high-frequency observation of unrealized and realized returns to study how different U.S. households respond to capital gain taxes and which households benefit the most from the current tax system.

Outline. The rest of the paper proceeds as follows. Section 2 discusses the construction of the datasets. Section 3 presents the effects of fiscal reforms on capital flows, fee structures, and competition for multiple institutional sectors. Section 4 evaluates the magnitudes of these effects relative to institutions' long-term growth. Section 5 discusses the effect of fiscal reforms on the cross-section of asset prices. Section 6 studies how individual households respond to tax incentives. Section 7 concludes.

2 Data

In this section, I present the data sources used in the paper. Further details on data construction and institutional background are provided in Appendix A and B.

CRSP Mutual Fund dataset. Information about retirement share classes, ETFs, variable annuities, and mutual funds are from the CRSP Survivor-Bias-Free dataset, from which I observe assets under management, returns, style, fees, the first date on which the fund was offered to the public, and the fund name at share class level. While assets under management and returns are available at monthly frequency, all other information is available at quarterly frequency starting from 1999Q1. For this reason, for each share class i and year-month t, I use information about assets under management A_{it} and returns r_{it} to construct monthly dollar flows into each share class as $F_{it} = A_{it} - A_{i,t-1}(1 + r_{it})$. I then convert assets under management, dollar flows, and returns to quarterly frequency, and I define $f_{it} = \frac{F_{it}}{A_{i,t-1}}$.[4] To avoid the incubation bias discussed in Evans (2010), I restrict the dataset to share classes for which assets under management are higher than $10 million both in the current and in the previous year-quarter.

I identify ETFs and mutual funds underlying variable annuities by using the ETF and variable annuity identifiers provided by CRSP. Differently from ETFs and variable annuities, retirement share classes are not directly identifiable in the CRSP dataset. However, in the majority of the cases, the fund name is complemented by the symbol of the share class being offered. I identify retirement share classes as those with names that include any of the following strings: "Class R", "Class R1", "Class R2", "Class R3", "Class R4", "Class R5", "Class R6", "Class R-1", "Class R-2", "Class R-3", "Class R-4", "Class R-5", "Class R-6".

Morningstar Annuity Intelligence. While comprehensive, the CRSP dataset does not correctly identify variable annuities before 2008. I therefore complement the CRSP dataset with information from Morningstar Annuity Intelligence. This dataset provides quarterly information at the contract level for the universe of variable annuities. I merge this information into the CRSP dataset and use it to correctly identify variable annuities before 2008.

CRSP-Compustat dataset. Information about individual securities and REITs are from the CRSP Monthly Stock dataset, which provides shares outstanding, market price, returns, SIC code, and share code at security level. I identify REITs as securities with share code 18 and I follow Boudoukh et al. (2007) and Belo et al. (2019) to construct monthly net equity issuance as $F_{it} = (S_{it} - S_{i,t-1})(P_{it} + P_{i,t-1})/2$, where S_{it} denotes the split-adjusted number of shares outstanding for security i in year-month t and P_{it} the split-adjusted price.[5] Conceptually, I interpret net equity issuance for REITs as the equivalent of net flows for retirement share classes, ETFs, and variable annuities. To ensure that the frequency is the same across all datasets, I convert net equity issuance of each REIT i to quarterly frequency by summing it across year-months in each year-quarter. I then define $f_{it} = \frac{F_{it}}{A_{i,t-1}}$, where A_{it} denotes the market capitalization of REIT i at the end of year-quarter t.

[4] I convert assets under management A_{it} to quarterly frequency by keeping the last value observed for each share class in each year-quarter. I convert dollar flows F_{it} by summing the equivalent monthly measure across year-months in each year-quarter for each share class. Finally, I construct quarterly returns for each share class i as $\prod_t (1 + r_{it})$, where the product includes all returns observed for each share class in each year-quarter.

[5] The main advantage of this measure of net equity issuance is that it is widely available across securities and at higher frequency compared, for instance, to equivalent measures that can be constructed from the Compustat North America Fundamentals dataset. However, none of the results in the paper are affected if I use the measure of net equity issuance in Jensen et al. (2022).

Whenever required, I obtain firm characteristics from the Compustat North America Fundamentals Annual and Quarterly datasets. All characteristics are constructed as in Jensen et al. (2022) and information is lagged by two year-quarters to ensure it is publicly available.

Ziman REIT dataset. The CRSP dataset does not provide information on the type of investments in which REITs specialize (e.g., commercial real estate, residential real estate, mortgages, MBS). For this reason, I augment the CRSP-Compustat dataset with information from the Ziman REIT database, which includes details on the types of investments in which different REITs specialize.

Addepar. Household-level data are from Addepar, a wealth management platform that specializes in a variety of services for financial advisors, including aggregation, analytics, and performance reporting. The dataset includes dollar holdings, flows, realized and unrealized returns at security level and at monthly frequency between January 2016 and March 2023. Moreover, it covers direct and indirect positions (e.g. mutual funds and ETFs) in both publicly traded and private asset classes. The observation of realized and unrealized returns at relatively high frequency makes this dataset ideal to study the response to capital gain taxes by individual U.S. households, including UHNW households that are typically underrepresented in alternative data sources.

Importantly, Addepar reconstructs the history of unrealized and realized returns directly based on the transactions and the purchase prices uploaded daily by clients, including new clients that join the platform, for which the history of transactions is backfilled. This feature of the dataset ensures that, differently from alternative data sources, no assumption about the purchase or sale price is required to construct unrealized and realized returns, which are therefore observed with high a level of accuracy. I mostly follow the selection process in Gabaix et al. (2024) to correct infrequent data errors and I provide additional details in Appendix A.2.

3 How does fiscal policy shape the financial industry?

I now turn to discussing how fiscal policy affects structure of the financial industry, including capital flows across financial institutions, competition, and fees charged. I first outline the general empirical framework in Section 3.1. I then study the effect of changes in contribution limits in Section 3.2, followed by income tax rates in Section 3.3, dividend tax rates in Section 3.4, and capital gain tax rates in Section 3.5. Section 3.6 compares these estimates and discusses policy implications.

3.1 Empirical framework

In Sections 3.2 to 3.5, I study the effects of fiscal reforms on capital flows, competition, and fees charged by multiple institutional sectors. Given the importance for the government budget, I focus on fiscal reforms that changed four key policy parameters: (i) contribution limits on defined contribution plans; (ii) personal income tax rates; (iii) dividend tax rates; (iv) capital gain tax rates.[6]

To estimate the effect of fiscal reforms, I use a DiD design and the following baseline regression

$$y_{it} = \alpha_{s(i)t} + \gamma_t \mathbb{I}\left(i \in Treatment\right) + \beta' X_{it} + \varepsilon_{it}, \tag{1}$$

where y_{it} denotes a generic outcome variable of interest, $\alpha_{s(i)t}$ denotes style times year-quarter fixed effects, $\mathbb{I}\left(i \in Treatment\right)$ is an indicator equal to 1 if institution i is treated by the reform and zero otherwise, $\gamma_t \mathbb{I}\left(i \in Treatment\right)$ interacts year-quarter fixed effects with the treatment status, and X_{it} denotes a set of observable controls.[7] In the baseline design, I use style fixed effects to control for differences across institutions in the characteristics of their portfolios of assets. All results are, however, robust when I include institution fixed effects.

As usual in DiD designs, the estimate $\widehat{\gamma}_t$ identifies the average treatment effect on the treated institutional sector under two assumptions: (i) in absence of the reform, treatment and control groups would have followed parallel trends; (ii) the control group is not affected by the reform. For each reform, I will discuss in details these assumptions as well as the definition of the treatment and control groups in the next sections.

3.2 The effect of changes in contribution limits

I start from studying how fiscal policy affects defined contribution plans, the second largest source of tax expenditure on financial activities for the U.S. federal government, accounting for \$225 billion in foregone tax revenue in 2023.

Details on the financial institution. Defined contribution plans (e.g., 401(k) plans and IRAs) offer several tax benefits. Contributions can be deducted from individual taxable in-

[6] Multiple reforms (surveyed in Appendix C) changed the four policy parameters of interest. Out of these reforms, I first exclude those not covered by any of the datasets used in the paper. This rules out reforms enacted before 1999, although I provide qualitative evidence on the effect of earlier reforms in Appendix C using aggregate data from the Fed Financial Accounts. Among the reforms enacted after 1999, I then select as baseline reform the one that minimizes the presence of contemporaneous confounding effects.

[7] Controls include the log of lagged assets $\log A_{i,t-1}$, lagged return $r_{i,t-1}$, turnover as observed in CRSP, the age of the institution. The latter is computed as the number of year-quarters between the institution first appeared on the market the current year-quarter t.

come and the taxation of dividends, interest income, and realized capital gains is deferred until retirement. However, to limit the use of these plans for tax avoidance, contributions are subject to annual limits that apply separately for employers and employees.

Details on the fiscal reform. If households value the tax benefits associated to defined contribution plans, we would expect that capital flows to defined contribution plans increase in the maximum contribution limits. I test this hypothesis using the unprecedented increase in contribution limits introduced by the 2001 Economic Growth and Tax Relief Reconciliation Act.[8] This reform increased total contribution limits on both 401(k)s and IRAs for the first time since 1982. The total contribution limit on 401(k) plans was increased from $30,000 in 2000 to $35,000 for fiscal year 2001 and $40,000 for fiscal year 2002. For IRAs, the contribution limit was increased from $2,000 to $3,000 for fiscal year 2002. The reform also included a scheduled increase in contribution limits for years after 2002 and established the automatic growth of contribution limits at annual inflation rates for years not explicitly covered by the schedule.[9] The scheduled increase in contribution limits was subsequently accelerated with the passage of the 2003 Jobs and Growth Tax Relief Reconciliation Act.[10]

I test whether households value the tax benefits of defined contribution plans by estimating equation (1) around the passage of the 2001 and 2003 acts using log flows, $\log(1 + f_{it})$, as the outcome variable.[11] The treatment group includes retirement share classes, which are only accessible by households through defined contribution plans.[12] The control group includes any other share class offered by mutual funds. I will refer to the control group as non-retirement share classes.[13] To estimate the DiD, I focus on a time window that goes from 1999Q2 to 2005Q2. This window is chosen to cover two full years after the 2003 reform was signed into law.

Results. For each year-quarter in 1999Q2-2005Q2, Panel A of Figure 3 reports the cumu-

[8]The act was signed into law on June 7, 2001.

[9]This annual inflation rate is published by the IRS based on the Consumer Price Index. Moreover, for both 401(k)s and IRAs, an additional increase in contribution limits known as the "catch-up" clause was established for workers older than 50 years old. Starting from 2002, these workers were allowed to make additional tax-deducible contributions up to $1,000 for 401(k)s and $500 for IRAs. The catch-up contribution was also set to grow at the inflation rate for fiscal years after 2002.

[10]Formally signed into law on May 23, 2003.

[11]Because the estimate of interest is the cumulative effect of reforms on capital flows, I use throughout $\log(1 + f_{it})$ instead of flows f_{it} as log flows naturally aggregate over time. However, because f_{it} is on average close to zero then $\log(1 + f_{it}) \approx f_{it}$ and none of the result is quantitatively affected if I use f_{it} instead of $\log(1 + f_{it})$ as the outcome variable.

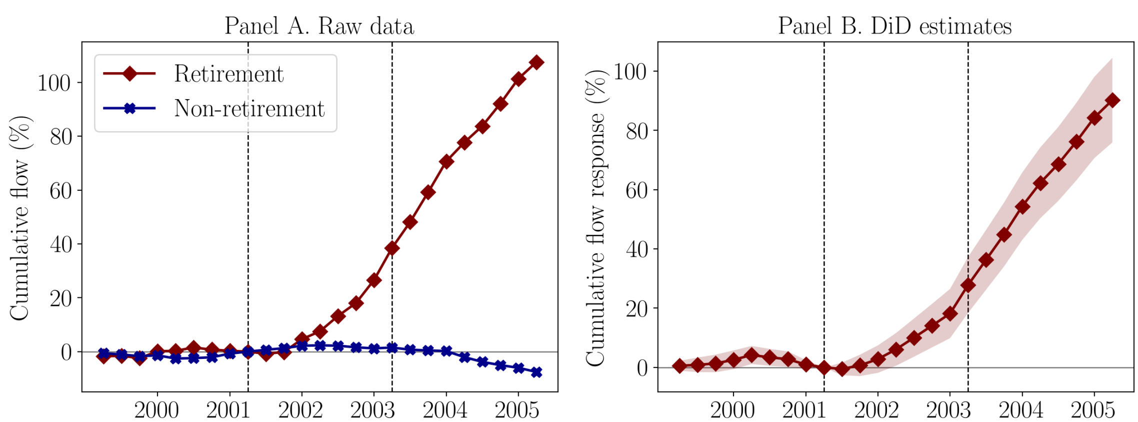
[12]Confirming this, Morningstar defines retirement share classes as "share classes that are explicitly created for retirement plans, such as 401(k)s". More information can be found at https://www.morningstar.com/funds/how-choose-among-fund-share-classes.

[13]I exclude from the control group share classes that are flagged as either variable annuities or ETFs since they may also be treated by fiscal shocks as discussed in Sections 3.3 and 3.5.

lative average log flow in the raw data for retirement and non-retirement share classes.[14] Panel B provides instead estimates of $\sum_{u=-T}^{t} \gamma_u$ from equation (1), where I use $u = -T$ throughout to denote the first year-quarter included in the estimation window. These estimates capture the cumulative effect of the reform on capital flows to retirement share classes.[15]

Capital flows to retirement and non-retirement share classes moved together until 2001Q2, when the first reform was passed. Starting from 2001Q2, flows to retirement share classes increased rapidly while the trend of non-retirement share classes remained unaffected. While mutual funds offer share classes other than retirement in defined contribution plans, the fact that the trend for non-retirement share classes did not increase after the reforms were passed suggests that the increase in contribution limits had no detectable effect on the control group. Moreover, even if the control group was affected, the DiD estimates discussed in this section would be a conservative lower bound of the true effect. Overall, based on the result in Panel B, the 2001 and 2003 reforms increased capital flows to retirement share classes by 90.23 percentage points as of 2005Q2.

Figure 3: **Effect on capital flows to retirement share classes**
Panel A reports the cumulative average log flow to retirement (treatment group) and non-retirement share classes (control group) in raw data. Panel B provides the cumulative effect on log flows to retirement share classes, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\gamma}_u$, where γ_t is defined in equation (1). In Panel B, standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



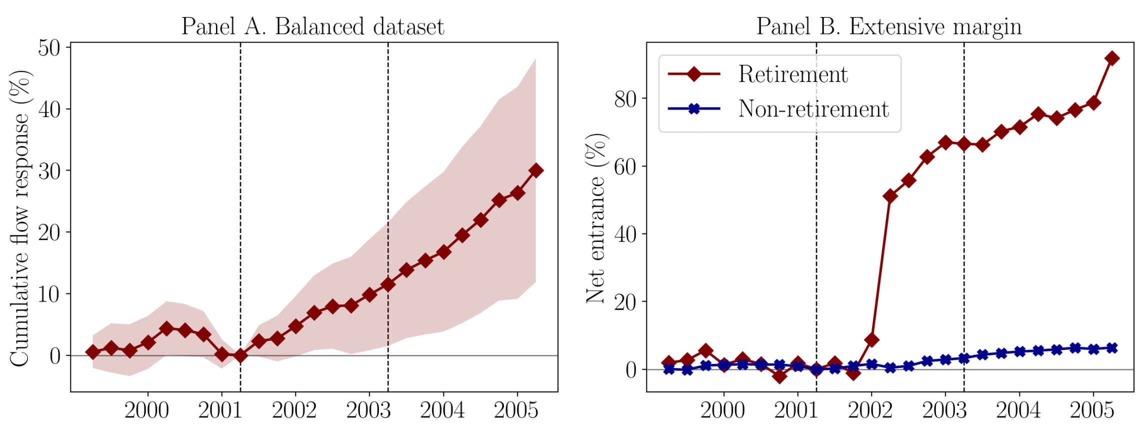
To investigate the importance of composition effects in driving this result, I repeat the

[14] For both the treatment and the control group, I construct the cumulative average log flow by first averaging the log flow in each year-quarter. I then rescale the resulting time-series by the average log flow in the period before the first reform was passed. I then sum the rescaled time-series over time.

[15] As in standard DiD designs, the effect is relative to the excluded year-quarter dummy, which I select to be 2001Q2, i.e., the year-quarter in which the reform was signed into law.

estimation after restricting the dataset to share classes alive throughout the estimation window (1999Q2-2005Q2). I report the results in Panel A of Figure 4. Although the estimate in 2005Q2 remains statistically significant, the cumulative effect of the reform decreases sharply from 90.23 to 30.05 percentage points. This sharp decrease suggests that the retirement sector experienced substantial entry of new institutions around the two reforms, i.e. a large increase in the supply of retirement share classes offered by mutual funds. This intuition is confirmed in Panel B, where I report the cumulative net entrance rate of retirement and non-retirement share classes around the passage of the two reforms.[16] The net entrance rates of retirement and non-retirement share classes moved together until the end of 2001. However, the net entrance rate of retirement share classes started to increase sharply in 2002Q1. By the end of 2002, the net entrance rate of retirement share classes increased by 62.79 percentage points. By 2005Q2, it had increased by 91.84 percentage points. In absolute terms, the number of retirement share classes offered by mutual funds increased from 167 to 271 in 2002, while the number of non-retirement share classes increased from 10,142 to 10,175.

Figure 4: **Effect on market entry of retirement share classes**
Panel A provides the cumulative effect on log flows after restricting the dataset to share classes available throughout the estimation window (1999Q2-2005Q2). Panel B reports the cumulative net entrance rate of retirement and non-retirement share classes in raw data. In each year-quarter, the net entrance rate is defined as the number of new share classes created less the number of share classes that exited the dataset. The difference is then rescaled by the total number of share classes observed in the dataset in the previous year-quarter. In Panel A, standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



[16]In each year, the net entrance rate for retirement share classes is defined as the number of new retirement share classes created less the number of retirement share classes that exited the dataset. The difference is then rescaled by the total number of retirement share classes observed in the dataset in the previous year. The definition for non-retirement share classes is analogous.

One additional question of interest concerns the impact, if any, of the reforms on the expense ratio charged by retirement share classes. The direction of the effect is in principle ambiguous as two forces act in opposite directions. The shift in demand predicts an increase in the expense ratio, provided that the retirement industry is not perfectly competitive. At the same time, the increase in market entry predicts an increase in competition and a decrease in the average expense ratio. In Appendix D.1, I show that these two opposing forces likely offset each other and the two reforms did not have a statistically significant effect on expense ratio charged by retirement share classes.

Robustness. In Appendix D.1, I also provide results from a series of robustness tests to address potential concerns about identification. First, I control for share class fixed effects to absorb unobservables at the share class level. Second, I use the fact that, between 1999Q1 and 2005Q2, around 85.47% of the retirement share classes offered on the market belonged to a mutual fund that also offered non-retirement share classes. I use this fact to strengthen the definition of treatment and control groups. Specifically, I restrict the sample to retirement and non-retirement share classes offered by the same mutual funds. I then repeat the estimation of the baseline DiD design to which I add fund fixed effects. Controlling for fund fixed effects implies that I am comparing retirement and non-retirement share classes offered by the same mutual fund and therefore identical in terms of portfolio allocation and portfolio manager. I find that results are robust and that the reforms increased capital flows to retirement share classes by 25.35 to 47.76 percentage points, depending on the specification considered.

One additional concern may involve confounding effects due to contemporaneous provisions included in the two reforms other than the increase in contribution limits. In particular, a second important provision in the 2001 Economic Growth and Tax Relief Reconciliation Act involved a decrease in personal income tax rates across all income brackets. However, because contributions to defined contribution plans can be deducted from taxable income, we would expect that a decrease in income tax rates would, if anything, decrease the tax benefit of holding securities in qualified accounts, consistent with models of asset location and allocation (e.g., Dammon et al., 2004; Gomes et al., 2009). If this is the case, the decrease in income tax rates should have, if anything, decreased capital flows to retirement share classes. The estimates in Figures 3 and 4 should then be interpreted as a conservative lower bound of the true effect. Consistent with these arguments, I show in the following section that the decrease in personal income tax rates enacted with the Tax Cut and Jobs Act (2017) was followed by an increase in capital flows to variable annuities and non-retirement share classes relative to retirement share classes.

3.3 The effect of changes in income tax rates

Since the beginning of the century, life insurance companies have become a key player in the retirement industry. This growth was mainly driven by variable annuities, which currently manage around $2 trillion in assets and that represent ideal candidates to study the effect of changes in personal income tax rates.

Details on the financial institution. As discussed in Koijen and Yogo (2022), a variable annuity is a mutual fund coupled with a longevity insurance and, potentially, a minimum guaranteed return.[17] From a tax perspective, variable annuities share many similarities with defined contribution plans. In both cases, the taxation of dividends and realized capital gains is deferred upon retirement, provided that the corresponding amount is not withdrawn. It follows that variable annuities also benefit from the tax-free compounding earned on the reinvestment of dividends and realized capital gains (Brown and Poterba, 2006). In addition, like defined contribution plans, withdrawals upon retirement are taxed at personal income tax rates.

While similar along many dimensions, variable annuities differ from defined contribution plans in two important ways. Contributions to variable annuities are not exempt from income taxes and, for this reason, there is no dollar limit on the amount that individual households can contribute every year.

Details on the fiscal reform. Because variable annuities are close substitutes to defined contribution plans but contributions are not deductible, we would expect that a decrease in personal income tax rates reduces the cost of saving for retirement through variable annuities relative to defined contribution plans.[18] I test this hypothesis by estimating the DiD design in equation (1) around the decrease in personal income tax rates introduced by the 2017 Tax Cut and Jobs Act using log flows as the outcome variable.[19] The treat-

[17]The variable annuity provides longevity insurance in that, upon retirement, the investor is entitled to withdraw a fixed amount per period until death. This fixed amount is proportional to the account balance upon retirement and is distributed to the investor even after the account balance is depleted.

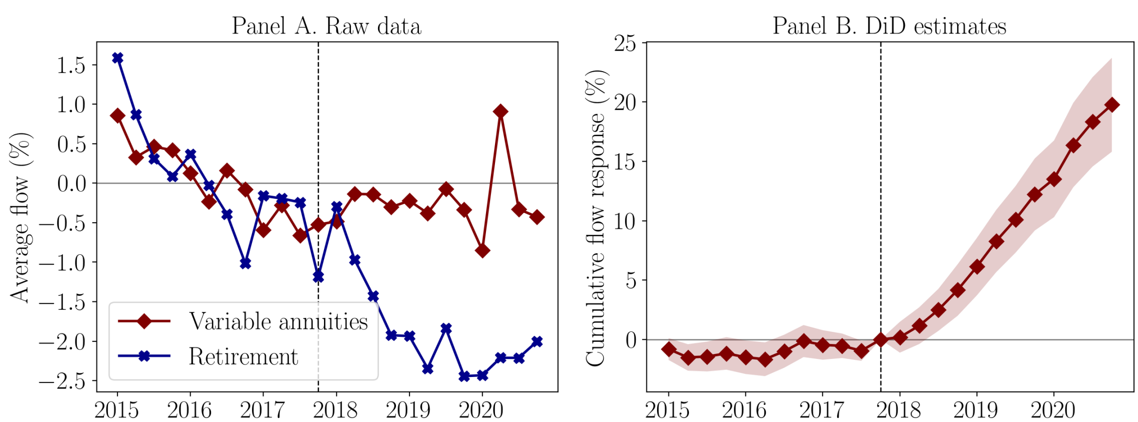
[18]While a key advantage of investing via qualified accounts is the tax-deductible nature of the contributions, the menu of accessible assets is generally limited to a subset of investment options otherwise available on the market. This is true in particular for 401(k) plans, as discussed in several previous papers (Pool et al. 2016; Badoer et al. 2020; Pool et al. 2022; Loseto 2023). If investors trade off the tax benefits offered by qualified accounts with the variety of investment options being offered, a decrease in income tax rates should reduce the cost of saving for retirement outside qualified accounts. Because variable annuities are specifically designed to save for retirement and because they preserve the deferred taxation of dividends and capital gains, we would expect that they are among the financial institutions that most benefit from a decrease in personal income tax rates.

[19]The act was signed into law on December 22, 2017. The decrease in personal income tax rates was particularly pronounced for joint filers with income between $238,000 and $315,000, for which the marginal rate decreased from 33% to 24%. For the highest income bracket, the marginal tax rate decreased from 39.6% to 37% for single filers and from 39% to 35% for married filers. The 2017 Tax Cut and Jobs Act further increased

ment group includes variable annuities while the control group includes retirement share classes. Differently from the 2001 and 2003 reforms studied in the previous section, the 2017 Tax Cut and Jobs Act was not followed by any reform that accelerated the phase-in of its provisions. For this reason, I estimate the DiD design in a three-year symmetric window around 2017Q4.

Results. In Panel A of Figure 5, I report the average log flow to variable annuities and retirement share classes in each year-quarter around the reform. Panel B provides instead estimates of $\sum_{u=-T}^{t} \gamma_u$ from equation (1). Capital flows to variable annuities and retirement share classes moved together up to 2017Q4, when the reform was passed. Starting from 2018Q1, flows to variable annuities started to increase relative to flows to retirement share classes, which remained on a downward trend throughout the estimation window. The demand for variable annuities increased further in 2020Q2, during the Covid-19 pandemic. Based on the results in Panel B, the 2017 decrease in personal income tax rates increased capital flows to variable annuities by 11.08 percentage points as of 2019Q4 and by 19.76 percentage points as of 2020Q4.

Figure 5: **Effect on capital flows to variable annuities**
Panel A reports the average log flow to variable annuities (treatment group) and retirement share classes (control group) in raw data. Panel B provides the cumulative effect of the reform on log flows to variable annuities, summarized for each quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\gamma}_u$, where γ_t is defined in equation (1). In Panel B, standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



Robustness. In Appendix D.2, I report results from two robustness tests. First, I repeat estimation in a balanced panel which is restricted to variable annuities and retirement share classes available throughout the estimation window. I find that results are robust

the exemptions for the application of the tax rates under the Alternative Minimum Tax (AMT) schedule from \$54,300 to \$84,500 for single filers and from \$70,300 to \$109,400 for married filers.

and capital flows to variable annuities increase by 17.80 and 28.45 percentage points as of 2019Q4 and 2020Q4, respectively. Second, I add share class fixed effects to the regression to absorb potential unobservables at the share class level (e.g., the minimum guaranteed return offered by some variable annuities). I find that results are robust and that the reform increased capital flows to variable annuities by 14.57 and 24.75 percentage points as of 2019Q4 and 2020Q4, respectively.

If the increase in capital flows to variable annuities was indeed driven by the lower cost of saving for retirement outside defined contribution plans, we would expect that the effect of the 2017 Tax Cut and Jobs Act was larger for financial products (like variable annuities) that are close substitutes to retirement share classes compared to financial products (like non-retirement share classes and ETFs) that can be used by investors for other purposes than saving for retirement. To test this hypothesis, I repeat estimation of the DiD design with non-retirement share classes and ETFs as treated groups. The control group is still represented by retirement share classes. I report the results for variable annuities, non-retirement share classes, and ETFs in Figure 6. Because different financial institutions may have been exposed differently to the Covid-19 pandemic for reasons unrelated to fiscal policy, I truncate the estimation window in 2019. Consistently with variable annuities being the closer substitute to retirement share classes, the cumulative effect is high for variable annuities, lower for non-retirement share classes, and virtually zero for ETFs. Overall, these results suggest that households actively trade off the tax benefits of different financial products when choosing how to save for retirement.

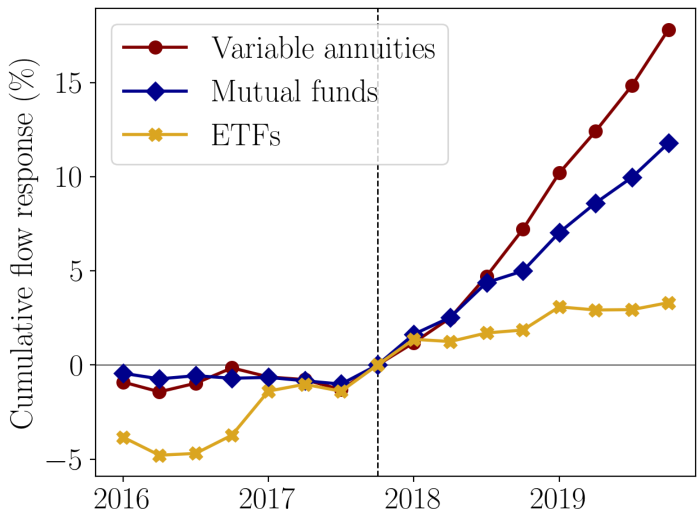
3.4 The effect of changes in dividend tax rates

I now turn to study the effect of changes in dividend and capital gain tax rates, the largest sources of tax expenditure on financial activities for the U.S. federal government, accounting for $265 billion of foregone tax revenue in 2023. I start in this section with taxes on dividends.

Details on the financial institution. Real estate investment trusts (REITs) are ideal candidates to study the effect of changes in dividend taxes. REITs are publicly traded firms that invest in real estate assets and that, differently from C corporations, can avoid the taxation of corporate earnings if they distribute at least 90% of their annual taxable income and if they meet other conditions listed in the internal revenue code. In practice, most REITs have a payout ratio close to 100% (Edgerton, 2010).[20]

[20] In particular, REITs can deduct distributions made to shareholders from the corporate pre-tax income if they distribute to shareholders at least 90% of their annual taxable income, if at least 75% of their assets is invested in real estate and cash, and if at least 75% of their gross income is obtained from real estate and

Figure 6: **Effect across financial institutions**
This figure reports the cumulative effect of the 2017 Tax Cut and Jobs Act on log flows to variable annuities, non-retirement share classes and ETFs. In each of the three specifications, the control group is represented by retirement share classes.



Details on the fiscal reform. Because dividends distributed by REITs are taxed at personal income tax rates, we would expect that a decrease in personal income tax rates increases household demand for REITs. Moreover, because REITs do not typically retain earnings to finance their activities, they should have strong incentive to increase equity issuance in response to an increase in demand. I test this hypothesis using, once again, the 2001 Economic Growth and Tax Relief Reconciliation Act. This reform is preferable to the 2017 Tax Cut and Jobs Act, which also introduced the largest decrease in corporate income tax rates in the history of the U.S.

As discussed in Poterba (2004) and in House and Shapiro (2006), the 2001 reform established a scheduled decrease over time in all personal income tax rates. This scheduled decrease was accelerated by the 2003 Jobs and Growth Tax Relief Reconciliation Act, which gave immediate effect to the tax cuts scheduled to become effective in 2004 and 2006. By the end of 2003, the top marginal income tax rate decreased from 39.6% to 35%.

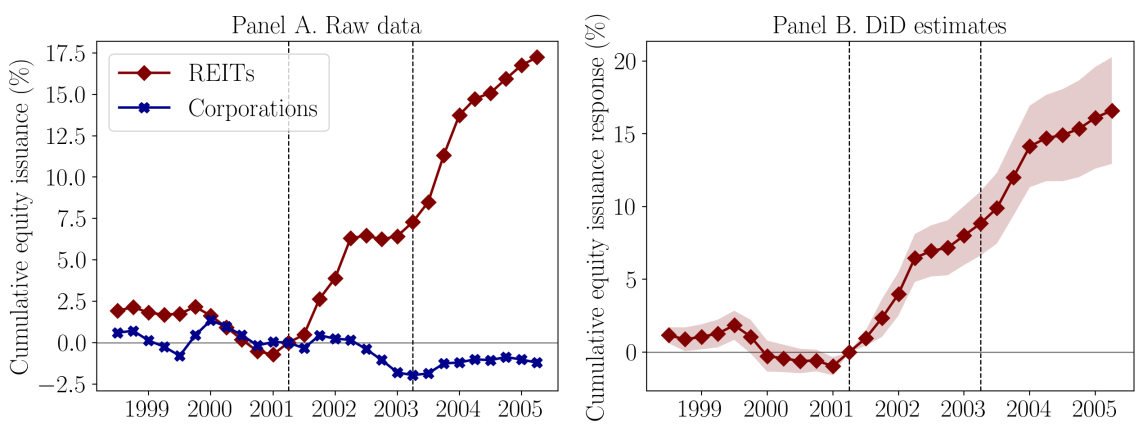
I estimate the DiD design in equation (1) around the 2001 and 2003 reforms, with log net equity issuance observed for security i in year-quarter t as the outcome variable. The treatment group includes REITs and the control group includes publicly traded corporations that operate in any of the SIC codes observed for REITs. I control for lagged market

related sources. REITs are also required to have at least 100 shareholders within the first taxable year and no more than 50% of shares outstanding can be held by five or fewer investors.

capitalization, EBIT to total assets, net income to total assets, lagged assets growth, lagged leverage, and lagged return $r_{i,t-1}$. As for retirement share classes, I extend the estimation window to cover two years after the 2003 reform was passed.

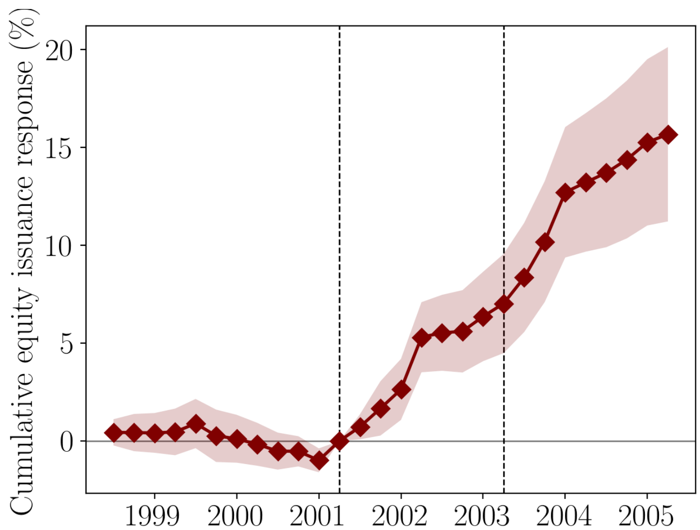
Results. For each year-quarter in 1998Q3-2005Q2, Panel A of Figure 7 reports the cumulative average log equity issuance in the raw data for REITs and corporations included in the control group. Panel B provides estimates of $\sum_{u=-T}^{t} \gamma_u$ from equation (1). Log equity issuance by REITs and corporations moved together until 2001Q2, when the first reform was enacted. After 2001Q2, REITs started to issue equity at a significantly higher rate compared to corporations. Equity issuance by REITs flattened slightly during 2002 and accelerated once more after the passage of the 2003 reform in 2003Q2. Based on the results in Panel B, the 2001 and 2003 reforms increased REIT equity issuance by 16.60 percentage points as of 2005Q2.

Figure 7: **Effect on equity issuance by REITs**
Panel A reports the cumulative average log equity issuance by REITs (treatment group) and corporations (control group) in raw data. Panel B provides the cumulative effect on log equity issuance by REITs, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\gamma}_u$, where γ_t is defined in equation (1). In Panel B, standard errors are clustered at industry times year-quarter level and 95% confidence intervals are provided.



Robustness. I further investigate the role of composition effects in driving the results and I repeat the estimation after restricting the dataset to REITs and corporations that appear throughout the estimation window. Results are provided in Figure 8. The cumulative effect on log equity issuance remains virtually identical as the reforms were not followed by a significant change in the creation of new REITs. In Appendix D.3, I also repeat the estimation after controlling for security fixed effects to absorb unobservables at security level. Moreover, to make sure that the results are not driven by the choice of the equity issuance measure from Boudoukh et al. (2007), I use the annual net equity issuance measure in Jensen et al. (2022), which is based on information from Compustat North America Fundamentals. In both cases results are similar and, if anything, larger.

Figure 8: **Effect on equity issuance by REITs in a balanced panel**
This figure reports the cumulative effect of on log equity issuance by REITs after restricting the dataset to securities available throughout the estimation window (1998Q3 - 2005Q2). Standard errors are clustered at industry times year-quarter level and 95% confidence intervals are provided.



An additional potential concern is that the increase in equity issuance by REITs could have been driven by the real estate bubble that started around the beginning of the century and that culminated with the Great Financial Crisis. In Appendix D.3, I show that this is unlikely to be the case as the results are unchanged if I exclude from the estimation REITs that invest in mortgages, MBS, and residential real estate.

A second reform that changed the tax rate on dividends paid by REITs was the 2017 Tax Cut and Jobs Act. In fact, this reform not only decreased personal income tax rates but it also introduced a 20% deduction on dividends paid by REITs.[21] By the same logic discussed for the 2001 and 2003 reforms, both the 20% deduction and the decrease in personal income tax rates should have increased equity issuance by REITs.

As discussed in Chodorow-Reich et al. (2024), however, the 2017 reform was accompanied by several other provisions and, in particular, by the largest decrease in corporate income tax rates ever passed in the U.S. This change likely increased equity issuance by corporations relative to REITs relative for two reasons: (i) it may have created an incentive

[21]Some distributions paid by REITs are taxed at capital gain rather than income tax rates. While they constitute a minority of total distributions paid out by REITs, these distributions were excluded from the 20% deduction.

for REITs to accept the payment of the corporate tax on non-distributed income and to increase the use of retained earnings to finance their investments; (ii) it may have redirected investors demand away from REITs and towards corporations subject to the corporate tax rate on all of their taxable income. In Appendix D.3, I repeat estimation of the DiD design around the 2017 Tax Cut and Jobs Act and provide evidence that equity issuance by corporations indeed increased after this reform relative to equity issuance by REITs.

3.5 The effect of changes in capital gain tax rates

ETFs represent the ideal candidates to study the effects of changes in capital gain tax rates, the last policy parameter of interest.

Details on the financial institution. ETFs allow investors to pay significantly lower capital gain taxes relative to mutual funds, their closest substitutes. Because they are publicly traded on a secondary market, ETFs are not forced to sell assets and realize capital gains when investors wish to redeem their positions. On the contrary, investors can simply sell ETF shares in the secondary market. This is not the case for mutual funds that are forced to sell assets and realize capital gains when facing net outflows in excess of cash reserves.[22] In addition to being publicly traded, ETFs enjoy a second key tax benefit relative to mutual funds, which relies on the so called "heartbeat trades" carried out by authorized participants and on the tax exemption for in-kind redemptions.[23]

Details on the fiscal reform. Given this superior tax efficiency, we would expect that an increase in capital gain tax rates increases capital flows to the ETF sector. I test this hypothesis by estimating the DiD specification in (1) around the passage of the 2012 American Taxpayer Relief Act, signed into law on January 2, 2013. This reform increased the top marginal tax rate on long-term capital gains from 15% to 20% and on short-term capital gains from 35.0% to 39.6%.[24] In addition, fiscal year 2013 also saw the introduction of a 3.8% surtax on investment income for households above specific income thresholds.[25]

[22] Every investor in the mutual fund is required to pay taxes on realized capital gains proportionally to the share of assets under management held, including investors that are not redeeming mutual fund shares.

[23] This exemption was established in 1969 and confirmed in Section 825(b)(6) of the 1986 Internal Revenue Code. Even in the event of extreme selling pressure that decreases the price of an ETF below the no-arbitrage price implied by the underlying portfolio of securities, one or more authorized participants (typically investment banks and brokerage firms) purchase ETF shares from the secondary market and exchange them "in-kind" with a representative basket of securities held by the ETF. The increase in demand on the secondary market contributes to increase the ETF share price back to the no-arbitrage value while the in-kind redemption does not constitute a tax event for the ETF or its investors.

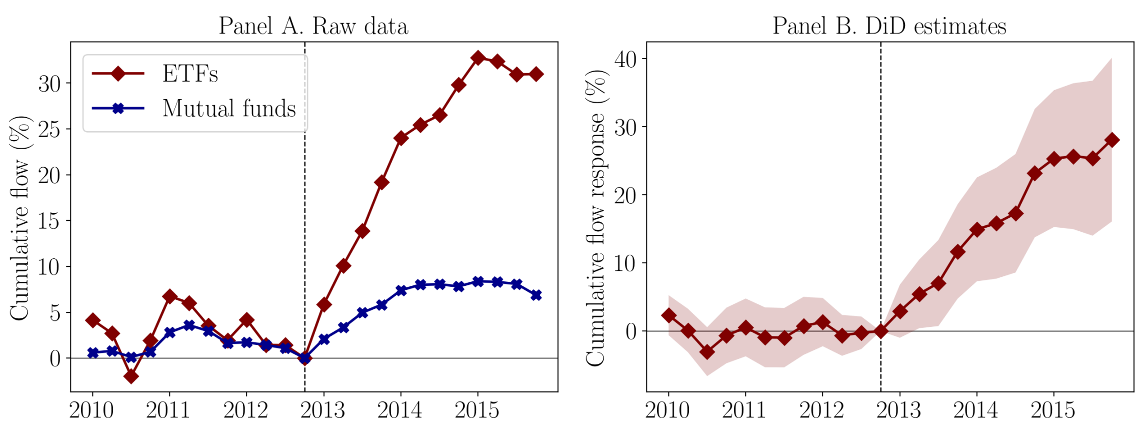
[24] As discussed in Perez Cavazos and Silva (2017), this act made permanent the decrease in tax rates on long- and short-term capital gains established in 2001 with the exception of marginal rates associated to the highest income bracket.

[25] The income thresholds were $200,000 for single filers and $250,000 for joint filers. This additional surtax is

Overall, starting in 2013, the highest tax rate on long-term capital gains increased from 15% to 23.8%, while the highest tax rate on short-term capital gains increased from 35.0% to 43.4%. The treatment group includes ETFs while the control group includes share classes offered by mutual funds.

Results. In Panel A of Figure 9, I report the cumulative average log flow to ETFs and mutual funds in each year-quarter in the estimation window, 2010Q1-2015Q4. Panel B provides instead estimates of $\sum_{u=-T}^{t} \gamma_u$ from equation (1). After 2012Q4, capital flows to ETFs increased substantially compared to flows to mutual funds which, however, also seem to experience a somewhat increasing pattern. Based on the result in Panel B, the increase in capital gain tax rates increased cumulative flows to ETFs by 28.11 percentage points as of 2015Q4.

Figure 9: **Effect on capital flows to ETFs**
Panel A reports the cumulative average log flow to ETFs (treatment group) and mutual funds (control group) in raw data. Panel B provides the cumulative effect on log flows to ETFs, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\gamma}_u$, where γ_t is defined in equation (1). In Panel B, standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.

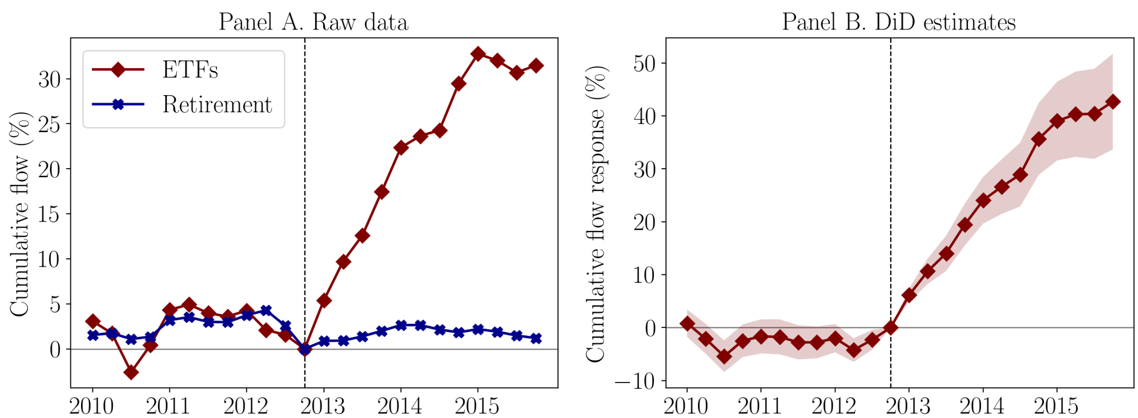


To evaluate whether the increase in capital flows to mutual funds documented in Figure 9 contributed to underestimate the effect of the reform on capital flows to ETFs, I repeat the estimation after I modify the control group to include only retirement share classes. The advantage is that retirement share classes are not directly treated by the increase in capital gain tax rates since realized capital gains within defined contribution plans are not taxed. While it could be that the reform created an incentive to hold a larger fraction of

commonly referred to as Net Investment Income Tax (NIIT) and was initially included as part of the Affordable Care Act, formally signed into law on March 23, 2010. It applies to any form of investment income, including long and short-term capital gains, dividends and interest income. The introduction of the NIIT was, however, delayed repeatedly and ultimately became effective starting from 2013.

assets in defined contribution plans to escape the higher tax rates on capital gains, flows to retirement share classes cannot increase above the annual contribution limits that the 2012 reform did not modify. I report the results in Figure 10. Based on Panel A, the control group now does not display a visible change after the reform. Based on Panel B, the effect of the reform on cumulative flows increases to 42.68 percentage points as of 2015Q4.

Figure 10: **Effect on capital flows to ETFs using retirement share classes as control**
Panel A reports the cumulative average log flow to ETFs (treatment group) and retirement share classes (control group) in raw data. Panel B provides the DiD estimates when retirement share classes are used as control for ETFs. In Panel B, standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



Given the documented effects, I study whether ETFs responded to the increase in demand by increasing their fees. In Panel A of Figure 11, I report the average expense ratio observed for ETFs and mutual fund share classes. The shaded area highlights the time window between 2012Q2, when the reform was first introduced in Congress, and 2012Q4, when the reform was approved.[26] To facilitate the visualization of the parallel trend assumption, I report time trends separately estimated for ETFs and mutual funds over the pre-reform period (2010Q1-2012Q2). Panel A of Figure 11 reveals that the the two time series were following parallel trends until 2012Q2, when the average expense ratio charged by ETFs increased and the expense ratio charged by mutual funds slightly decreased compared to the projected time trend.

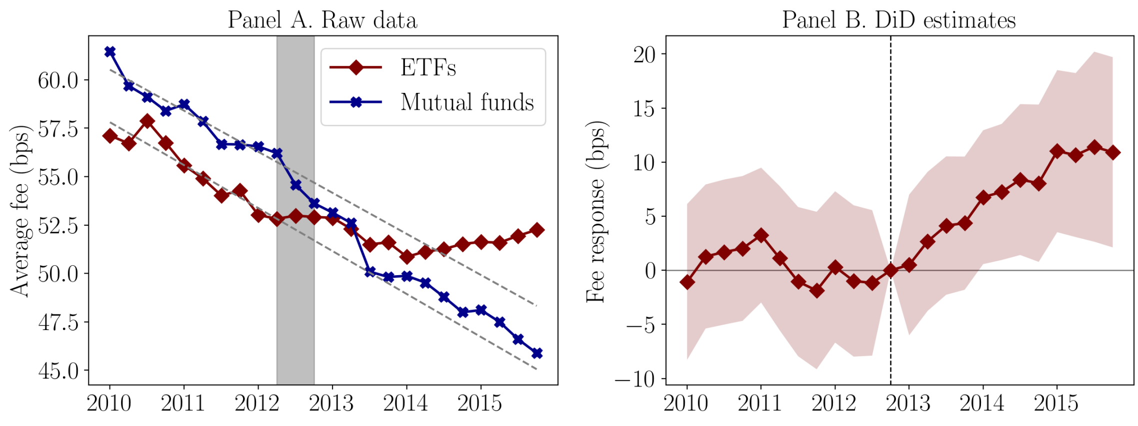
I formally quantify the magnitude of this effect by estimating the DiD design in equation (1) with the expense ratio charged by each share class i in year-quarter t as the outcome variable. Panel B provides estimates of γ_t. Overall, the reform increased the cost for house-

[26]Specifically, the American Taxpayer Relief Act was first introduced in the House of Representatives on July 24, 2012 as the Job Protection and Recession Prevention Act (`https://www.congress.gov/bill/112th-congress/house-bill/8`).

holds to access ETFs relative to mutual funds by 11 bps.[27] In Appendix D.4, I further show that this effect was mainly driven by ETFs that entered the market after the reform was passed, which increased their expense ratio by 19 bps. Moreover, I show that the effect was higher in styles that experienced larger capital inflows, confirming that the increase in fees charged by ETFs after 2012Q4 was indeed a response to the increase in demand. The share of tax savings captured by the financial industry is economically significant. While ETFs allowed households to save on average 89 bps in taxes per year after the reform was passed (Moussawi et al., 2022), households had to pay 11 bps higher fees to access the average ETF and up to 19 bps higher fees to invest in new ETFs. Hence, the average ETF retained 12% of household tax savings and new ETFs up to 21%.

Overall, this result suggests that management companies responded to the increase in demand induced by the reform and increased the average cost of investing in ETFs. More generally, this result suggests that the response from the financial sector is quantitatively important and should be taken into account when evaluating the welfare implications of fiscal policy actions that directly or indirectly promote specific financial products. In fact, part of the surplus created by these actions is likely to be retained by financial institutions.

Figure 11: **Effect on the expense ratio charged by ETFs**
Panel A reports the average expense ratio charged by ETFs (treatment group) and mutual funds (control group) in raw data. Panel B provides the effect on the expense ratio charged by ETFs, summarized for each year-quarter t by the estimate $\widehat{\gamma}_t$. In Panel B, standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



Robustness. In Appendix D.4, I provide results from a series of robustness tests. I show that the response of capital flows is identical to the baseline estimate in Figure 9 when

[27] In Appendix D.4, I show that the magnitude of the effect remains virtually unchanged and is equal to 12 bps when evaluated relative to 2012Q2.

I restrict the dataset to share classes alive throughout the estimation window, 2010Q1-2015Q4. This result confirms that the reform was not followed by a substantial increase in the entry of new ETFs, allowing ETFs already on the market to increase on average the fees charged to households.

Second, if the increase in capital flows to ETFs was indeed driven by the increase in capital gain taxes, we would expect the effect to be higher for styles in which asset returns mainly come in the form of capital gains rather than dividend distributions. In Appendix D.4, I show that this is indeed the case. The effect on both capital flows and fees was stronger for small-cap and mid-cap ETFs and lower for large-cap ETFs. This ranking mirrors the ranking of these three styles in terms of dividend yield, which is on average lower for small-cap and mid-cap stocks and higher for large-cap stocks. Similarly, the effect was stronger for growth ETFs compared to income ETFs.

3.6 Taking stock

Table 1 summarizes the DiD estimates discussed so far. To ensure that the effects of different reforms are comparable, I convert them into elasticities and I rescale them by the percentage change in the relevant policy parameter changed by each reform.[28] In Appendix E, I further report elasticities with respect to the average keep rate and semi-elasticities, computed by rescaling the effect by the change in the average tax rate.

I measure the contribution limit on defined contribution plans as the sum of the total limit on 401(k) plans and the limit on IRAs, both of which were affected by the 2001 and 2003 reforms studied in Section 3.2. For income, dividend, and capital gain tax rates, I construct an average marginal tax rate across U.S. households based on statistics computed by the Internal Revenue Service (IRS) from the universe of tax filings processed every year.[29]

The third column provides the elasticities of capital flows with respect to different policy parameters. I find that the elasticity with the respect to the contribution limit is 0.83, implying that a 1% increase in the contribution limit increases assets in defined contribution plans by 0.83%. The magnitude can be better interpreted by noticing that this elasticity is in general bounded above by 1, as the percentage change in assets held in defined contribution plans cannot in general exceed the percentage change in the contribution limit

[28] To isolate the response of household demand, the elasticities of capital flows are computed based on the DiD estimates obtained from a balanced panel of institutions. This avoids that the elasticities also capture the response from the financial sector, especially on the extensive margin.

[29] I provide details on the construction of both the contribution limits and the average marginal tax rates in Appendix A.4.

Table 1: **Elasticities of capital flows, fees, and market entry**
This table summarizes the DiD estimates obtained for different policy parameters. To ensure that the estimates are comparable, I convert them into elasticities and I rescale them by the percentage change in the relevant policy parameter changed by each reform. The horizon used to compute elasticities ranges from two to four years and it coincides with the post-reform period used to estimate the DiD designs around each reform.

		Elasticity of:		
Policy parameter	**Institutional sector**	**Flows**	**Fees**	**Entry**
Contribution limit	Defined contribution plans	0.83	≈0	1.08
Income tax rate	Life insurance	1.60	-	-
Dividend tax rate	REITs	1.59	-	-
Capital gain tax rate	ETFs	1.56	0.60	≈0

itself.[30] I find the estimated elasticity to be close to the upper bound of 1 and not statistically different from it. While a previous literature has emphasized how participants in defined contribution plans are inert (Benartzi and Thaler 2001; Madrian and Shea 2001; Agnew et al. 2003), this result shows that, in the aggregate, U.S. households actively take advantage of the tax benefits on these plans. The difference between average and aggregate behavior suggests that wealthier U.S. households are significantly more responsive to these tax benefits, consistent with the evidence by Chetty et al. (2014) on retirement accounts in Denmark.[31]

Turning to the elasticity of capital flows with respect to income, dividend, and capital gain tax rates, I find them to be all higher than 1 in absolute value, implying that the percentage change in household demand more than offset the percentage change in the tax rate. While these elasticities have not been estimated in previous work, it is useful to compare them with other elasticities to tax rates estimated in the public finance literature. I find them to be three times higher than the elasticity of realized capital gains to changes in the capital gain tax rate (Agersnap and Zidar, 2021) and up to ten times higher

[30] This is the case unless the reforms triggered a response from households for which the contribution limit was not binding.

[31] A back-of-the-envelope calculation using evidence in Chetty et al. (2014) can also help to rationalize my estimated elasticity of 0.83 relative to the earlier evidence of inert U.S. households. Chetty et al. (2014) (Section 5, Figure 5) estimate that Danish households around the 80th percentile of the income distribution decreased capital pension contributions by 2,449 Danish kroners (DKr) when the Danish government decreased the pension subsidy from 59 cents to 45 cents per DKr contributed. This was a 48% decrease in pension contributions relative to a pre-reform average of DKr 5,113. Relative to the percentage change in the tax subsidy (-23.73%), the implied elasticity is 2.02. Consistent with wealthier households being more responsive to tax subsidies, my estimated elasticity of 0.83 is lower than 2.02 as it reflects the aggregate the response of all households, wealthier and less wealthy.

than the elasticity of taxable wealth to changes in the Danish wealth tax (Jakobsen et al., 2020). These differences have important implications for the design of fiscal policy: while households may be reluctant to reduce trading in financial markets or to reduce wealth accumulation in response to an increase in taxes, they actively re-optimize their portfolio allocation across financial products to save taxes going forward.[32]

The fourth and fifth columns focus on the response from the financial sector and report the elasticities of fees and market entry. Because I do not observe fees charged by life insurance companies and REITs, I focus on retirement share classes and ETFs. These estimates reveal a stark contrast. When the increase in capital flows is not followed by an increase in market entry (like for ETFs), financial institutions respond by increasing fees. This result suggests that not all the surplus generated by fiscal reforms accrues to households, as part of it is retained by the financial sector. On the contrary, when fiscal reforms are followed by an increase in market entry (like for retirement share classes), the effect on fees is insignificant, consistent with an increase in competition.

The magnitudes of these effects are economically significant. A 10% increase in the average capital gain tax rate increases fees charged by ETFs by 6 bps, an 11.54% increase compared to the average fee of 52 bps observed in 2012Q4, when the 2012 reform was passed. A 10% increase in the contribution limit increases the probability of entry of new retirement share classes by 10.80 percentage points.

3.7 A conceptual framework to interpret the DiD estimates

One potential concern is that the DiD estimates and therefore the elasticities discussed in Section 3.6 are overestimated if households reallocate capital from the control to the treatment group in response to fiscal reforms. In this case, the Single Unit Treatment Value Assumption (SUTVA), which is required for consistent DiD estimates, does not hold because the control group is treated as well.

Let χ denote the effect of a fiscal reform on the treatment group (i..e, the effect of interest) and $\widehat{\chi}$ the DiD estimate. If households reallocate from the control to the treatment group in response to a fiscal reform, then $\widehat{\chi} > \chi$ and the DiD estimate is biased upward. In Appendix F, I show that standard portfolio choice models imply the following relation

[32] In Appendix J, I provide intuition on the origin of this difference between my elasticities and the elasticity of taxable wealth to the wealth tax in Jakobsen et al. (2020). I extend the model in Jakobsen et al. (2020) and I allow households to trade multiple risky assets. I show that the model can jointly match both my elasticities and the elasticity in Jakobsen et al. (2020).

between χ and $\widehat{\chi}$ under assumptions validated in the data

$$\chi = \widehat{\chi}\,(1-\omega)\,, \tag{2}$$

where $\omega \in (0,1)$ is the fraction of total assets in the treatment group relative to the sum of total assets in the treatment and control group. Equation 2 shows that if ω is sufficiently small, then the DiD estimate $\widehat{\chi}$ is sufficiently close to the effect of interest χ. The intuition is that ω is small if total assets managed by the control group are large compared to total assets managed by the treatment group. In this case, every dollar outflow from the control group is negligible, once rescaled for total assets, compared to flows into the treatment group. In Appendix F, I show that this is indeed the case and that, across all DiD experiments discussed earlier in this section, $\frac{\chi}{\widehat{\chi}}$ ranges from 0.82 to 0.99.

I also emphasize that $1-\omega$ represents a conservative lower bound of how much $\widehat{\chi}$ should be corrected to back out χ. The reason is that equation (2) is derived under the assumption that households can only substitute between the treatment and the control group. In practice, capital flows into the treatment group can be financed with labor income or by selling financial assets other than the control group. In this case, the DiD estimates are consistent.

4 Aggregation

The results in Section 3 show that capital flows are highly responsive to tax incentives. I now provide additional perspective on the magnitude of these effects and I ask how much of the growth of aggregate institutional sectors is explained by fiscal policy, both in the short term and in the long term. Answering this question is important to understand whether fiscal policy can or cannot affect the structure of the financial system and, therefore, financial stability. I use two empirical strategies to address this question.

For each reform studied in Section 3, let $u = 0$ denote the first year-quarter in which the reform became effective and $u = T$ the last year-quarter in the estimation window. The first empirical strategy uses the DiD estimates presented in Section 3 and constructs the effect of each reform on the growth rate of aggregate assets managed by the treated institutional sector as $\sum_{u=0}^{T}\widehat{\gamma}_u$. The second strategy recognizes that $\sum_{u=0}^{T}\widehat{\gamma}_u$ correctly identifies the effect of the reform on the growth rate of aggregate assets under the assumption of homogeneous treatment across institutions.[33] To relax this assumption and allow for

[33]Indeed, only under this assumption do the effects on capital flows estimated in Section 3 represent not only the effect of a reform on the average growth rate of assets but also on the growth rate of total assets.

heterogeneous treatment, I extend the baseline DiD design in equation (1) and estimate

$$\log\left(1+f_{it}\right)=\alpha_{s(i)t}+\gamma_{it}\mathbb{I}\left(i\in Treatment\right)+\beta' X_{it}+\varepsilon_{it}, \tag{3}$$

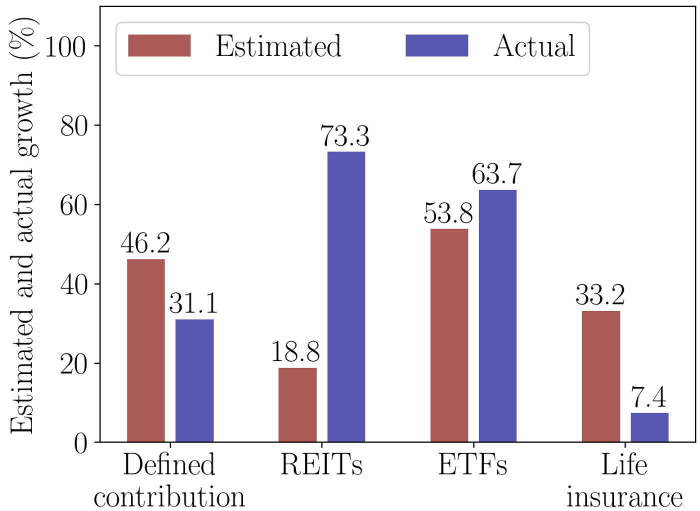
where $\gamma_{it}=\gamma_t' Z_i$. The only difference compared to equation (1) is that γ_{it} is now allowed to depend on a vector of institutions' observable characteristics Z_i. I include in Z_i all characteristics also used as controls in X_{it}: the log of average assets under management observed for each institution i in the pre-reform period, average return, average turnover, and the number of years since the institution was first introduced on the market. I also include a vector of style indicators to allow for heterogeneous effects across institutions that invest in assets with different characteristics. Given estimates of equation (3), the effect of each reform on the growth rate of assets managed by institution i is $\sum_{u=0}^{T}\widehat{\gamma}_{iu}$ and the effect of the reform on the growth rate of assets managed by an aggregate institutional sector is $\widehat{g}=\log\left(\frac{\sum_i A_{i0}\exp\left(\sum_{u=0}^{T}\widehat{\gamma}_{iu}\right)}{\sum_i A_{i0}}\right)$.

For each institutional sector, Figure 12 compares $\widehat{g}$ estimated under the assumption of heterogeneous treatment with the actual growth rate in aggregate assets, $\log\left(\frac{\sum_i A_{iT}}{\sum_i A_{i0}}\right)$, observed around the reform. In Appendix G.2, I show that results are identical under the assumption of homogeneous treatment. The effects of fiscal reforms account for 25.65% and 84.46% of the actual growth rate of REITs and ETFs, respectively. For defined contribution plans and life insurance, the estimated effect is higher than the actual growth due to either negative returns or outflows unrelated to fiscal policy.

While Figure 12 shows that fiscal policy explains a substantial fraction of the aggregate growth rates observed around fiscal reforms, the more interesting question is whether fiscal policy can affect the structure of the U.S. financial system in the long term. To address this question, for each institutional sector, I construct a measure of long-term growth defined as the cumulative value-weighted flow $g=\sum_t\sum_i\frac{A_{it}}{\sum_i A_{it}}\log\left(1+f_{it}\right)$ observed between 2000Q1 and 2020Q4. For each estimate $\widehat{g}$ presented in Figure 12, Panel A of Figure 13 reports $\frac{\widehat{g}}{g}$. Even in the long term, fiscal policy is a key driver behind the structure of the U.S. financial system: one fiscal reform already explains around 20% of the long-term growth of aggregate institutional sectors and up to 30.8% for variable annuities offered by life insurance companies.

While meaningful, this result is potentially biased downward. The reason is that, between 2000Q1 and 2020Q4, contribution limits and tax rates experienced changes other than those introduced by the fiscal reforms studied in Section 3. For example, contribution limits kept increasing after 2005 because the 2001 reform indexed them to annual inflation rates. To capture the effect of these additional changes in the estimate $\widehat{g}$, I multiply the

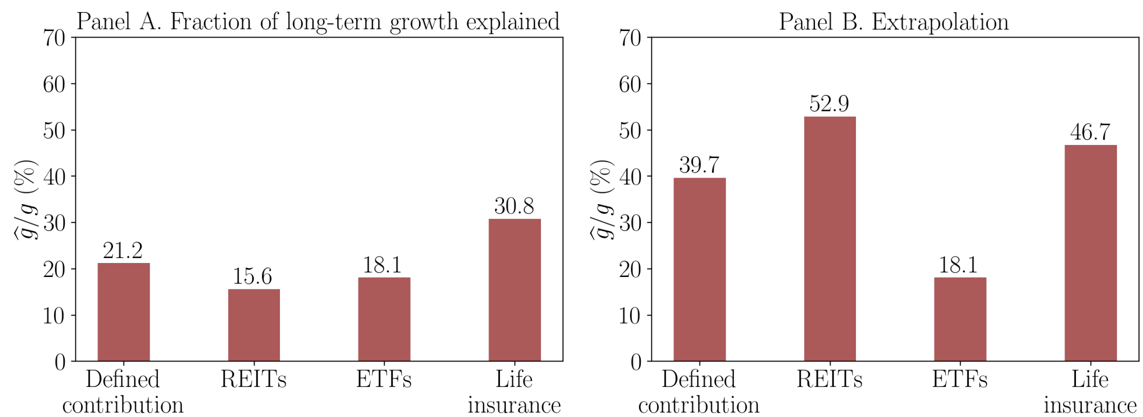
Figure 12: **Estimated and actual growth of institutional sectors around fiscal reforms**
This figure reports the estimated effect of fiscal reforms on the growth rate of aggregate assets managed by different institutional sectors. For each institutional sector, the actual growth rate of aggregate assets is also reported.



elasticities presented in Section 3.6 by changes in the relevant policy parameter that happened between 2000Q1 and 2020Q4 and that I could not test directly.[34] Panel B of Figure 13 reports the result of this extrapolation exercise. Specifically, it provides estimates of $\frac{\widehat{g}}{g}$, with $\widehat{g}$ now inclusive of all changes in the relevant policy parameter. After accounting for all changes in policy parameters, fiscal policy explains up to 53% of institutions' long-term growth. This result shows that fiscal reforms have not only a large but also a persistent effect on capital flows, making fiscal policy a key driver behind the long-term structure of the U.S. financial system.

[34] I provide additional details on this extrapolation exercise in Appendix G.1. However, to set ideas, consider for instance the case of REITs. While the 2017 Tax Cut and Jobs Act decreased personal income tax rates and introduced a 20% deduction on dividends distributed by REITs, it also decreased corporate income tax rates. This contemporaneous effect made it impossible to use this reform to study the effect of changes in income taxes on equity issuance by REITs. Let ζ denote the elasticity of equity issuance by REITs with respect to the income tax rate estimated using the 2001 Economic Growth and Tax Relief Reconciliation Act. Let also $\Delta \log \tau$ denote the decrease in the average income tax rates introduced by the 2017 Tax Cut and Jobs Act. The extrapolated effect of the 2017 Tax Cut and Jobs Act on equity issuance by REITs is simply computed as $\zeta \Delta \log \tau$. This extrapolated effect is then added to $\widehat{g}$.

Figure 13: **Fraction of long-term growth explained by fiscal policy**
Panel A reports the fraction of long-term growth of aggregate institutional sectors explained by fiscal policy, where the long-term growth of each institutional sector is computed as the cumulative value-weighted flow observed between 2000Q1 and 2020Q4. Panel B uses the elasticities discussed in Section 3.6 to extrapolate the effect of changes in the relevant policy parameter that occurred outside the reforms studied in Section 3.



5 The impact of fiscal reforms on asset prices

Because financial institutions hold different portfolios of assets, fiscal policy may have a large and persistent effect not only on capital allocation but also on the cross-section of asset prices. While a large literature has studied the relation between asset returns and taxes, most of it has focused either on the capitalization of taxes into asset valuations or on the role of tax clienteles.[35] However, there is surprisingly limited evidence on how fiscal reforms affect the cross-section of asset returns.[36]

Using the increase in capital flows to ETFs around the 2012 American Taxpayer Relief Act, I show that stocks held relatively more by ETFs than mutual funds persistently outperformed after the reform was passed. I focus on the 2012 reform as it offers an ideal empirical setting: it allows me to restrict attention to index ETFs and mutual funds that track predetermined benchmarks and do not respond to contemporaneous changes in asset returns. In particular, the methodology combines insights from the benchmark inclusion (Shleifer, 1986) and Bartik instrument (Goldsmith-Pinkham et al., 2020) literatures. From the benchmark inclusion literature, it uses the fact that index funds track pre-determined

[35] See for instance McGrattan and Prescott (2005) and Sialm (2009) for the former and Desai and Jin (2011), Sialm and Starks (2012), and Babina et al. (2021) for the latter.

[36] One notable exception is represented by an early literature that has looked at the change in the valuation of dividends around tax reforms in the U.S. (Michaely, 1991), in the U.K. (Poterba and Summers, 1984b; Bell and Jenkinson, 2002) and in Canada (Lakonishok and Vermaelen, 1983; Booth and Johnston, 1984).

benchmarks. From the Bartik instrument literature, it uses the fact that the exposure to an aggregate (fiscal) shock depends on local (portfolio) shares.

Let A_{iat} denote the dollar holdings of stock a in the portfolio of index ETF or mutual fund i in year-quarter t.[37] Let also E denote the set of ETFs and M the set of mutual funds in the dataset. The weight on stock a in the portfolio of an aggregate ETF is $\theta_{at}^{(E)} = \frac{\sum_{i \in E} A_{iat}}{\sum_{i \in E} \sum_a A_{iat}}$ and the weight on stock a in the portfolio of an aggregate mutual fund is $\theta_{at}^{(M)} = \frac{\sum_{i \in M} A_{iat}}{\sum_{i \in M} \sum_a A_{iat}}$. Define the following measure of "mismatch" between ETF and mutual fund portfolio weights

$$m_{at} = \left(\theta_{at}^{(E)} - \theta_{at}^{(M)}\right) \frac{\sum_{i \in M} \sum_a A_{iat}}{ME_{at}} \tag{4}$$

where ME_{at} denotes the market capitalization of stock a. The measure m_{at} has a simple economic intuition. Consider a representative household that holds (directly and indirectly via mutual funds and ETFs) the entire equity market and that in response to the 2012 reform sells its position in the aggregate mutual fund and reallocates that capital to the aggregate ETF. As a result of this trade, stock a would experience a net capital inflow equal to $\left(\theta_{at}^{(E)} - \theta_{at}^{(M)}\right) \sum_{i \in M} \sum_a A_{iat}$ in dollar terms and equal to m_{at} relative to its market capitalization. It follows that m_{at} would capture the percentage change in the demand for stock a if all assets in the aggregate mutual fund were to be reallocated to the aggregate ETF.

This argument is made slightly more complicated by the evidence in Figure 14, where I report the fraction of total assets managed by ETFs and mutual funds in different styles. ETFs and mutual funds are not randomly assigned to different styles. Instead, compared to mutual funds, ETFs have a lower fraction of their assets invested in Large Cap and Growth & Income stocks. They also have a larger fraction of assets allocated to Growth stocks. At the same time, stocks in different styles may respond differently to changes in capital gain tax rates. For instance, growth stocks distribute less dividends than large-cap stocks and they are therefore more exposed to an increase in capital gain taxes.

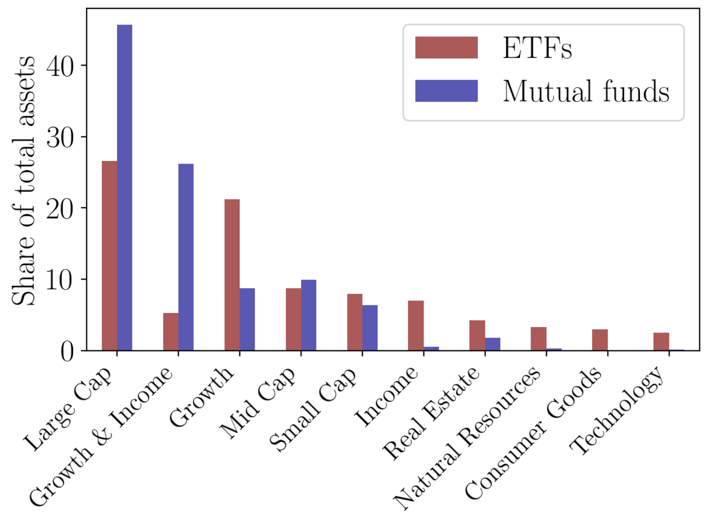
To control for this source of selection, I modify equation (4) and construct m_{at} as follows

$$m_{at} = \sum_s \left(\theta_{as(i)t}^{(E)} - \theta_{as(i)t}^{(M)}\right) \frac{\sum_{i \in M} \sum_a A_{ias(i)t}}{ME_{at}} \tag{5}$$

where $s(i)$ denotes the style observed for mutual fund or ETF i, $\theta_{as(i)t}^{(E)} = \frac{\sum_{i \in E} A_{ias(i)t}}{\sum_{i \in E} \sum_a A_{ias(i)t}}$,and

[37] Quarterly data on mutual fund and ETF dollar holdings are from the CRSP Mutual Fund Holdings dataset. I identify passive ETFs and mutual funds as those with index_fund_flag in CRSP equal to "D". These are funds that CRSP defines as "Pure Index Funds" with the "objective to match the total investment performance of a publicly recognized securities market index. The fund will hold virtually all securities in the noted index with weightings equal to those in the index".

Figure 14: **Share of assets in different styles**
This figure reports the fraction of total assets managed by ETFs and mutual funds in different styles. Numbers are reported for 2012Q4, the last year-quarter before the 2012 reform was passed.



$\theta^{(M)}_{as(i)t} = \frac{\sum_{i \in M} A_{ias(i)t}}{\sum_{i \in M} \sum_a A_{ias(i)t}}$. Based on equation (5), m_{at} captures the increase in demand that stock a would experience if all assets held by the aggregate mutual fund in each style s were suddenly reallocated to the aggregate ETF in the same style.

I test whether stocks with higher m_{at} outperform stocks with low m_{at} using a DiD design with continuous treatment

$$r_{at} = \alpha_{c(a)t} + \delta_t m_{a2012Q4} + \psi' \left(X_{a,t-1} \right) R_t + \varepsilon_{at}, \tag{6}$$

where r_{at} denotes the excess return of security a in year-month t, $m_{a2012Q4}$ denotes the mismatch measure (continuous treatment) observed for security a in the year-quarter just before the reform was passed (2012Q4), $\delta_t m_{a2012Q4}$ interacts year-quarter fixed effects with the continuous treatment, $\alpha_{c(a)t}$ denotes industry (3-digit SIC code) times year-quarter fixed effects, R_t is the vector of five Fama-French factors plus momentum, and $\psi \left(X_{a,t-1} \right)$ is the vector of factor loadings expressed as a function of stock a lagged characteristics. I assume $\psi \left(X_{a,t-1} \right) \equiv \Psi X_{a,t-1}$ to be a linear function and I include in $X_{a,t-1}$ all sorting measures underlying the construction of the five Fama-French factors and momentum: market capitalization, book equity, return, EBIT to total assets, net income to total assets, assets growth, and leverage.[38]

[38] To avoid the possibility that the effects are driven by extremely small firms, I exclude micro stocks from the analysis. I follow Jensen et al. (2022) and identify micro stocks as those with market capitalization lower

For each year-quarter in 2010Q1-2015Q4, Panel A of Figure 15 provides estimates of $\sum_{u=-T}^{t} \delta_u$ from equation (6). To facilitate interpretation of the magnitudes, I multiply these estimates by $m_{a2012Q4} = 1\%$ so that they can be interpreted as the effect on returns of a 1% increase in demand relative to a stock's market capitalization. For completeness, I report in Panel B the distribution of $m_{a2012Q4}$ across stocks.

Before the 2012 reform was passed, there is no relation between returns and $m_{a2012Q4}$. However, after the reform, securities with higher $m_{a2012Q4}$ reported significantly higher returns. Estimates are both statistically and economically significant. When demand increases by 1%, then returns increase by 4.24 percentage points annually and 1.06 percentage points quarterly in the three years after the reform was passed. While the implied multiplier of 1.06 is within the range of estimates proposed by previous work (Lou, 2012; Chang et al., 2015; Schmickler, 2020; Pavlova and Sikorskaya, 2023), the time horizon that I consider is around ten times longer. This result shows that fiscal reforms have a large and persistent effect not only on capital allocation but also on the cross-section of asset prices. Moreover, notice that the shape of the response is hard to explain by existing macro-finance models which would generally imply a discontinuous jump of prices on impact of the reform. On the contrary, I find that prices keep increasing at relatively constant rates for several years before flattening out. This result suggests that active market participants did not anticipate the effect of the reform on capital flows and asset prices, as they would have otherwise traded in advance.

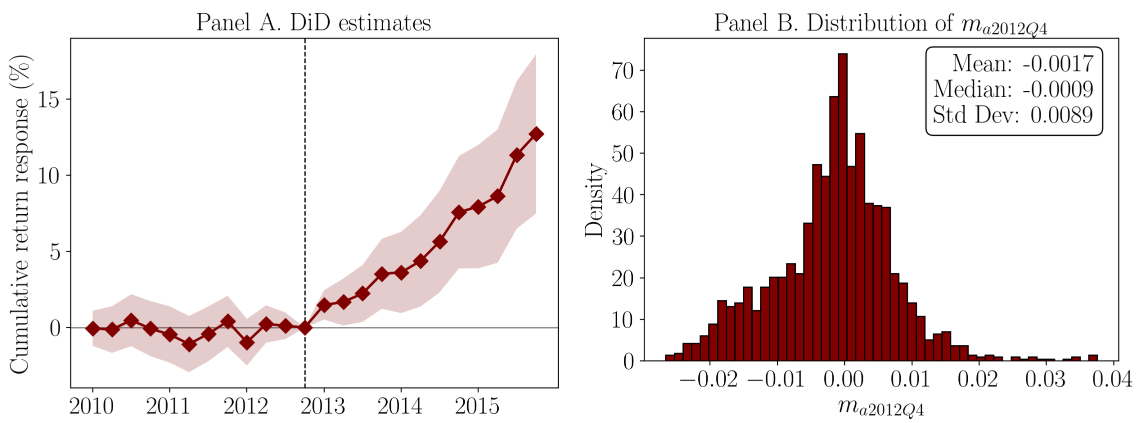
In Appendix H, I report results from a series of robustness tests. I verify that results are unchanged if I use the mismatch measures observed in 2012Q2, 2011Q4, and 2011Q2. This test rules out that ETFs tilted ex-ante their holdings towards securities more likely to benefit from the 2012 reform, which would introduce positive bias in the estimates. I also report estimates from a DiD design with discrete treatment, where the treatment (control) group includes all stocks a with $m_{a2012Q4}$ above (below) the cross-sectional median.[39] Results remain statistically significant with treated stocks outperforming stocks in the control group by 6.97 percentage points annually in the three years after the reform.

One additional concern could be that equation (6) does not control for stock fixed effects. To control for unobservables at the security level, I estimate the change in alpha around the reform separately for each individual security. I find that the change in annual alpha increases monotonically in m_{2012Q4}, going from -6.67% on average for stocks in the bottom quintile to 5.26% for stocks in the top quintile. Overall, these robustness tests confirm the large and persistent effect of fiscal reforms on the cross-section of asset returns.

than the 20th percentile among stocks traded on the NYSE.

[39] I include estimates from a discrete treatment specification because DiD designs with continuous treatment are based on the stronger identification assumption of parallel trends for any pair of treatment values.

Figure 15: **Effect on asset prices**
Panel A reports the cumulative effect of the 2012 reform on returns, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\delta}_u$, where δ_t is defined in equation (6). To facilitate interpretation of the magnitudes, Panel B provides the distribution of $m_{a2012Q4}$ across securities together with mean, median, and standard deviation. In Panel A, standard errors are clustered at industry times year-month level and 95% confidence intervals are provided.



Finally, notice that the quarterly multiplier of 1.06 discussed earlier is likely a conservative lower bound of the true multiplier. The reason is that, in response to the fiscal reform, households did not sell their entire position in mutual funds and the increase in demand on each stock a was likely lower than $m_{a2012Q4}$. In Appendix H, I show that this intuition is correct and, by estimating the actual shift in demand on impact of the fiscal reform, I find a multiplier just above 2.

6 Who responds to tax incentives?

Given the large and persistent effects of fiscal policy on capital flows, a natural question arises: which households are more responsive to tax incentives and benefit from the current tax system? While crucial for understanding the distributional implications of fiscal policy, answers to this question have been generally limited by data availability.[40] In this section, I address this issue by using detailed household-level data from Addepar. Given the unique feature of the dataset, which includes realized and unrealized returns at high

[40] Among others, previous papers that have studied the individual-level response to tax incentives include Odean (1998), Barber and Odean (2000), Gruber and Saez (2002), Poterba and Samwick (2003), Bergstresser and Poterba (2004), Ivković et al. (2005), Amromin et al. (2007), Piketty et al. (2018), and Chodorow-Reich et al. (2024) in the U.S., Chetty et al. (2014), Andersen et al. (2020), and Jakobsen et al. (2020) in Denmark, Zoutman (2018) in the Netherlands, Fagereng et al. (2019) in Norway, and Duran-Cabré et al. (2019) in Spain.

frequency, I focus on the response of households to capital gain taxes, the largest source of tax expenditure for the U.S. federal government.

6.1 Coverage of the wealth distribution and advisor types

The Addepar platform has grown rapidly over time and has reached a broad coverage of household financial activities. The number of unique portfolios in the dataset has increased from 12,479 in January 2016 to 228,446 in March 2023 and total wealth has increased from $157 billion to $2.3 trillion over the same period.

Let A_{it} denote total assets held by household i in year-month t. Panel A of Figure 16 summarizes the coverage of the wealth distribution in March 2023, the last year-month covered by the sample. I assign each household i to one of five quintiles based on A_{it}. For each quintile, I report the average of A_{it} across households. I include households between the 90th and 99th percentiles as well as households in the top 1% as separate groups. The average wealth is $210,000 for households in the bottom 20% of the wealth distribution. It increases to $2.54 million for households in the fourth quintile and reaches $207.47 million for households in the top 1%. These statistics confirm the broad coverage of the wealth distribution offered by the dataset.

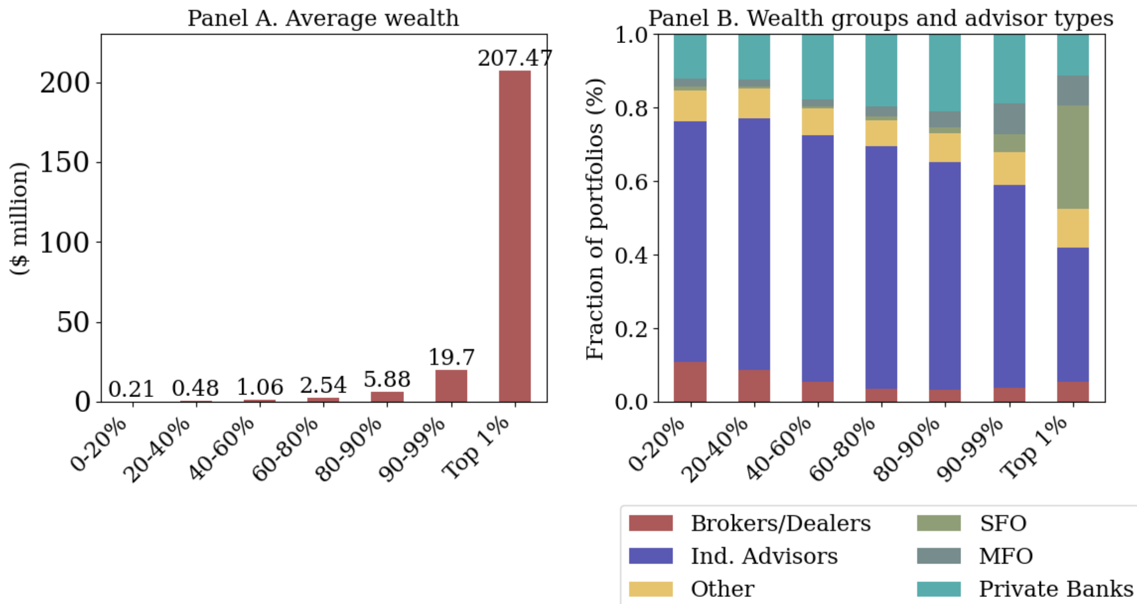
In Panel B, I report the fraction of households in each wealth group that are advised by different types of financial advisors. Consistent with the correlation between wealth and advisor types documented in Gabaix et al. (2024), households at the bottom of the wealth distribution are mainly advised by independent advisors. The importance of independent advisors however decreases with wealth and more than 40% of households at the top of the wealth distribution are advised by private banks, single-family offices (SFO), and multi-family offices (MFO). The greater access to private banks, single-family offices, and multi-family offices by households at the top of the wealth distribution will be important in the subsequent analysis.

6.2 Unrealized and realized gains across U.S. households

Because the key objective is to study how individual U.S. households trade in response to tax incentives, I restrict attention to liquid asset classes as defined in Gabaix et al. (2024). These asset classes mainly include equity and fixed-income positions that can be easily traded in public markets.[41]

[41] More specifically, liquid assets classes in Gabaix et al. (2024) include Municipal Bonds, U.S. Government/Agency Bonds, Corporate Bonds, Bond Funds, ABS/MBS, Structured Debt, International Govern-

Figure 16: **Average wealth and advisor types across the wealth distribution**
Panel A reports the average wealth A_{it} across households in different wealth quintiles. Households with wealth between the 90th and 99th percentiles as well as households in the top 1% are included as separate groups. Panel B reports the fraction of households in different wealth groups advised by different types of advisors. The results are presented for March 2023.



To motivate the analysis in the following sections, I first provide stylized facts regarding the importance of unrealized gains and losses in the portfolios of U.S. households. Let UG_{it} and UL_{it} denote dollar unrealized gains and losses in the portfolio of household i at the end of year-month t. I also denote unrealized gains and losses expressed as a fraction of total wealth as $ug_{it} = \frac{UG_{it}}{A_{it}}$ and $ul_{it} = \frac{UL_{it}}{A_{it}}$, respectively.[42]

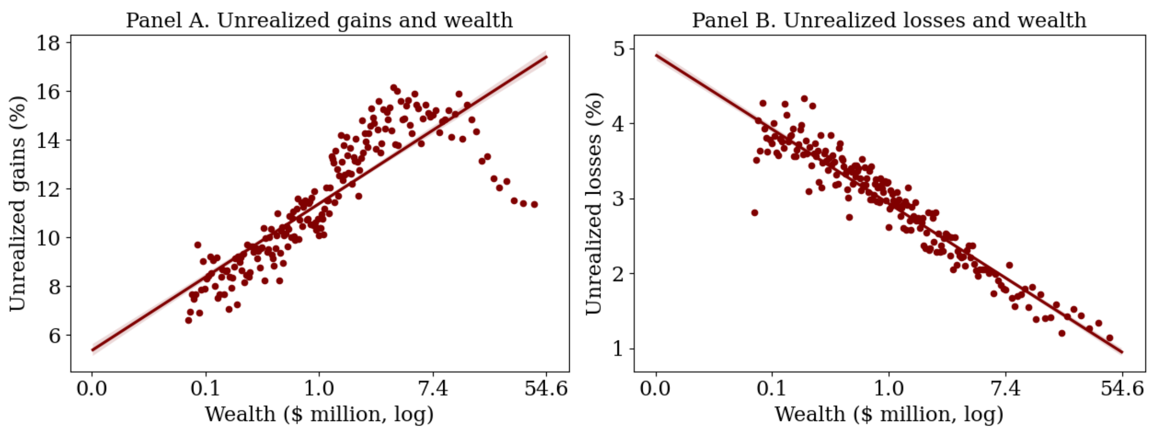
Panel A of Figure 17 reports the relation between unrealized gains ug_{it} and the log of initial wealth $A_{i,t-1}$ across households in March 2023, the last year-month covered by the sample. Unrealized gains are strongly increasing in wealth, ranging from around 8% of total assets for households with wealth around \$100,000 up to 16% for households with

ment/Agency Bonds, Other Government/Agency Bonds, U.S. Equity, Global Equity, Developed Market Equity, Emerging Market Equity, and REITs, while illiquid asset classes include Private Equity & Venture, Hedge Funds, Direct Real Estate, Direct Private Companies, Fund of Funds, Real Estate Funds, Unknown Alternatives, Collectibles, Crypto, Derivatives, Liabilities, Other Equity, Other Debt, Other Funds, and Other Non-Financial Assets. Finally, a special role in Gabaix et al. (2024) is played by Cash which includes positions in Money Market Fund, Certificate of Deposit, Commercial Paper, CAD, CHF, EUR, USD, and Other Currency.

[42]Because the share of total assets invested in liquid asset classes decreases with wealth (Gabaix et al. 2024), I prefer to rescale unrealized gains and losses by total assets rather than assets in liquid asset classes. That said, the results are robust to rescaling unrealized gains and losses by assets in liquid asset classes.

wealth around $10 million. This pattern confirms for the U.S. the positive relation between unrealized gains and wealth documented by Fagereng et al. (2019) for Norway. Panel B plots instead unrealized losses ul_{it} against the log of initial wealth and shows that the sign of the relation is reversed: unrealized losses decrease from 4% to 1% of total assets as wealth increases. Overall, while unrealized gains increase in wealth by a factor of 2 from 8% to 16%, unrealized losses decrease by a factor of 4 from 4% to 1%.

Figure 17: **Unrealized gains and losses across the wealth distribution**
Panel A provides a binned scatterplot of unrealized gains ug_{it} against the log of initial wealth $A_{i,t-1}$ across households. Panel B reports instead a binned scatterplot of unrealized losses ul_{it} against the log of initial wealth $A_{i,t-1}$ across households. The results are presented for March 2023.



The facts in Figure 17 are consistent in principle with two hypotheses. It could be that some households ex-ante select securities that generate higher returns ex-post, and that infrequent rebalancing (due to inertia, inattention, or trading frictions) leads to the accumulation of unrealized gains in their portfolios. Households that ex-ante select better-performing securities will then have ex-post higher wealth, higher unrealized gains, and lower unrealized losses.

Alternatively, it could be that wealthier households are more responsive to tax incentives and realize a larger fraction of capital losses than capital gains to reduce their tax expenses. In fact, realized capital losses can be deducted from capital gains realized in the same fiscal year, and any difference can be carried forward indefinitely to offset capital gains realized in future years.[43] In the next sections, I provide empirical evidence consistent with the latter hypothesis.

[43]If realized capital losses exceed realized capital gains in a fiscal year, the difference can also be deducted from taxable income up to a limit of $3,000.

6.3 Realization of losses and gains across the wealth distribution

Let RG_{it} and RL_{it} denote gains and losses realized by household i in year-month t. Let also $rg_{it} = \frac{RG_{it}}{A_{i,t-1}}$ and $rl_{it} = \frac{RL_{it}}{A_{i,t-1}}$ denote realized gains and losses expressed as a fraction of previous period wealth. If wealthier households are more responsive to tax incentives than less-wealthy households, we would expect that they realize, on average, a larger fraction of unrealized losses in their portfolio and a lower fraction of unrealized gains.

To test this hypothesis, I assign each household i in each year-month t to one of five quintiles g based on initial wealth $A_{i,t-1}$. I then estimate the following panel regression

$$rl_{it} = \alpha_{g(i)t} + \gamma_{g(i)} ul_{i,t-1} + \beta' X_{it} + \varepsilon_{it}, \tag{7}$$

where $\alpha_{g(i)t}$ denotes wealth quintiles times year-month fixed effects, $\gamma_{g(i)} ul_{i,t-1}$ interacts wealth quintile dummies with initial unrealized losses, and X_{it} is a vector of controls which includes turnover in liquid asset classes, the log of initial wealth $A_{i,t-1}$, the portfolio weight on liquid asset classes $\theta_{i,t-1} = \frac{\sum_{a \in \mathcal{L}} A_{ia,t-1}}{A_{i,t-1}}$, the number of months since the portfolio was first boarded onto the platform, and the return r_{it} on the portfolio of household i between $t-1$ and t. For each wealth quintile, $\gamma_{g(i)}$ captures the fraction of losses that are realized on average. I also repeat the estimation of equation (7) with rg_{it} as the outcome variable and $ug_{i,t-1}$ on the right-hand side.

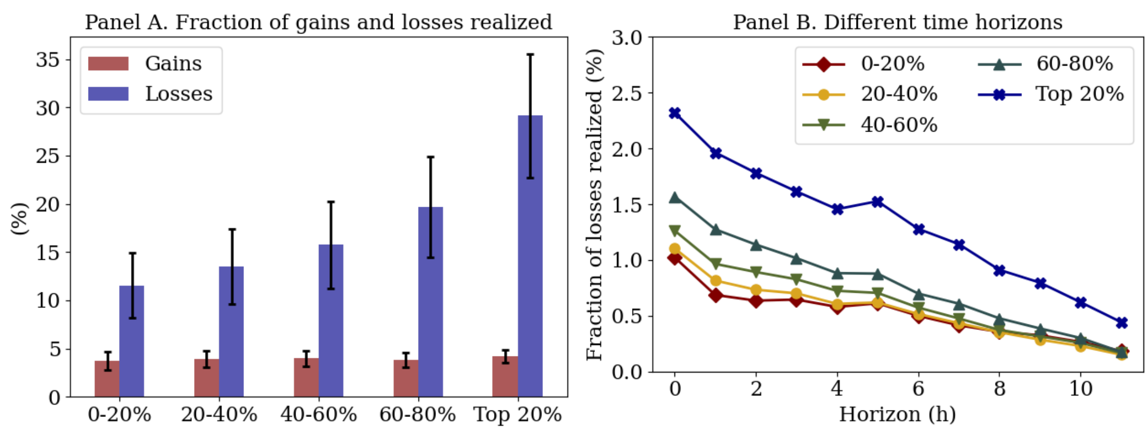
Panel A of Figure 18 reports estimates of $\gamma_{g(i)}$ separately for losses and gains.[44] While households in the bottom 20% of the wealth distribution realize on average 11.52% of unrealized losses annually, the rate increases monotonically in wealth and reaches 29.16% for households in the top 20%. This difference is statistically significant at any significance level. The fraction of gains realized is instead stable across the wealth distribution and, for every wealth quintile, it is lower than the fraction of losses realized. Overall, these results show that while all households realize losses at a higher rate than gains, wealthier households realize losses at a significantly higher rate than less-wealthy households.

One possible explanation behind this result is that less-wealthy households are more inert and they do respond to tax incentives but with a delay. To test this hypothesis, I estimate equation (7) with realized losses $rl_{i,t+h}$ observed h year-months ahead as the outcome variable. Panel B of Figure 18 reports the results for $h \in \{0, 1, .., 11\}$, which show that wealthier households realize a larger fraction of unrealized losses at any horizon h. The difference between the top and bottom 20% remains statistically significant up to $h = 5$ year-months. These results confirm that less-wealthy households do not respond with a

[44] I annualize the estimates by multiplying $\widehat{\gamma}_{g(i)}$ by 12.

delay to tax incentives but, on the contrary, they are unconditionally less responsive than wealthier households.

Figure 18: **The fraction of losses and gains realized**
Panel A provides the fraction of unrealized losses that households in each wealth quintile g realize on average, summarized by the estimate $\widehat{\gamma}_{g(i)}$ in equation (7). I also report the fraction of unrealized gains that households realize on average, which are obtained from estimating equation (7) with rg_{it} as the outcome variable and $ug_{i,t-1}$ on the right-hand side. Panel B reports estimates of equation (7) with the outcome variable being $rl_{i,t+h}$ for different horizons $h \in \{0, 1, .., 11\}$. In Panel A, standard errors are clustered at wealth quintile times year-month level and 95% confidence intervals are provided.



In Appendix I, I provide additional robustness tests to ensure that the results in Figure 18 are not driven by factors other than differences in the response to tax incentives across the wealth distribution. One concern is that households at the lower end of the wealth distribution have a larger fraction of assets invested in retirement accounts (e.g. 401(k) plans and IRAs). Because capital gains realized in these accounts are not taxed, then less-wealthy households may have less incentive to realize capital losses. While the Addepar dataset does not flag whether assets are held within or outside retirement accounts, more than 95% of the assets in retirement accounts in the U.S. are allocated to mutual funds (including balanced funds, target-date funds, and money-market funds).[45] I then repeat the estimation of the results in Figure 18 after restricting the dataset to directly held equity and fixed-income securities.[46] I find that results are unchanged. Households in the bottom 20% still realize 8.40% of unrealized losses compared to 21.00% for households in the top

[45]See, for instance, the annual report issued for 2022 by the Investment Company Institute: https://www.ici.org/system/files/2024-04/per30-03.pdf

[46]I include in the estimation directly held government bonds, corporate bonds, convertible bonds, municipal bonds, ABS/MBS, common stocks, preferred stocks, and convertible stocks. I also include depository receipts on equity and debt that represent stocks and bonds issued by foreign corporations and publicly traded in U.S. markets.

20%. Moreover, I cannot reject the null hypothesis that the new estimates are all equal to the original ones.

One additional concern could be that households in the top 20% are not necessarily more responsive to tax incentives but, rather, they follow trading strategies (e.g. momentum) that require selling losing assets. In Appendix I, I show that this is unlikely to be the case by looking at discontinuities in the purchase and sale of assets in a narrow interval around zero unrealized returns. Both tax-loss harvesting and momentum require selling losing securities. However, momentum also requires purchasing winning securities. I find that households in the top 20% increase discontinuously the sale of assets with unrealized returns slightly below zero but they do not increase discontinuously the purchase of assets with unrealized returns slightly above zero. This result is consistent with tax-loss harvesting but not with momentum. Interestingly, the magnitude of the discontinuity in sales increases monotonically in wealth, confirming that wealthier households are more responsive to tax incentives.

6.4 The role of financial advisors

Informed by the correlation between wealth and advisor types in Figure 16, I investigate how different advisors respond to tax incentives. To this end, I modify equation (7) and estimate

$$rl_{it} = \alpha_{v(i)t} + \gamma_{v(i)} ul_{i,t-1} + \beta' X_{it} + \varepsilon_{it}, \tag{8}$$

where $\alpha_{v(i)t}$ denotes advisor type times year-month fixed effects and $\gamma_{v(i)} ul_{i,t-1}$ interacts advisor type dummies with initial unrealized losses. The coefficient $\gamma_{v(i)}$ captures the fraction of unrealized losses that are realized on average by households advised by a given type of financial advisor. As before, I also repeat the estimation of equation (8) for realized gains and for different time horizons $h \in \{0, 1, .., 11\}$.

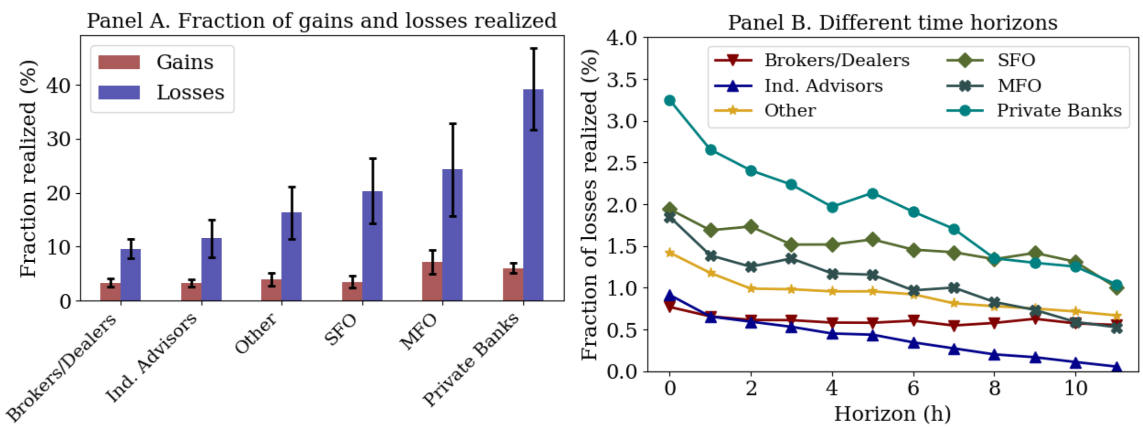
Panel A of Figure 19 reports estimates of $\gamma_{v(i)}$ separately for gains and losses. It shows that the gradient in the fraction of realized losses by advisor type is significantly steeper than the gradient by wealth, suggesting that some types of advisors (family offices and private banks) specialize in tax optimization. Interestingly, private banks stand out as the most tax-sophisticated financial advisor. This is consistent with recent industry trends, where leading private banks are increasingly specializing in tax optimization for wealthy households and aggressively entering this market through acquisitions of tax-oriented financial advisors.[47] Because family offices and private banks are accessible only above

[47] See, for instance, JPMorgan's acquisition of 55ip in 2020 and Morgan Stanley's acquisition of Parametric in 2021. Asset managers are also entering this industry as demonstrated by BlackRock's acquisition of Aperio in

minimum levels of wealth, access to sophisticated advisors appears to be an important mechanism behind the different responses to tax incentives across the wealth distribution.

Panel B reports the fraction of losses realized at different horizons. Private banks realize a larger fraction of losses compared to other advisors at virtually any horizon $h \in \{0, 1, .., 11\}$. The decrease in the fraction of losses realized is, however, faster for private banks than for SFOs which seem somewhat more inert.

Figure 19: **The fraction of losses and gains realized**
Panel A provides the fraction of unrealized losses that households advised by different financial advisors realize on average, summarized by the estimate $\widehat{\gamma}_{v(i)}$ in equation (8). I also report the fraction of unrealized gains that households realize on average, which are obtained from estimating equation (8) with rg_{it} as the outcome variable and $ug_{i,t-1}$ on the right-hand side. Panel B reports estimates of equation (8) with the outcome variable being $rl_{i,t+h}$ for different horizons $h \in \{0, 1, .., 11\}$. In Panel A, standard errors are clustered at advisor type times year-month level and 95% confidence intervals are provided.



6.5 Tax alpha

Given the substantial difference in the response to tax incentives across the wealth distribution, a natural question is whether these differences have quantitatively large distributional effects.

For each household i, I start by computing total gains and losses realized in each fiscal year. I then follow the prescriptions of the internal revenue code to construct taxable capital gains. To do that, I first subtract losses from gains realized in the same fiscal year. If, in a given fiscal year, realized losses exceed realized gains, I bring forward the difference and

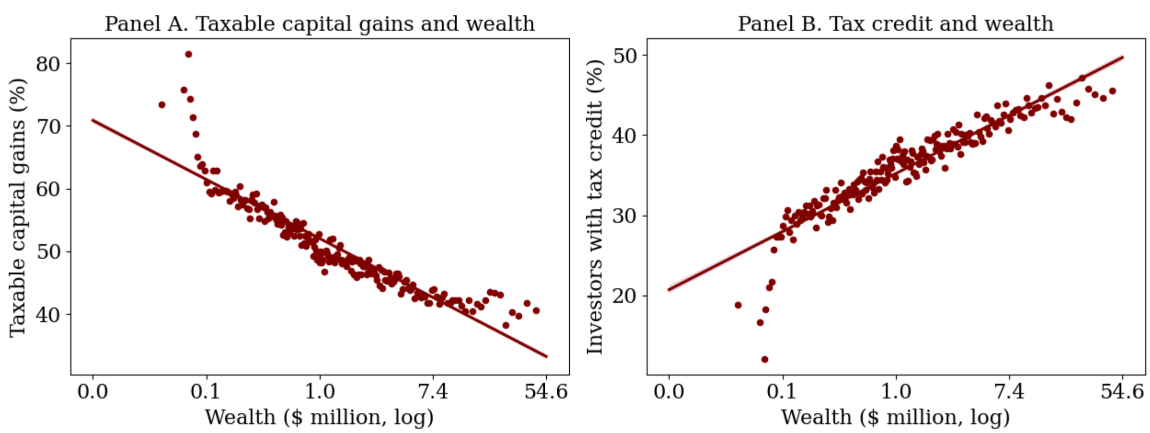
2021, Vanguard's acquisition of Just Invest in 2021, and, again, BlackRock's acquisition of SpiderRock Advisors in 2024.

I subtract it from taxable capital gains in the first fiscal year in which they are positive. The ratio of taxable capital gains over total realized gains captures the fraction of total realized gains that are taxable after deducting realized losses.

In Panel A of Figure 20, I report the relation between taxable capital gains over total realized gains and initial log wealth $A_{i,t-1}$ across all household-years in the dataset. Given the higher fraction of losses realized by wealthier households, taxable capital gains decrease strongly in wealth from 70% of total realized gains for households with wealth around \$100,000 to 40% for households with wealth around \$50 million. This result implies that 60% of realized gains are not taxable, on average, for households at the top of the wealth distribution.

In Panel B, I further report the fraction of household-years for which I observe zero taxable capital gains or, equivalently, a tax credit that can be carried forward to future years. Around 50% of households with wealth around \$50 million report zero taxable capital gains compared to 25% of households with wealth around \$100,000.

Figure 20: **Taxable capital gains across the wealth distribution**
For each household-year in the dataset, Panel A provides a binned scatterplot of the taxable capital gains over total realized gains on the y-axis against the log of total wealth observed, for each household, at the beginning of each year. Panel B provides instead the fraction of household-years for which I observe zero taxable gains or, equivalently, a tax credit that can be carried forward to future years.



In order to translate realized gains net of realized losses into a tax expense, two further steps are required. First, for each household-year, I separate long-term and short-term gains. Short-term gains are defined as those realized within 12 months from the initial purchase of the security and are taxed at income tax rates rather than capital gain tax rates. I distinguish long-term and short-term capital gains based on the year-months in which a given security was purchased and subsequently sold by a given household.[48]

Second, because the dataset does not provide household taxable income, I need to make assumptions about the applicable tax rate. To this end, I use the highest statutory tax rates on income and capital gains for short-term and long-term capital gains, respectively.[49] I multiply long-term and short-term capital gains by the relevant tax rate to obtain a tax expense in dollar terms for each household and fiscal year. I then rescale the resulting tax expense by the wealth in liquid asset classes observed for each household at the beginning of each fiscal year.

In Figure 21, I report the relation between the tax expense and the initial log wealth $A_{i,t-1}$ across all household-years in the dataset. The relation is strongly negative and the gradient is economically significant. While taxes on capital gains amount to 1% of initial wealth for households with wealth around $100,000, they amount to 0.5% for households with wealth around $50 million. The difference of 50bps is large, especially when considering that it can be achieved for any desired risk exposure and at minimum incremental risk. It therefore represents a virtually risk-free tax alpha gained by wealthier households.

Overall, these results suggest that differences in the response to tax incentives have important distributional effects, with wealthier households better positioned to take advantage of the current tax system.

7 Conclusion

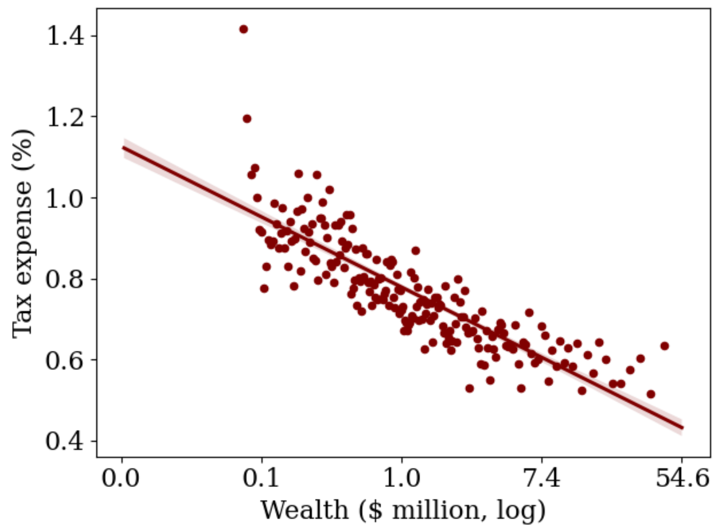
This paper studies the role of fiscal policy as a driver of capital flows, the cost and supply of financial products, and asset prices.

In the first part of the paper, I use difference-in-differences designs around the passage of fiscal reforms that shifted household demand for financial products and I find that fiscal policy has large and persistent effects on capital flows. Quantitatively, fiscal reforms in-

[48] The distinction between short-term and long-term gains and losses complicates slightly the construction of taxable long-term and short-term capital gains. I proceed as prescribed by the internal revenue code and first construct taxable long-term capital gains by offsetting long-term gains and long-term losses realized in the same fiscal year. I also construct taxable short-term capital gains by offsetting short-term gains and short-term losses realized in the same fiscal year. If the difference between realized losses and gains is positive in any of the two groups, I use it to offset any positive taxable gain in the other category. If the difference is negative in both categories, both differences are carried forward and used to offset taxable capital gains in the same category in the first fiscal year in which they are positive. If positive taxable gains in the same category are not available, then the difference carried forward from previous years can be used to offset taxable gains in the other category.

[49] The highest statutory tax rate on income is equal to 43.4% until fiscal year 2017 and 40.8% thereafter. The highest statutory tax rate on capital gains is equal to 23.8% in all fiscal years covered by the dataset.

Figure 21: **Taxes on capital gains and performance**
For each household-year in the dataset, Panel A provides a binned scatterplot of the annual tax expense, including taxes on both long-term and short-term capital gains, relative to the initial wealth in U.S. Equity against the log of total wealth observed, for each household, at the beginning of each year.



creased the annual growth rate of assets under management by 22.56 percentage points for retirement share classes, 4.15 percentage points for real estate trusts, 9.37 percentage points for ETFs, and 5.54 percentage points for life insurance companies. These effects translate into an elasticity of capital flows equal to 0.83 for contribution limits, 1.60 for income tax rates, 1.59 for dividend tax rates, and 1.56 for capital gain tax rates over two to four years.

To put these numbers in perspective, I compare the effects with the long-term cumulative capital flow observed for each institutional sector over two decades, between 2000Q1 and 2020Q4. I find that the response of capital flows to fiscal reforms explains up to 53% of institutions' long-term growth. Because different institutional sectors face different regulations, funding structures, and capital requirements, this result highlights a strong interaction between fiscal policy, financial development, and financial stability

Given these large shifts in demand, I then examine whether financial institutions respond to fiscal reforms by adjusting their fee structures. I find that when the effect on market entry is limited, institutions increase their fees by 11 basis points following favorable reforms. This increase is mostly driven by institutions that enter the market after the reform is passed, which increase fees by 19 basis points. To put these numbers in perspective, for every dollar of tax savings, financial institutions retain up to 21 cents. However, when reforms trigger significant entry of new institutions, the increase in fees is not significant, consistent with a rise in competition.

In the second part of the paper, I investigate how the capital reallocation driven by fiscal reforms affects asset prices. I find that when demand increases by 1% on impact of a fiscal reform then returns increase by 4.24 percentage points annually (1.06 percentage points quarterly) over three years after the reform is passed. While the implied multiplier of 1.06 is within the range of estimates proposed by a growing literature on demand estimation in financial markets, the time horizon that I consider is significantly longer, suggesting that fiscal reforms have a very persistent effect not only on capital allocation but also on the cross-section of asset prices.

Given the large effects of fiscal reforms on capital flows, the third part of the paper studies which households are more responsive to tax incentives. To this end, I use detailed household-level data from Addepar, which includes holdings, flows, realized and unrealized gains at high frequency, for virtually all asset classes and across the wealth distribution, including UHNW households with wealth above $100 million. Given the unique feature of the dataset, I focus on the response of households to capital gain taxes, the largest source of tax expenditure for the U.S. federal government.

I find that households in the top wealth quintile realize three times the fraction of losses realized by households in the bottom wealth quintile, indicating greater tax awareness. Access to tax-sophisticated advisors (private banks and family offices) is strongly correlated with the higher tax awareness of wealthier households. In fact, family offices and, especially, private banks stand out as the most tax-sophisticated financial advisors, consistent with recent industry trends.

The stronger response of wealthier households to tax incentives carries substantial distributional effects. I estimate that UHNW households deduct on average 60% of their realized gains against realized losses. In terms of performance, households at the low end of the wealth distribution pay about 1% annually in capital gains taxes relative to their wealth, compared to 0.50% for UHNW households. This tax alpha of 50 basis points per year is economically meaningful, especially considering that it can be achieved for any desired risk exposure and with minimal incremental risk.

Taken together, the three parts of this paper have the objective to paint a comprehensive picture of how fiscal policy shapes the financial system. Fiscal reforms are followed by significant changes in the structure of the financial system, which in turn affects fee structures and asset prices. However, the benefits of these changes are not evenly distributed, with wealthier households better positioned to take advantage of tax incentives. These findings have important implications for the design of fiscal policy and its role in shaping both financial markets and wealth inequality.

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ONLINE APPENDIX

A Details on data construction

In this section, I provide details on the construction of the datasets used in the paper.

A.1 Financial institutions

A.1.1 CRSP Survivor-Bias Free dataset

The CRSP Survivor-Bias-Free dataset provides assets under management, returns, style, fees, the first date on which the fund was offered to the public, the fund name, an ETF identifier, and a variable annuity identifier at share class level. Share classes are uniquely identified by the CRSP Fund Number (*crsp_fundno*). The dataset also provides a separate identifier for mutual funds (which can offer multiple share classes at the same time) called CRSP Class Group (*crsp_cl_grp*). Information on assets under management and returns are available at monthly frequency starting from January 1960 (I will refer to this dataset as the "return dataset"). All other information is instead available at quarterly frequency starting from 1999Q1 (I will refer to this dataset as the "summary dataset").

I start from the return dataset and I construct monthly dollar flows for each share class i and year-month t as $F_{it} = A_{it} - A_{i,t-1}(1 + r_{it})$, where A_{it} denotes assets under management and r_{it} denotes the return between $t-1$ and t. I then convert the dataset to quarterly frequency. For each share class and year-quarter, I sum F_{it} across year-months and I keep the last available A_{it}. I also construct quarterly returns in the standard way $\prod_t (1 + r_{it}) - 1$. Using the quarterly dataset, I define flows relative to assets under management as $f_{it} = \frac{F_{it}}{A_{i,t-1}}$ where, with a little abuse of notation, I now use t to index year-quarters rather than year-months. Finally, to avoid the incubation bias documented in Evans (2010), I restrict the dataset to share classes with assets under management greater than \$10 million both in the current and in the previous year-quarter, i.e. share classes that satisfy $A_{it} \geq \$10$ million and $A_{i,t-1} \geq \$10$ million.

Before merging the return and summary datasets, I correct some infrequent data errors in the summary dataset. First, while the ETF and variable annuity identifiers (*et_flag* and *vau_fund* respectively) are unique for each share class, these identifiers are frequently missing in the first and last year-quarters in which a share class appears in the dataset. For each share class, I refill these missing values using the values of the identifiers observed in other year-quarters.

For each share class, I also refill infrequent missing values observed for its style (*crsp_obj_cd*) and the mutual fund identifier (*crsp_cl_grp*) by carrying forward non-missing values. If missing values are still present, I refill them by carrying backward non-missing values. This refill of missing values has a minor impact and it only affects 0.45% of all observations for *crsp_obj_cd* and 0.14% of all observations for *crsp_cl_grp*. After correcting these infrequent data errors, I merge the return and summary datasets by *crsp_fundno* and year-quarter.

In the merged dataset, I identify ETFs and share classes underlying variable annuities using the ETF (*et_flag*) and variable annuity (*vau_fund*) identifiers provided in the summary dataset. Specifically, I classify as an ETF any share class for which I observe *et_flag* equal to “F”. I classify as a variable annuity any share class for which I observe *vau_fund* equal to “Y”.

Differently from ETFs and variable annuities, retirement share classes are not directly identifiable in the CRSP dataset. However, in the majority of the cases, the fund name is complemented by the symbol of the share class being offered. I identify retirement share classes as those with name that includes any of the following strings: “Class R”, “Class R1”, “Class R2”, “Class R3”, “Class R4”, “Class R5”, “Class R6”, “Class R-1”, “Class R-2”, “Class R-3”, “Class R-4”, “Class R-5”, “Class R-6”. If a share class is marked as a retirement share class in any year-quarter, I classify it as a retirement share class throughout.

I conclude the cleaning steps by winsorizing flows f_{it} and returns r_{it} at the 2.5% and 97.5% percentiles by style and year-quarter.

A.1.2 CRSP Mutual Fund Holdings dataset

To estimate the effect of fiscal reforms on asset prices discussed in Section 5, I need information on equity holdings of mutual funds and ETFs, which I obtain from the CRSP Mutual Fund Holdings dataset. This dataset provides the portfolio shares on publicly traded stocks at quarterly frequency starting from 2010Q1. Because multiple share classes hold the same portfolio of stocks, CRSP provides this information at portfolio level (*crsp_portno*) and not at share class level.

I merge the CRSP Mutual Fund Holding dataset into the dataset discussed in Appendix A.1.1 by *crsp_portno* and year-quarter. To obtain dollar holdings A_{iat} of share class i on asset a and year-quarter t, I simply multiply the portfolio share (*percent_tna*) provided by CRSP by total assets A_{it} observed for share class i in year-quarter t.

A.1.3 REITs dataset

The dataset used for REITs combines two sources: the CRSP Monthly Stock File and the Ziman REIT dataset.

From the CRSP Monthly Stock File I observe monthly prices (*prc*), returns (*ret*), shares outstanding (*shrout*), SIC code (*hsiccd*), share code (*shrcd*), cumulative factor to adjust shares (*cfacshr*), and cumulative factor to adjust prices (*cfacpr*) at security level. I restrict the dataset to REITs (share code equal to 18) and ordinary common shares (share code equal to 10 or 11) that operate in any of the SIC code observed for REITs.

I follow Boudoukh et al. (2007) and Belo et al. (2019) to construct monthly net equity issuance as $F_{it} = \left(S_{it} - S_{i,t-1}\right)\left(P_{it} + P_{i,t-1}\right)/2$, where S_{it} denotes the split-adjusted number of shares outstanding for security i in year-month t and P_{it} the split-adjusted price. I then convert the dataset to quarterly frequency like I did for the CRSP mutual fund dataset. For each security and year-quarter, I sum F_{it} across year-months and I keep the last available market capitalization, which I compute as price multiplied by shares outstanding. In the quarterly dataset, I construct net equity issuance relative to the market capitalization as $f_{it} = \frac{F_{it}}{A_{i,t-1}}$, where A_{it} denotes the market capitalization of security i at the end of year-quarter t. To limit the impact of outliers, I winsorize f_{it} at the 2.5% and 97.5% percentiles by year-quarter.

I complement information in the CRSP Monthly Stock File with details on the type of investments in which REITs specialize from the Ziman REIT dataset. This dataset provides three variables to classify the investments in which REITs specialize: property type (*ptype*), REIT type (*rtype*), and sub-property type (*psub*). I use the property type as it separately identifies REITs that specialize in mortgages, as well as REITs that specialize in mortgage-backed securities and residential real estate. I convert the Ziman REIT dataset from monthly to quarterly frequency by keeping the last available property type for each REIT (as identified by the CRSP *permno*) and year-quarter. I then merge this dataset with the CRSP Monthly Stock File converted to quarterly frequency by *permno* and year-quarter.

A.1.4 Firm characteristics

All firm characteristics are from Compustat North America Fundamentals Quarterly and Annual datasets.

Using the quarterly dataset, I follow Jensen et al. (2022) to construct book equity (*be*), operating income after depreciation (*ebit*), net income (*ni*), and total debt (*debt*). I construct asset growth as the quarterly growth in total assets (*at*), leverage as total debt divided by total assets, EBIT to total assets as operating income after depreciation divided by lagged total assets, net income to total assets as net income divided by lagged total assets. All firm characteristics are lagged by two year-quarters to make sure that they were publicly available at the time when they are used.

Using the annual dataset, I construct all firm characteristics already discussed for the quarterly dataset, which I lag by one year to make sure that they were publicly available information at the time they are used. I also construct the net equity issuance measure (*eqnetis*) defined in Jensen et al. (2022). This annual net equity issuance measure will be used to check that the results for REITs discussed in Section 3.4 are not driven by the choice of the equity issuance measure from Boudoukh et al. (2007).

To limit the impact of outliers, I winsorize the quarterly growth in total assets, EBIT to total assets, and net income to total assets at the 2.5% and 97.5% percentiles by year-quarter or year. Because leverage is by construction always positive, I winsorize it at the 97.5 percentile by year-quarter or year.

A.2 Addepar

The construction process of the Addepar dataset follows Gabaix et al. (2024) with minor differences that are specifically designed to correct infrequent data errors in unrealized and realized returns. Importantly, because part of the analysis in this paper is performed at security-level, I adapt the cleaning process that Gabaix et al. (2024) implement on the dataset at asset class level and I implement it on the security-level dataset.

For each household i, security a, and year-month t, let A_{iat} denote the end-of-month dollar holding, $A_{ia,t-1}$ the beginning-of-month dollar holding, F_{iat} the dollar flow, and $R^{\$}_{iat}$ the dollar return. Let also UR_{iat} denote the dollar unrealized return and RR_{iat} the dollar return realized during year-month t. I start by removing the year-month in which a household appears on the platform for the first time as beginning-of-period holdings are unknown for some or all of the securities. Although it is rare for households to leave the platform, I also remove the last year-month in which a household is observed on the platform as a similar logic applies for end-of-period assets.

Second, for each household i, year-month t, and security a, the following intertemporal budget constraint should hold in the data

$$A_{iat} = A_{ia,t-1} + R^{\$}_{iat} + F_{iat},$$

where A_{iat} denote the end-of-month dollar holdings, $A_{ia,t-1}$ the beginning-of-month dollar holdings, F_{iat} the dollar flow, and $R^{\$}_{iat}$ the dollar return. I then remove household-month observations when a security that belongs to a liquid asset class has a missing item of the budget constraint. I follow the definition of liquid asset classes in Gabaix et al. (2024), which mainly include equity and fixed-income positions that can be easily traded in public markets.[50] I also remove household-month observations if the budget constraint does not hold for at least one security that is classified as part of a liquid asset class. I allow for a small margin of error of $1,000 or 0.5% of the average (absolute value) of the beginning-of-month and end-of-month holdings.

Third, for a small fraction of observations, the beginning-of-month and end-of-month holdings coincide. While this can happen for cash accounts, it is unlikely to be correct for risky assets. Confirming this prior, in these cases flows are often the negative of dollar returns. This happens because the system has additional information about either the dollar return or the flow, and completes the missing items in those instances to ensure that the budget constraint holds. Moreover, unrealized returns are frequently extreme for these observations. I then set dollar returns, dollar flows, and unrealized returns to zero for these observations to the extent that they are classified as part of a liquid asset class.

I implement two additional corrections on unrealized returns. First, for a minority of observations, I observe positive beginning-of-month holdings $A_{ia.t-1} > 0$, end-of-month holdings equal to zero $A_{iat} = 0$, and the dividends distributed by the security during year-month t coincide with the beginning-of-period holding $A_{ia,t-1}$. These observations typically correspond to instances when the household has sold a position in a security before year-month t, but after a dividend was declared. The household is still entitled to receive the dividend which is distributed in year-month t. I find that these observations frequently have positive unrealized returns which is likely a data error since the household has already liquidated its position in the security. Moreover, the household has no residual claim on future cash flows since the dividend was distributed in year-month t. For these observations, I set UR_{iat} to zero.

I also set UR_{iat} to zero for a minority of observations for which I observe beginning-of-period holdings equal to zero $A_{ia,t-1} = 0$, end-of-period holding A_{iat} and unrealized return UR_{iat} both equal to the dollar return $R^{\$}_{iat}$. These are instance in which a household

[50] More specifically, liquid assets classes in Gabaix et al. (2024) include Municipal Bonds, U.S. Government/Agency Bonds, Corporate Bonds, Bond Funds, ABS/MBS, Structured Debt, International Government/Agency Bonds, Other Government/Agency Bonds, U.S. Equity, Global Equity, Developed Market Equity, Emerging Market Equity, and REITs, while illiquid asset classes include Private Equity & Venture, Hedge Funds, Direct Real Estate, Direct Private Companies, Fund of Funds, Real Estate Funds, Unknown Alternatives, Collectibles, Crypto, Derivatives, Liabilities, Other Equity, Other Debt, Other Funds, and Other Non-Financial Assets. Finally, a special role in Gabaix et al. (2024) is played by Cash which includes positions in Money Market Fund, Certificate of Deposit, Commercial Paper, CAD, CHF, EUR, USD, and Other Currency.

has liquidated its position before year-month t, but it is still entitled to receive the dividend whose amount is declared in year-month t. Because in these cases UR_{iat} capture a claim over future cash flows rather than an unrealized capital gain, I set UR_{iat} for these observations to zero. I remark that, other than in these corrected instances, UR_{iat} does not include dividends but only unrealized capital gains.

Finally, I drop household-month observations for which I observe less than $100,000 in total assets. This screen mitigates the concern that, for these households, I only observe part of their portfolios. I then restrict the dataset to liquid asset classes and collapse the dataset to household-month level by summing, holdings A_{iat}, dollar flows F_{iat}, unrealized returns UR_{iat}, and realized returns RR_{iat} across securities. I also construct household-level unrealized gains and losses as

$$UG_{it} = \sum_a UR_{iat}\mathbb{I}\left(UR_{iat} > 0\right),$$
$$UL_{it} = -\sum_a UR_{iat}\mathbb{I}\left(UR_{iat} < 0\right),$$

where $\mathbb{I}\left(UR_{iat} > 0\right)$ is an indicator equal to 1 if $UR_{iat} > 0$. I construct household-level realized gains RG_{it} and losses RL_{it} analogously

$$RG_{it} = \sum_a RR_{iat}\mathbb{I}\left(RR_{iat} > 0\right),$$
$$RL_{it} = -\sum_a RR_{iat}\mathbb{I}\left(RR_{iat} < 0\right).$$

Notice that unrealized and realized gains and losses are all non-negative by construction. To conclude the data construction process, I construct unrealized gains and losses expressed as a fraction of total wealth (including liquid and illiquid asset classes) as $ug_{it} = \frac{UG_{it}}{A_{it}}$ and $ul_{it} = \frac{UL_{it}}{A_{it}}$, respectively. Total wealth is defined as $A_{it} = \sum_a A_{iat}$. Similarly, I construct realized gains and losses as a fraction of beginning-of-month total wealth as $rg_{it} = \frac{RG_{it}}{A_{i,t-1}}$ and $rl_{it} = \frac{RL_{it}}{A_{i,t-1}}$. Because the share of total wealth invested in liquid asset classes decreases with wealth (Gabaix et al. 2024), I prefer to rescale unrealized and realized gains and losses by total wealth rather than wealth in liquid asset classes. To limit the impact of potential remaining outliers, I winsorize ug_{it}, ul_{it}, rg_{it}, and rl_{it} at the 97.5% percentile by year-month.

A.3 Summary statistics

For each year between 1999 and 2020, Table 2 provides total assets under management and the number of institutions for each of the four financial products studied in the paper (retirement share classes, variable annuities, real estate trusts, and ETFs). Table 3 provides the average flow and the average expense ratio charged by retirement share classes and ETFs.

Table 2: **Total assets and number of institutions**
For each year between 1999 and 2020, this table reports total assets under management and the number of institutions for each of the four financial products studied in the paper. Assets are in trillions of dollars.

Year	Retirement share classes		Variable annuities		Real estate trusts		ETFs	
	Assets	N	Assets	N	Assets	N	Assets	N
1999	0.035	129	1.036	1,368	0.264	169	0.032	12
2000	0.037	157	1.025	1,542	0.275	159	0.063	41
2001	0.035	157	0.944	1,733	0.270	148	0.080	60
2002	0.035	194	0.772	1,882	0.310	142	0.094	65
2003	0.057	288	0.967	2,059	0.354	138	0.134	75
2004	0.091	365	1.158	2,229	0.459	145	0.187	99
2005	0.134	479	1.181	2,325	0.607	164	0.225	123
2006	0.195	614	1.378	2,437	0.661	158	0.290	192
2007	0.255	766	1.493	2,521	0.686	137	0.393	284
2008	0.183	773	1.128	2,682	0.589	130	0.330	316
2009	0.269	920	1.129	2,920	0.530	127	0.454	394
2010	0.332	1,042	1.366	2,985	0.579	138	0.586	434
2011	0.329	1,131	1.218	2,836	0.745	142	0.624	482
2012	0.396	1,267	1.428	3,012	0.913	149	0.770	513
2013	0.540	1,497	1.678	3,011	1.136	161	1.037	543
2014	0.619	1,678	1.748	3,012	1.216	180	1.260	572
2015	0.683	1,779	1.703	3,008	1.299	187	1.277	620
2016	0.785	1,971	1.751	2,978	1.275	185	1.593	660
2017	1.115	2,183	1.890	3,020	1.315	184	2.060	723
2018	1.125	2,337	1.688	3,056	1.372	184	2.029	805
2019	1.441	2,393	1.927	3,098	1.596	184	2.711	853
2020	1.697	2,385	2.079	3,076	1.527	185	3.411	896

Table 3: **Flows and fees**
For each year between 1999 and 2020, this table reports average flows to each of the four financial products studied in the paper. For retirement share classes and ETFs, I also report the average expense ratio charged.

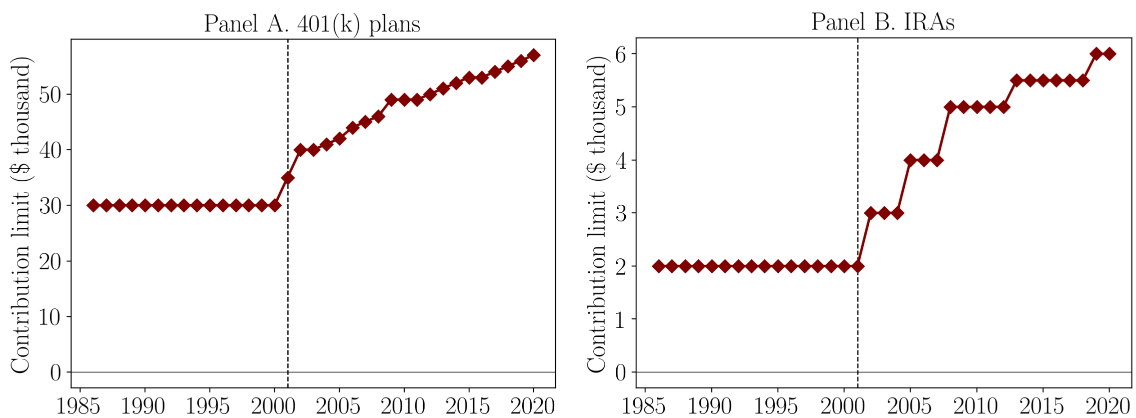
Year	Retirement share classes		Variable annuities	Real estate trusts	ETFs	
	Flows	Fees (%)	Flows	Flows	Flows	Fees (%)
1999	0.017	0.968	0.341	0.009	0.282	0.375
2000	0.036	0.981	0.202	0.002	0.275	0.366
2001	0.022	0.950	0.419	0.018	0.225	0.364
2002	0.103	0.888	0.202	0.019	0.163	0.386
2003	0.158	0.944	0.207	0.025	0.126	0.409
2004	0.115	0.988	0.137	0.020	0.146	0.364
2005	0.090	1.006	0.327	0.019	0.145	0.386
2006	0.094	1.001	0.238	0.020	0.137	0.429
2007	0.110	0.998	0.221	0.014	0.129	0.514
2008	0.068	0.978	0.183	0.014	0.099	0.527
2009	0.066	0.963	0.027	0.040	0.079	0.570
2010	0.045	0.935	0.020	0.037	0.057	0.568
2011	0.038	0.916	0.012	0.030	0.049	0.543
2012	0.033	0.906	0.014	0.031	0.027	0.529
2013	0.038	0.872	0.013	0.032	0.085	0.516
2014	0.035	0.851	0.008	0.027	0.062	0.515
2015	0.032	0.831	0.003	0.017	0.039	0.523
2016	0.020	0.820	-0.003	0.017	0.039	0.517
2017	0.024	0.807	-0.009	0.021	0.045	0.517
2018	0.011	0.782	-0.005	0.015	0.039	0.492
2019	0.000	0.766	-0.007	0.021	0.022	0.483
2020	0.000	0.746	-0.005	0.016	0.048	0.478

A.4 Contribution limits and average marginal tax rates

Information about contribution limits on 401(k) plans and Individual Retirement Accounts (IRAs) are from the Internal Revenue Service (IRS). Panel A of Figure 22 provides the annual time-series of the total contribution limit on 401(k) plans since 1986. In any fiscal year, the sum of the contribution made by an individual employee and the contribution made on her behalf by the employer cannot exceed this limit. Panel B provides instead the time-series of the contribution limit on IRAs. Both limits remained unchanged for almost twenty years, between 1982 and 2001 when the Economic Growth and Tax Relief Reconciliation Act (highlighted by the vertical line) increased both. Since then, both limits have been constantly increasing since the 2001 Economic Growth and Tax Relief Reconciliation

Act indexed their growth to annual inflation rates for fiscal years after 2005. To construct the elasticity of capital flows with respect to the change in contribution limits on defined contribution plans, I rescale the estimated effect on capital flows by the percentage change in the sum of the two limits between 2000 and 2005. I find this percentage change to be equal to 36.29%.

Figure 22: **Contribution limits on 401(k) plans and IRAs**
Panel A provides the time series of the total contribution limit on 401(k) plans while Panel B reports the time series of the contribution limit on IRAs. The vertical line highlights fiscal year 2001, when the Economic Growth and Tax Relief Reconciliation Act was passed. The sample period is from 1986 to 2020 and data are from the IRS.



Turning to the construction of the average marginal tax rates, the IRS publishes annually the aggregate taxable income and the number of filings recorded for 18 different income buckets: (i) < $5,000; (ii) $5,000-$10,000; (iii) $10,000-$15,000; (iv) $15,000-$20,000; (v) $20,000-$25,000; (vi) $25,000-$30,000; (vii) $30,000-$40,000; (viii) $40,000-$50,000; (ix) $50,000-$75,000; (x) $75,000-$100,000; (xi) $100,000-$200,000; (xii) $200,000-$500,000; (xiii) $500,000-$1,000,000; (xiv) $1,000,000-$1,500,000; (xv) $1,500,000-$2,000,000; (xvi) $2,000,000-$5,000,000; (xvii) $5,000,000-$10,000,000; (xviii) > $10,000,000. For each income bucket, I first compute the taxable income of the average household as the aggregate taxable income divided by the number of filings. I then assign to each income bucket a marginal tax rate on income, dividends, and capital gains based on the taxable income of the average household in that bucket. Finally, I construct the average marginal tax rate on income, dividends, and capital gains across income buckets, where the average is weighted by the share of aggregate taxable income observed in each bucket.

The procedure is made slightly more complicated by the fact that different types of filers (e.g., single filers, joint filers etc) face different marginal tax rates on income, dividends, and capital gains. However, the IRS provides the aggregate taxable income and the number of filings for different income buckets separately for four filer types (married filers filing jointly, married filers filing separately, single filers, heads of households). I am therefore able to account for differences in filer types when constructing the average marginal tax rates.

For each reform, I then compute the percentage changes in the average marginal tax rates between the last fiscal year before the reform was passed and the last fiscal year covered in the estimation of the DiD. These percentage changes are used to rescale the DiD estimates and convert them into the elasticities discussed in Section 3.6. Importantly, to avoid that the percentage changes in the average marginal tax rates capture changes in taxable income due to household behavioral response to the reforms, the weights used to compute the average marginal tax rates are fixed to those observed in the fiscal year before each reform was passed.

Tables 4 to 8 report the inputs required to compute the average marginal tax rates. For each of the 18 income buckets and for each of the four filer types (married filers filing jointly, married filers filing separately, single filers, heads of households), Table 4 provides the average personal income. Table 5 provides the income shares. Tables 6 and 7 report the marginal personal income tax rate and the capital gain tax rate assigned to each bucket and to each filer type based on the average personal income. Table 8 provides the average personal income tax rates and average capital gain tax rates computed as weighted average of the tax rates across buckets and filer types with weights given by the income shares.

Table 9 provides the percentage change in the average tax rates between the last fiscal year before the reform was passed and the last fiscal year covered in the estimation of the DiD. These percentage changes are used to construct the elasticities discussed in Section 3.6. For completeness, I also report in Table 10 the percentage changes in the average keep rates, defined as one minus the average tax rate, and in Table 11 the changes in average tax rates.

Table 4: **Average personal income**
This table reports the average personal income in each of the 18 income buckets published by the IRS and for the four filer types (married filers filing jointly, married filers filing separately, single filers, heads of households). For each income bucket and for each filer type, the average personal income is computed as aggregate taxable income divided by the number of filings. Numbers are reported in thousands of dollars.

Year	< 5	5 - 10	10 - 15	15 - 20	20 - 25	25 - 30	30 - 40	40 - 50	50 - 75	75 - 100	100 - 200	200 - 500	500 - 1,000	1,000 - 1,500	1,500 - 2,000	2,000 - 5,000	5,000 - 10,000	> 10,000
	Joint filers																	
2000	0.000	0.000	0.822	3.039	6.653	10.711	17.444	26.321	41.208	61.545	100.734	244.109	604.749	1,096.282	1,566.166	2,728.486	6,261.254	23,656.580
2005	0.000	0.000	0.932	3.446	7.545	12.147	19.784	29.850	46.734	69.798	114.242	276.844	685.845	1,243.291	1,776.185	3,094.371	7,100.875	26,828.879
2012	0.000	0.000	0.000	0.377	2.183	5.358	10.733	19.446	36.047	58.080	99.671	229.930	577.000	1,054.801	1,504.527	2,623.817	6,073.814	25,710.806
2015	0.000	0.000	0.000	0.389	2.254	5.531	11.080	20.075	37.212	59.958	102.894	237.364	595.656	1,088.904	1,553.170	2,708.648	6,270.189	26,542.071
2017	0.000	0.000	0.000	2.227	1.659	4.330	9.864	18.367	34.891	58.022	101.747	232.532	591.092	1,067.960	1,526.678	2,634.807	6,046.843	25,536.992
2020	0.000	0.000	0.000	2.351	1.752	4.572	10.415	19.393	36.840	61.263	107.430	245.520	624.108	1,127.613	1,611.953	2,781.978	6,384.600	26,963.405
	Separate filers																	
2000	0.651	1.877	2.274	4.588	9.133	13.969	20.359	29.046	41.315	62.787	105.121	249.171	599.563	1,084.936	1,532.796	2,735.880	6,204.292	28,075.951
2005	0.739	2.129	2.579	5.203	10.358	15.842	23.089	32.941	46.856	71.206	119.217	282.584	679.963	1,230.423	1,738.341	3,102.756	7,036.274	31,840.879
2012	2.658	2.077	2.905	7.028	11.574	15.235	22.216	30.623	49.630	0.000	135.776	0.000	3,400.469	0.000	0.000	0.000	0.000	0.000
2015	2.744	2.144	2.999	7.255	11.948	15.727	22.935	31.613	51.235	0.000	140.166	0.000	3,510.411	0.000	0.000	0.000	0.000	0.000
2017	0.000	2.071	2.492	6.608	10.869	15.245	21.836	30.917	43.685	66.436	104.400	257.123	597.378	1,054.570	1,451.303	2,620.834	5,995.162	38,294.739
2020	0.000	2.187	2.631	6.977	11.476	16.096	23.055	32.644	46.125	70.147	110.231	271.485	630.746	1,113.475	1,532.368	2,767.226	6,330.032	40,433.759
	Single filers																	
2000	0.996	2.189	5.256	9.773	14.525	19.041	25.538	34.146	47.173	68.905	107.914	248.073	610.556	1,089.828	1,557.286	2,716.758	6,224.841	24,591.713
2005	1.130	2.483	5.961	11.083	16.473	21.594	28.963	38.724	53.499	78.145	122.385	281.339	692.431	1,235.972	1,766.115	3,081.070	7,059.579	27,889.412
2012	1.049	1.739	3.162	7.283	11.763	16.380	23.096	32.095	45.809	66.947	105.846	242.124	593.644	1,059.903	1,498.457	2,608.218	5,986.490	28,848.679
2015	1.083	1.796	3.264	7.518	12.144	16.910	23.842	33.133	47.290	69.111	109.268	249.952	612.837	1,094.172	1,546.904	2,692.545	6,180.042	29,781.396
2017	0.000	1.753	2.988	6.855	11.362	15.905	22.808	32.035	46.210	67.561	107.378	241.743	588.597	1,059.144	1,514.898	2,599.037	5,988.917	31,591.215
2020	0.000	1.851	3.155	7.238	11.997	16.794	24.082	33.824	48.792	71.334	113.376	255.246	621.474	1,118.304	1,599.516	2,744.211	6,323.439	33,355.798
	Heads of households																	
2000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
2005	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
2012	0.000	0.000	1.159	2.019	4.480	8.282	14.821	23.729	38.262	58.697	95.387	230.883	584.412	1,041.275	1,514.036	2,672.199	6,103.605	26,902.740
2015	0.000	0.000	1.196	2.084	4.624	8.550	15.300	24.496	39.499	60.595	98.471	238.348	603.307	1,074.940	1,562.987	2,758.595	6,300.943	27,772.542
2017	0.000	0.000	0.840	1.422	4.057	7.722	14.097	22.890	37.722	59.355	97.005	238.403	585.620	1,078.989	1,499.875	2,674.916	6,259.670	25,109.077
2020	0.000	0.000	0.887	1.501	4.284	8.154	14.884	24.169	39.829	62.671	102.424	251.720	618.331	1,139.258	1,583.653	2,824.328	6,609.314	26,511.589

Table 5: **Income shares**
This table reports the income shares for each of the 18 income buckets published by the IRS and for the four filer types (married filers filing jointly, married filers filing separately, single filers, heads of households). The income share is computed for each income bucket and each filer type as aggregate taxable income observed for a given bucket and filer type rescaled by total aggregate taxable income across buckets and filer types. Numbers are reported in percentage points.

Year	< 5	5 - 10	10 - 15	15 - 20	20 - 25	25 - 30	30 - 40	40 - 50	50 - 75	75 - 100	100 - 200	200 - 500	500 - 1,000	1,000 - 1,500	1,500 - 2,000	2,000 - 5,000	5,000 - 10,000	> 10,000
	Joint filers																	
2000	0.000	0.000	0.005	0.121	0.334	0.555	1.903	3.099	10.606	9.543	15.207	9.738	4.443	2.001	1.282	3.301	1.994	4.808
2005	0.000	0.000	0.005	0.121	0.334	0.555	1.903	3.099	10.606	9.543	15.207	9.738	4.443	2.001	1.282	3.301	1.994	4.808
2012	0.000	0.000	0.000	0.000	0.032	0.121	0.630	1.135	5.373	7.693	19.954	12.759	5.445	2.342	1.419	3.588	2.086	5.681
2015	0.000	0.000	0.000	0.000	0.032	0.121	0.630	1.135	5.373	7.693	19.954	12.759	5.445	2.342	1.419	3.588	2.086	5.681
2017	0.000	0.000	0.000	0.000	0.010	0.059	0.377	0.750	3.795	6.061	19.705	15.379	6.374	2.492	1.444	3.525	1.967	5.172
2020	0.000	0.000	0.000	0.000	0.010	0.059	0.377	0.750	3.795	6.061	19.705	15.379	6.374	2.492	1.444	3.525	1.967	5.172
	Separate filers																	
2000	0.000	0.006	0.059	0.302	0.492	0.579	1.113	0.910	1.339	0.467	0.515	0.302	0.158	0.080	0.057	0.154	0.111	0.382
2005	0.000	0.006	0.059	0.302	0.492	0.579	1.113	0.910	1.339	0.467	0.515	0.302	0.158	0.080	0.057	0.154	0.111	0.382
2012	0.000	0.000	0.006	0.018	0.036	0.044	0.142	0.156	0.461	0.000	0.386	0.000	0.857	0.000	0.000	0.000	0.000	0.000
2015	0.000	0.000	0.006	0.018	0.036	0.044	0.142	0.156	0.461	0.000	0.386	0.000	0.857	0.000	0.000	0.000	0.000	0.000
2017	0.000	0.000	0.003	0.013	0.027	0.044	0.119	0.151	0.345	0.237	0.353	0.165	0.080	0.041	0.034	0.103	0.074	0.407
2020	0.000	0.000	0.003	0.013	0.027	0.044	0.119	0.151	0.345	0.237	0.353	0.165	0.080	0.041	0.034	0.103	0.074	0.407
	Single filers																	
2000	0.051	0.287	0.779	1.243	1.551	1.674	3.354	2.706	4.003	1.822	2.360	1.446	0.666	0.312	0.192	0.544	0.314	0.728
2005	0.051	0.287	0.779	1.243	1.551	1.674	3.354	2.706	4.003	1.822	2.360	1.446	0.666	0.312	0.192	0.544	0.314	0.728
2012	0.007	0.060	0.323	0.675	0.920	1.095	2.465	2.468	4.794	2.791	3.540	1.713	0.724	0.334	0.200	0.576	0.334	1.028
2015	0.007	0.060	0.323	0.675	0.920	1.095	2.465	2.468	4.794	2.791	3.540	1.713	0.724	0.334	0.200	0.576	0.334	1.028
2017	0.000	0.050	0.216	0.504	0.776	0.955	2.270	2.392	5.055	3.149	4.338	2.104	0.808	0.345	0.200	0.522	0.289	1.039
2020	0.000	0.050	0.216	0.504	0.776	0.955	2.270	2.392	5.055	3.149	4.338	2.104	0.808	0.345	0.200	0.522	0.289	1.039
	Heads of households																	
2000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
2005	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
2012	0.000	0.000	0.001	0.031	0.145	0.256	0.681	0.651	1.323	0.711	0.810	0.382	0.137	0.068	0.036	0.103	0.065	0.189
2015	0.000	0.000	0.001	0.031	0.145	0.256	0.681	0.651	1.323	0.711	0.810	0.382	0.137	0.068	0.036	0.103	0.065	0.189
2017	0.000	0.000	0.000	0.011	0.085	0.184	0.561	0.601	1.272	0.796	1.082	0.477	0.181	0.082	0.040	0.107	0.052	0.150
2020	0.000	0.000	0.000	0.011	0.085	0.184	0.561	0.601	1.272	0.796	1.082	0.477	0.181	0.082	0.040	0.107	0.052	0.150

Table 6: **Marginal income tax rates**

This table reports the marginal income tax rates for each of the 18 income buckets published by the IRS and for the four filer types (married filers filing jointly, married filers filing separately, single filers, heads of households). The marginal income tax rate is assigned to each income bucket and each filer type based on the average income reported in Table 4. Numbers are reported in percentage points.

Year	< 5	5 - 10	10 - 15	15 - 20	20 - 25	25 - 30	30 - 40	40 - 50	50 - 75	75 - 100	100 - 200	200 - 500	500 - 1,000	1,000 - 1,500	1,500 - 2,000	2,000 - 5,000	5,000 - 10,000	> 10,000
	Joint filers																	
2000	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	28.0	28.0	36.0	39.6	39.6	39.6	39.6	39.6	39.6
2005	10.0	10.0	10.0	10.0	10.0	10.0	15.0	15.0	15.0	25.0	25.0	33.0	35.0	35.0	35.0	35.0	35.0	35.0
2012	10.0	10.0	10.0	10.0	10.0	10.0	10.0	15.0	15.0	15.0	25.0	33.0	35.0	35.0	35.0	35.0	35.0	35.0
2015	10.0	10.0	10.0	10.0	10.0	10.0	10.0	15.0	15.0	15.0	25.0	33.0	39.6	39.6	39.6	39.6	39.6	39.6
2017	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	15.0	15.0	25.0	28.0	39.6	39.6	39.6	39.6	39.6	39.6
2020	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	12.0	12.0	22.0	24.0	37.0	37.0	37.0	37.0	37.0	37.0
	Separate filers																	
2000	15.0	15.0	15.0	15.0	15.0	15.0	15.0	28.0	28.0	31.0	36.0	39.6	39.6	39.6	39.6	39.6	39.6	39.6
2005	10.0	10.0	10.0	10.0	15.0	15.0	15.0	25.0	25.0	28.0	33.0	35.0	35.0	35.0	35.0	35.0	35.0	35.0
2012	10.0	10.0	10.0	10.0	15.0	15.0	15.0	15.0	25.0	25.0	33.0	33.0	35.0	35.0	35.0	35.0	35.0	35.0
2015	10.0	10.0	10.0	10.0	15.0	15.0	15.0	15.0	25.0	25.0	33.0	33.0	39.6	39.6	39.6	39.6	39.6	39.6
2017	10.0	10.0	10.0	10.0	15.0	15.0	15.0	15.0	25.0	25.0	28.0	39.6	39.6	39.6	39.6	39.6	39.6	39.6
2020	10.0	10.0	10.0	10.0	12.0	12.0	12.0	12.0	22.0	22.0	24.0	35.0	37.0	37.0	37.0	37.0	37.0	37.0
	Single filers																	
2000	15.0	15.0	15.0	15.0	15.0	15.0	15.0	28.0	28.0	31.0	31.0	36.0	39.6	39.6	39.6	39.6	39.6	39.6
2005	10.0	10.0	10.0	15.0	15.0	15.0	15.0	25.0	25.0	28.0	28.0	33.0	35.0	35.0	35.0	35.0	35.0	35.0
2012	10.0	10.0	10.0	10.0	15.0	15.0	15.0	15.0	25.0	25.0	28.0	33.0	35.0	35.0	35.0	35.0	35.0	35.0
2015	10.0	10.0	10.0	10.0	15.0	15.0	15.0	15.0	25.0	25.0	28.0	33.0	39.6	39.6	39.6	39.6	39.6	39.6
2017	10.0	10.0	10.0	10.0	15.0	15.0	15.0	15.0	25.0	25.0	28.0	33.0	39.6	39.6	39.6	39.6	39.6	39.6
2020	10.0	10.0	10.0	10.0	12.0	12.0	12.0	12.0	22.0	22.0	24.0	35.0	37.0	37.0	37.0	37.0	37.0	37.0
	Heads of households																	
2000	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0
2005	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0
2012	10.0	10.0	10.0	10.0	10.0	10.0	15.0	15.0	15.0	25.0	25.0	33.0	35.0	35.0	35.0	35.0	35.0	35.0
2015	10.0	10.0	10.0	10.0	10.0	10.0	15.0	15.0	15.0	25.0	25.0	33.0	39.6	39.6	39.6	39.6	39.6	39.6
2017	10.0	10.0	10.0	10.0	10.0	10.0	15.0	15.0	15.0	25.0	25.0	33.0	39.6	39.6	39.6	39.6	39.6	39.6
2020	10.0	10.0	10.0	10.0	10.0	10.0	12.0	12.0	12.0	22.0	24.0	35.0	37.0	37.0	37.0	37.0	37.0	37.0

Table 7: **Capital gain tax rates**

This table reports the capital gain tax rates for each of the 18 income buckets published by the IRS and for the four filer types (married filers filing jointly, married filers filing separately, single filers, heads of households). The capital gain tax rate is assigned to each income bucket and each filer type based on the average income reported in Table 4. Numbers are reported in percentage points.

Year	< 5	5 - 10	10 - 15	15 - 20	20 - 25	25 - 30	30 - 40	40 - 50	50 - 75	75 - 100	100 - 200	200 - 500	500 - 1,000	1,000 - 1,500	1,500 - 2,000	2,000 - 5,000	5,000 - 10,000	> 10,000
	Joint filers																	
2000	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0
2005	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0
2012	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0
2015	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8
2017	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8
2020	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8
	Separate filers																	
2000	10.0	10.0	10.0	10.0	10.0	10.0	10.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0
2005	5.0	5.0	5.0	5.0	5.0	5.0	5.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0
2012	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0
2015	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8
2017	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8	23.8
2020	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8	23.8
	Single filers																	
2000	10.0	10.0	10.0	10.0	10.0	10.0	10.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0	20.0
2005	5.0	5.0	5.0	5.0	5.0	5.0	5.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0
2012	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0
2015	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8
2017	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8
2020	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8
	Heads of households																	
2000	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0
2005	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0
2012	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0	15.0
2015	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8
2017	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8
2020	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	15.0	15.0	15.0	23.8	23.8	23.8	23.8	23.8	23.8

Table 8: **Average tax rates**
This table reports the average tax rates on personal income, capital gains, qualified dividends, and dividends paid by REITs. Personal income and capital gain tax rates are computed as weighted average of the tax rates across buckets and filer types with weights given by the income shares. The average tax rate on qualified dividends is equal to the average tax rate on personal income in 2000 and the average tax rate on capital gains thereafter. The average tax rate on dividends paid by REITs equal the average personal income tax rate except for 2020, when it also includes the 20% deduction introduced by the 2017 Tax Cut and Jobs Act. Numbers are reported in percentage points.

Year	**Average tax rates**			
	Personal income	**Capital gain**	**Qualified dividends**	**REIT dividends**
2000	27.953	17.189	27.953	27.953
2005	25.322	12.189	12.189	25.322
2012	26.083	11.027	11.027	26.083
2015	27.242	13.246	13.246	27.242
2017	27.318	14.068	14.068	27.318
2020	24.435	14.068	14.068	19.548

Table 9: **Percentage change in average tax rates**
This table reports the percentage change in average tax rates on personal income, capital gains, qualified dividends, and dividends paid by REITs. The average tax rates are reported in Table 8. The percentage changes are computed between the last fiscal year before each reform was passed and the last fiscal year covered in the estimation of the DiD designs discussed in Section 3. Numbers are reported in percentage points.

Year	**Percentage change in average tax rates**			
	Personal income	**Capital gain**	**Qualified dividends**	**REIT dividends**
2000-2005	-9.884	-34.375	-83.002	-9.884
2012-2015	4.350	18.332	18.332	4.350
2017-2020	-11.152	0.000	0.000	-33.466

Table 10: **Percentage change in average keep rates**
This table reports the percentage change in average keep rates on personal income, capital gains, qualified dividends, and dividends paid by REITs. The average tax rates are reported in Table 8 and the keep rate is defined as one minus the average tax rate. The percentage changes are computed between the last fiscal year before each reform was passed and the last fiscal year covered in the estimation of the DiD designs discussed in Section 3. Numbers are reported in percentage points.

Year	**Percentage change in average keep rates**			
	Personal income	**Capital gain**	**Qualified dividends**	**REIT dividends**
2000-2005	3.586	5.863	19.787	3.586
2012-2015	-1.581	-2.525	-2.525	-1.581
2017-2020	3.889	0.000	0.000	10.156

Table 11: **Change in average tax rates**
This table reports the change in average tax rates on personal income, capital gains, qualified dividends, and dividends paid by REITs. The average tax rates are reported in Table 8. The changes are computed between the last fiscal year before each reform was passed and the last fiscal year covered in the estimation of the DiD designs discussed in Section 3. Numbers are reported in percentage points.

Year	**Change in average tax rates**			
	Personal income	**Capital gain**	**Qualified dividends**	**REIT dividends**
2000-2005	-2.631	-5.000	-15.764	-2.631
2012-2015	1.160	2.219	2.219	1.160
2017-2020	-2.883	0.000	0.000	-7.770

B Institutional details

In this section, I provide additional details on the financial institutions studied in the paper with a focus on the features that make each financial institution unique for fiscal purposes.

B.1 Retirement share classes

In addition to provide general access to capital markets by pooling and investing resources from investors, mutual funds further manage retirement savings of households that participate in defined-contribution and other retirement saving plans, including 401(k) plans, and Individual retirement accounts (IRAs). Because investments for retirement face specific administrative, fiscal, and transparency requirements, mutual funds have developed share classes that are specifically designed to being offered within qualified accounts. These share classes are labeled as "R" and range from R1 to R6 depending on the minimum required investment and the fee structure.[51]

Investments in qualified accounts provide unique tax benefits. Specifically, contributions to qualified accounts (e.g. 401(k) plans and IRAs) can be deducted from individual taxable income. Moreover, the taxation of dividends, interest income, and realized capital gains is deferred upon retirement, provided that the corresponding amount is reinvested within the account and not withdrawn. While, on the one hand, investments in qualified accounts benefit of deferred taxation, withdrawals made upon retirement are subject to income tax rates, higher than tax rates on dividends and capital gains.[52] Moreover, to limit the use of retirement contributions for tax avoidance, contributions to qualified accounts are subject to annual limits that apply separately for employers and employees, 401(k) plans and IRAs. For Roth IRAs, investors face not only annual maximum contributions but also a maximum income threshold. Households with modified adjusted gross income above an annual threshold cannot contribute to Roth IRAs.

[51] An individual mutual fund typically offers several share classes, each designed for access by different categories of investors. Share classes of the same mutual fund offer different shareholder rights and obligation although, in summary, the two most notable distinctions concern the minimum investment required for access and the fee structure charged to investors. The most well-known and studied distinction is between retail and institutional share classes. Previous papers have studied differences in performance and flow-return sensitivity of retail and institutional investors, including Chevalier and Ellison (1997); Gruber (1996); Sirri and Tufano (1998); Zheng (1999); Keswani and Stolin (2008); Basak and Pavlova (2013).

[52] Withdrawals made before the age of 59 years and 6 months are considered early withdrawals by the Internal Revenue Service and subject to an additional 10% penalty on top of the application of income tax rates.

B.2 Variable annuities

Variable annuities are savings vehicles sold by insurance companies as a form of investment for retirement. As discussed in Koijen and Yogo (2022), a variable annuity is a mutual fund coupled with a longevity insurance and, potentially, a minimum guaranteed return.[53]

In addition to these well-known and distinctive features, variable annuities also offer tax benefits similar to those offered by investments in qualified accounts, as summarized by Brown and Poterba (2006). The most important similarity is that the taxation of dividends and realized capital gains is deferred upon retirement, provided that the corresponding amount is reinvested and not withdrawn. It follows that, like retirement accounts, variable annuities benefit of the tax-free compounding earned on the reinvestment of dividends and realized capital gains. Importantly, this tax benefit is conferred even when variable annuities are held outside qualified accounts. In addition, like qualified accounts, withdrawals upon retirement are taxed at income tax rates.

At the same time, variable annuities have two important features that distinguish them from qualified accounts. On the one hand, contributions to variable annuities are not exempt from income taxes, unless variable annuities are held within qualified accounts. Moreover, because contributions to variable annuities are not tax-exempt, differently from qualified accounts, there is no dollar limit on the amount that individual households can invest in variable annuities every year nor a maximum level of annual income above which households cannot contribute, like for Roth IRAs.

B.3 Real estate investment trusts

Real estate investment trusts (REITs) are publicly traded firms that invest in real estate assets and that, differently from standard "C" corporations, can avoid the double taxation of dividends at both corporate and individual level. Specifically, REITs can deduct distributions made to their shareholders from their corporate pre-tax income provided that they meet conditions listed in the internal revenue code. In particular, REITs can benefit of the corporate tax deduction if they distribute to shareholders at least 90% of their annual taxable income, if at least 75% of their assets is invested in real estate and cash, and if at least 75% of their gross income is obtained from real estate and related sources. REITs are also required to have at least 100 shareholders within the first taxable year and no more than 50% of shares outstanding can be held by five or fewer investors.

[53] The variable annuity provides longevity insurance in that, upon retirement, the investor is entitled to withdraw a fixed amount per period. This fixed amount is proportional to the account balance upon retirement and is distributed to the investor even after the account balance is depleted.

While, on the one hand, REITs can avoid the double taxation at corporate and individual level, on the other hand, the majority of distributions made by REITs are subject to income rather than capital gain tax rates. This includes dividends paid out by REITs, which were excluded from the definition of qualified dividends introduced with the 2003 Jobs and Growth Tax Relief Reconciliation Act and therefore taxed like ordinary dividends at income rather than capital gain tax rates. This exception is used in a number of empirical papers including, among others, Amromin et al. (2008) and Edgerton (2010).

B.4 Exchange-traded funds

ETFs generate significantly lower tax expenses on capital gains compared to their closest substitutes, mutual funds. The tax efficiency of ETFs rely on two key institutional details.

On the one hand, ETFs are publicly traded on a secondary market. It follows that, differently from mutual funds, ETFs are not forced to sell assets in their portfolio and realize capital gains when an investor wishes to redeem her positions. On the contrary, the investor will simply sell the ETF shares in the secondary market and the price of the ETF share will adjust until the market clears. This is not the case for the generic mutual fund, that is not traded on the secondary market and that is forced to sell assets and realize capital gains to liquidate the positions of redeeming investors in excess of cash reserves and contemporaneous inflows. Importantly, every investor in the mutual fund is required to pay taxes on the realized capital gains proportionally to the share of assets under management held, including non-redeeming investors.

In addition to being publicly traded, ETFs enjoy a second key tax benefit relative to mutual funds, which relies on the so called "heartbeat trades" carried out by authorized participants and on the tax exemption for in-kind redemptions established in 1969 and confirmed in Section 825(b)(6) of the 1986 Internal Revenue Code. Specifically, in the event of extreme selling pressure that decreases the price of an ETF below the no-arbitrage price implied by the underlying portfolio of securities, one or more authorized participants (typically investment banks and brokerage firms) purchase ETF shares from the secondary market and exchange them "in-kind" with a representative basket of securities held by the ETF. The increase in demand on the secondary market contributes to increase the ETF share price back to the no-arbitrage value while the in-kind redemption does not constitute a tax event for the ETF or its investors.

Once the authorized participant obtains the underlying securities, it sells them in the secondary market and pockets the difference between the lower ETF price and the higher value of the underlying basket of securities. The superior tax efficiency of ETFs is quantitatively meaningful. According to Moussawi et al. (2022), around 65% of U.S. equity mutual funds but only 6% of U.S. Equity ETFs distributed capital gains to their investors in 2017.

C History of fiscal reforms on financial activities

Table 12 summarizes the history of fiscal reforms that affected financial activities and financial institutions since 1970. While reforms passed before 1999 are not covered by the datasets discussed in Appendix A, in this section I provide qualitative evidence on the effects of earlier reforms using quarterly aggregate data from the Fed Financial Accounts.

Table 12: **History of fiscal reforms on financial activities**
This table provides the history of fiscal reforms on financial activities since 1970. The first column provides the name of the reform, the second column reports the date on which the reform was approved, and the third column summarizes the key points of the reform with a focus on its effect on financial activities and financial institutions.

Reform	Year	Summary
Employee Retirement Income Secruity Act (ERISA)	September 2, 1974	- Introduced disclosure requirements, standards of conduct and sources of liability for pension plans.
Revenue Act	November 6, 1978	- Added section 401(k) to the Internal Revenue Code that establish the commonly known 401(k) plans.
Economic Recovery Tax Act (ERTA)	August 13, 1981	- Allowed all working tax payers to establish IRAs. - Decreased highest individual marginal tax rate from 70% to 50%. - Decreased estate, corporate and capital gain tax rates.
Tax Reform Act (TRA)	October 22, 1986	- Decreased highest individual marginal tax rate from 50% to 33%. - Deductions on individual interest expenses limited to mortgages (before the reform, all individual interest expenses were deductible). - Decreased deductions for contributions to IRAs.
Technical and Miscellaneous Revenue Act	November 10, 1988	- Limited the use of life insurance policies as tax shelters, i.e. as investment vehicles with tax-free distributions. - Introduced the "7-pay test" which caps the amount that can be contributed to life insurance as a percentage of the death benefit.
Omnibus Budget Reconciliation Act (OBRA)	August 10, 1993	- Increased the highest marginal income tax rate from 31% to 39.6% and created a new 36% income bracket. - Increased the highest marginal tax rate on capital gains from 28% to 29.19%.

I start from the 1978 Revenue Act which introduced section 401(k) in the Internal Revenue Code and allowed employees to save for retirement via 401(k) plans. Because most of the assets in 401(k) plans are managed by mutual funds, we would expect that the reform was followed by an increase in the market share of mutual funds relative to other financial institutions.

Panel A of Figure 23 plots the share of assets managed by mutual funds (including money market funds) relative to total assets managed by mutual funds, pension funds, and life insurance companies. The vertical line highlights the year-quarter in which the reform was formally signed into law. To avoid double counting, I subtract from the total assets of each institutional sector the dollar amount of assets that the institutional sector

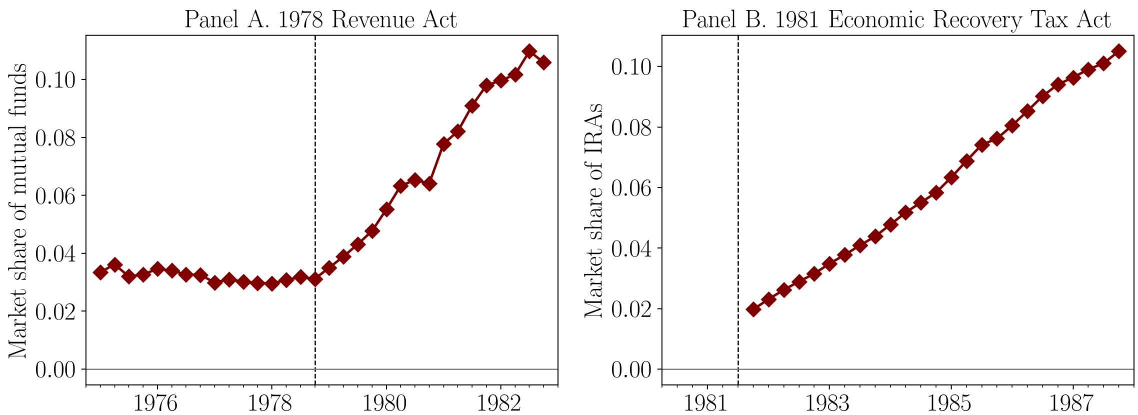
Table 4: Continued

Reform	Year	Summary
Taxpayer Relief Act (TRA)	August 5, 1997	- Created Roth IRAs to which individuals contribute using post-tax money but withdrawals are then tax-free.
Economic Growth and Tax Relief Reconciliation Act (EGTRRA)	June 7, 2001	- Decreased personal income tax rates. - Increased contribution limits to 401(k) plans and IRAs. - Allowed asset transfers across different accounts (e.g. from 401(k) plans to IRAs) without realizing capital gains.
Jobs and Growth Tax Relief Reconciliation Act (JGTRRA)	May 23, 2003	- Decreased tax rates on dividends and capital gains. - Increased the threshold for application of the Alternative Minimum Tax (AMT) schedule.
Pension Protection Act (PPA)	August 17, 2006	- Introduced a penalty for underfunded pension plans. - Allowed transfer of IRAs to non-spouse beneficiaries without realizing capital gains. - Made permanent the tax deduction for contributions to 401(k) and IRAs. - Introduced limited liability protections for plan sponsors if Target Date Funds (TDFs) and other qualified default investment alternatives (QDIAs) are used as default options in 401(k) plans.
American Taxpayer Relief Act (ATRA)	January 2, 2013	- Increased the highest marginal tax rates on personal income and capital gains to 39.6% and 20% respectively.
Tax Cuts and Jobs Act (TCJA)	December 22, 2017	- Decreased personal and corporate income tax rates. - Decreased the deductible limit for interest on mortgages and real estate loans. - Introduced a 20% personal tax deduction on distributions made by REITs and on Qualified Business Income (QBI).

invests through any other institutional sector. For example, I subtract from the total assets observed for life insurance companies the assets that life insurance companies hold in mutual funds. The market share of the mutual fund industry increased rapidly after the 1978 Revenue Act was passed, from 4% in 1978 to 10% just three years later.

I then look at the 1981 Economic Recovery Tax Act, which allowed for the first time all workers to invest for retirement in Individual Retirement Accounts (IRAs). Figure **??** reports the share of assets managed by IRAs relative to total assets managed by IRAs, defined contribution plans, and defined benefit plans. As before, the vertical line highlights the year-quarter in which the reform was formally signed into law. The market share of IRAs increased rapidly after the 1981 Economic Recovery Tax Act was passed, increasing from 2% at the end of 1981 to 8% in 1985 and 12% by 1989.

Figure 23: **Market share of mutual funds and IRAs**
Panel A provides the market share of mutual funds around the 1978 Revenue Act. The market share of mutual funds is computed as assets managed by mutual funds relative to total assets managed by mutual funds, pension funds, and life insurance companies. Panel B provides the market share of IRAs around the 1981 Economic Recovery Tax Act. The market share of IRAs is computed as assets managed by IRAs relative to total assets managed by IRAs, defined contribution, and defined benefit plans. Data are from the Fed Financial Accounts.



D Additional results on financial institutions

D.1 Retirement share classes

In this section, I provide additional results on the effect of an increase in contribution limits on log flows to defined contribution plans.

The baseline DiD design in equation (1) controls for style fixed effects, which allow to restrict the comparison across share classes that hold assets with similar characteristics. One potential concern is that the baseline specification does not absorb unobservables at the share class level. These unobservables could include, for instance, the skills of portfolio managers to the extent that they are constant over the time window used in the estimation. To control for unobservables at the share class level, I modify the baseline specification in equation (1) and estimate

$$\log\left(1 + f_{it}\right) = \alpha_i + \alpha_t + \gamma_t \mathbb{I}\left(i \in Treatment\right) + \beta' X_{it} + \varepsilon_{it}, \tag{9}$$

The only difference compared to equation (1) is that I now include share class α_i and year-quarter α_t fixed effects. To ensure that share class fixed effects can be properly estimated, I focus on a balanced panel that includes only share classes alive throughout the estimation window (1999Q2-2005Q2).

Results are reported in Figure 24 which shows that the estimates are quantitatively close to the ones reported in Figure 4. The cumulative effect of the change in contribution limits passed by the 2001 Economic Growth and Tax Relief Reconciliation Act on log flows to retirement share classes is 25.35 percentage points with share class fixed effects compared to 30.05 percentage points with style fixed effects.

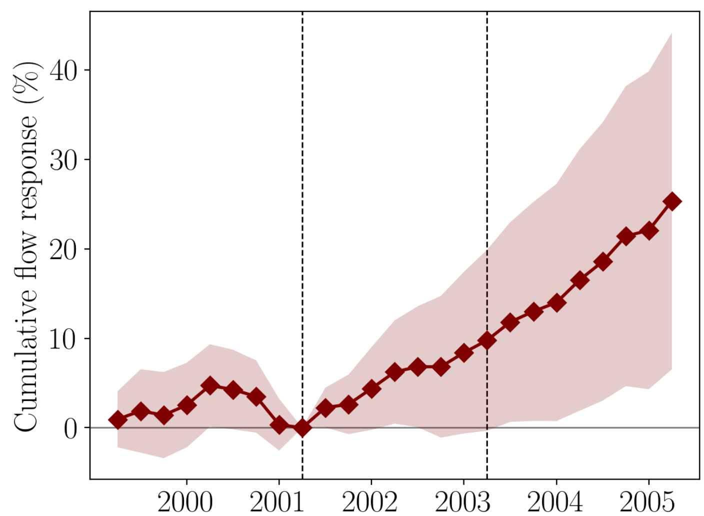
One additional concern could be that style fixed effects are still too broad and that, by using style fixed effects, I am still comparing share classes that are sufficiently different, for instance, in terms of assets held in their portfolio. To address this concern, I use the fact that, between 1999Q1 and 2005Q2, around 85.47% of the retirement share classes offered on the market belonged to a mutual fund that also offered non-retirement share classes. I use this institutional detail to strengthen the definition of treatment and control groups.

Specifically, I restrict the sample to retirement and non-retirement share classes offered by the same mutual funds. I then estimate the following variant of the baseline DiD design

$$\log\left(1 + f_{it}\right) = \alpha_{m(i)} + \alpha_t + \gamma_t \mathbb{I}\left(i \in Treatment\right) + \beta' X_{it} + \varepsilon_{it}, \tag{10}$$

where $\alpha_{m(i)}$ denotes mutual fund fixed effects. Controlling for mutual fund fixed effects

Figure 24: **Share class fixed effects**
This figure provides the cumulative effect of the increase in contribution limits passed by the 2001 Economic Growth and Tax Relief Reconciliation Act on log flows to retirement share classes, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\gamma}_u$, where γ_t is defined in equation (9). Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.

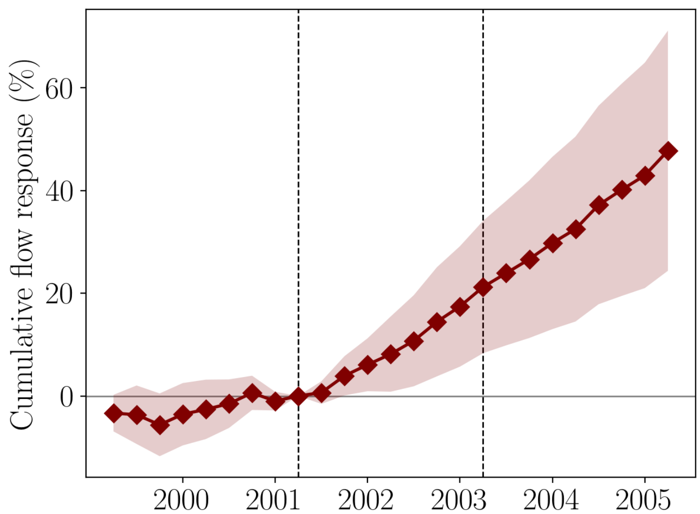


implies that I am comparing retirement and non-retirement share classes offered by the same mutual fund, and therefore identical in terms of portfolio allocation and portfolio manager. I report results in Figure (25). I find that the cumulative effect of the change in contribution limits on log flows to retirement share classes increases to 47.76 percentage points compared to a baseline estimate of 30.05 percentage points.

One reason behind this increase could be that, by comparing identical retirement and non-retirement share classes, I am capturing not only the increase in log flows to retirement share classes but also a decrease in log flows to non-retirement share classes which would violate the Stable Unit Treatment Value Assumption (SUTVA). In other words, households may have more incentive to sell non-retirement share classes and buy retirement share classes when the two are nearly identical. Because this would bias my estimates upwards, I prefer to use the specification discussed in the main body of the paper as baseline estimate.

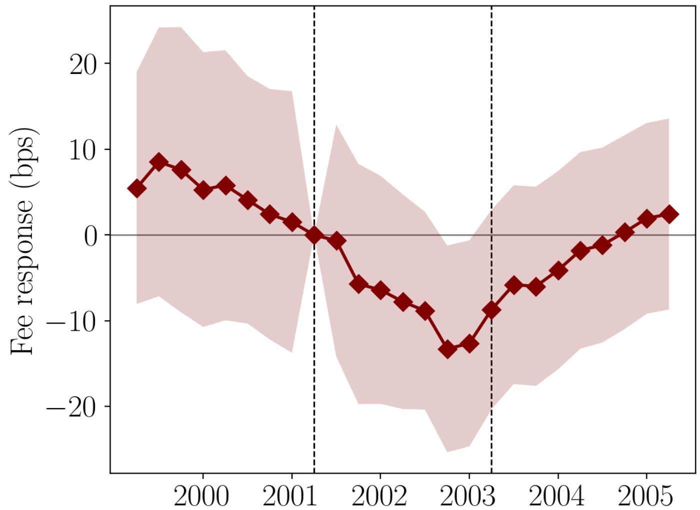
Finally, I report estimates of the baseline equation (1) with the expense ratio charged by each share class in each year-quarter as the outcome variable. I find no detectable effect of the reform on the expense ratio charged by retirement share classes. This is consistent with the increase in market entry (and consequent increase in competition which would predict a decrease in the expense ratio) offsetting the increase in demand for retirement

Figure 25: **Retirement and non-retirement mutual funds**
This figure provides the cumulative effect of the increase in contribution limits passed by the 2001 Economic Growth and Tax Relief Reconciliation Act on log flows to retirement share classes, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\gamma}_u$, where γ_t is defined in equation (10). Standard errors are clustered at mutual fund times year-quarter level and 95% confidence intervals are provided.



share classes (which would predict, if anything, an increase in the expense ratio).

Figure 26: **Effect on the expense ratio charged by retirement share classes**
This figure provides the effect of changes in contribution limits on the expense ratio charged by retirement share classes, summarized for each year-quarter t by the estimate $\widehat{\gamma}_t$, where γ_t is defined in equation (1). Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.

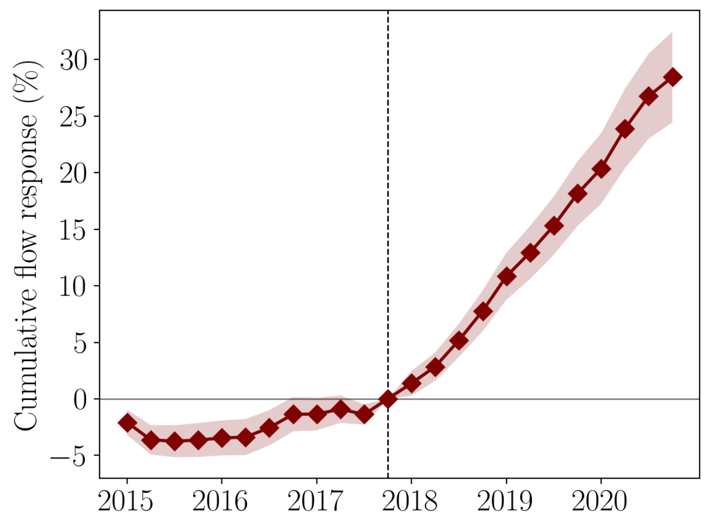


D.2 Variable annuities

In this section, I provide additional results on the effect of a decrease in personal income tax rates on log flows to variable annuities.

I first repeat the estimation of equation (1) in a balanced panel, which I construct by restricting the sample to retirement share classes and variable annuities that appear in the dataset throughout the estimation window (2015Q1-2020Q4). I report the results in Figure 27. The cumulative effect of the decrease in personal income tax rates passed by the 2017 Tax Cut and Jobs Act increases from 19.76 to 28.45 percentage points.

Figure 27: **Effect in a balanced panel**
This figure reports the cumulative effect of the decrease in personal income tax rates passed by the 2017 Tax Cut and Jobs Act on log flows to variable annuities. The dataset is balanced and restricted to variable annuities and retirement share classes that appear in the dataset throughout the estimation window (2015Q1-2020Q4). Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



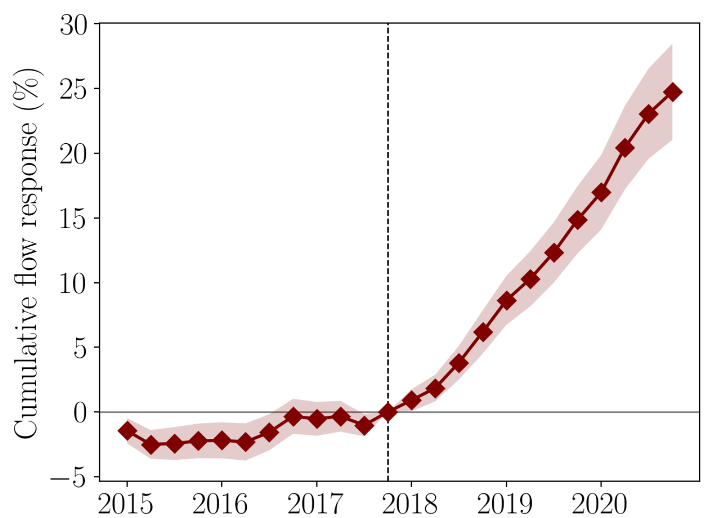
The baseline DiD design in equation (1) controls for style fixed effects, which allows to use variation between retirement share classes and variable annuities that hold assets with similar characteristics. One potential concern is that the baseline specification does not absorb unobservables at the share class and variable annuity level. These unobservables could include, for instance, minimum returns guaranteed by some variable annuities. To control for these potential unobservables, I modify the baseline specification in equation (1) and estimate

$$\log\left(1+f_{it}\right)=\alpha_i+\alpha_t+\gamma_t\mathbb{I}\left(i\in Treatment\right)+\beta' X_{it}+\varepsilon_{it}, \tag{11}$$

The only difference compared to equation (1) is that I now include unit α_i and year-quarter α_t fixed effects. To ensure that unit fixed effects can be properly estimated, I focus on a balanced panel that includes only retirement share classes and variable annuities alive throughout the estimation window (2015Q1-2020Q4).

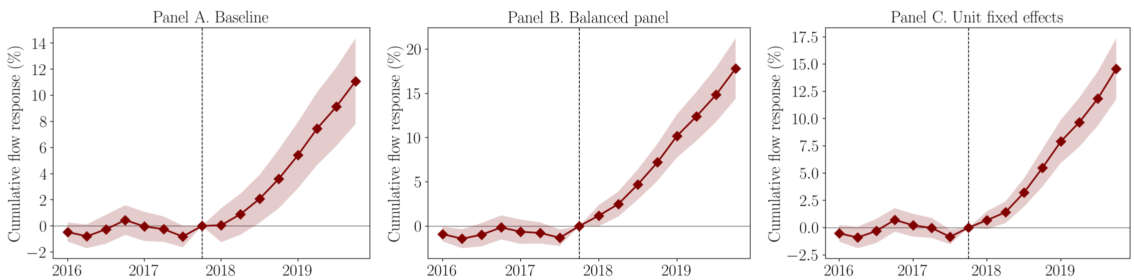
Results are reported in Figure 28. The cumulative effect of the decrease in personal income tax rates passed by the 2017 Tax Cut and Jobs Act on log flows to variable annuities is 24.75 percentage points compared to 19.76 percentage points in the baseline specification.

Figure 28: **Unit fixed effects**
This figure provides the cumulative effect of the decrease in personal income tax rates passed by the 2017 Tax Cut and Jobs Act on log flows to variable annuities, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\gamma}_u$, where γ_t is defined in equation (11). Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



Because Covid-19 may have led to an increase in capital flows to variable annuities for reasons unrelated to fiscal policy, I repeat all estimates in a two-year symmetric window around the reform (2016Q1-2019Q4) that excludes 2020 and the Covid-19 crisis. I report results in Figure 29. The effect of the decrease in personal income tax rates is 11.08 percentage points when estimating the baseline specification in equation (1), 17.80 percentage points when using a balanced panel, and 14.57 percentage points when including unit fixed effects.

Figure 29: **Effect on log flows to variable annuities excluding Covid**
This figure reports the cumulative effect of the decrease in personal income tax rates passed by the 2017 Tax Cut and Jobs Act on log flows to variable annuities in a two-year symmetric window around the reform (2016Q1-2019Q4) that excludes 2020 and the Covid-19 crisis. Panel A provides the baseline estimates. Panel B provides the estimates in a balanced panel that is restricted to variable annuities and retirement share classes that appear in the dataset throughout the estimation window. Panel C repeats the estimation in Panel B but controls for unit and year-quarter fixed effects rather than style times year-quarter fixed effects. Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.

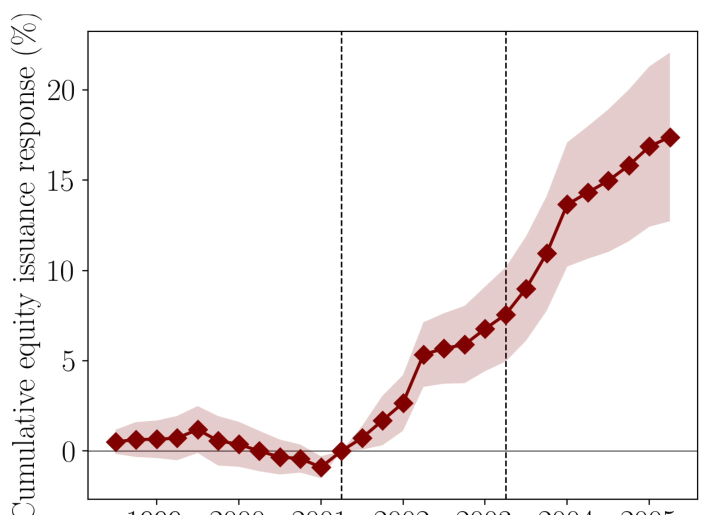


D.3 Real estate trusts

In this section, I provide additional results about the effect of a decrease in dividend tax rates on equity issuance by REITs.

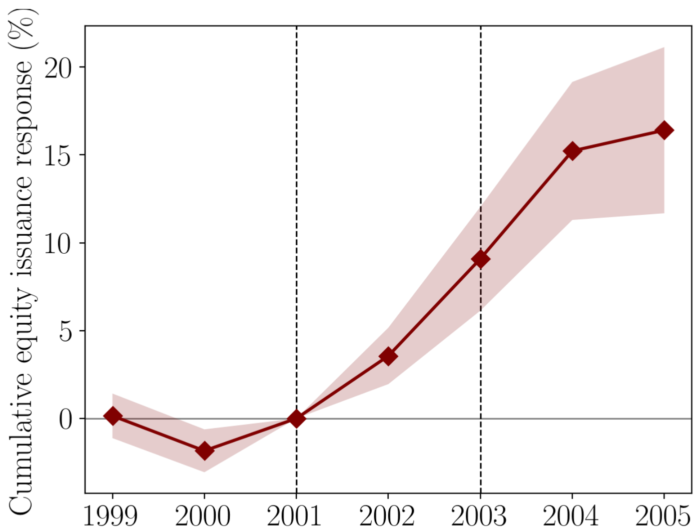
I first extend the baseline DiD design in equation (1) to include security α_i and year-quarter fixed effects instead of industry times year-quarter fixed effects. To ensure that security fixed effects can be properly estimated, I focus on a balanced panel that includes only securities that appear in the dataset throughout the estimation window (1998Q3-2005Q2). The cumulative effect of the decrease in personal income tax rates on equity issuance by REITs is quantitatively similar and increases from 16.60 to 17.39 percentage points.

Figure 30: **Security fixed effects**
This figure provides the cumulative effect of the decrease in personal income tax rates passed by the 2001 Economic Growth and Tax Relief Reconciliation Act on log equity issuance by REITs. The dataset is balanced and restricted to securities that appear in the sample throughout the estimation window (1998Q3-2005Q2). I include security α_i and year-quarter fixed effects α_t. Standard errors are clustered at industry times year-quarter level and 95% confidence intervals are provided.



Second, I check that the results are not driven by the choice of the equity issuance measure from Boudoukh et al. (2007) and Belo et al. (2019). To this end, I use as outcome variable the annual net equity issuance measure constructed from the Compustat Fundamentals North America Annual dataset as in Jensen et al. (2022) and report the estimates in Figure 31. The cumulative effect at the end of the estimation window is 16.41 percentage points, almost identical to the estimate of 16.59 percentage points obtained using the net equity issuance measure from Boudoukh et al. (2007) and Belo et al. (2019)

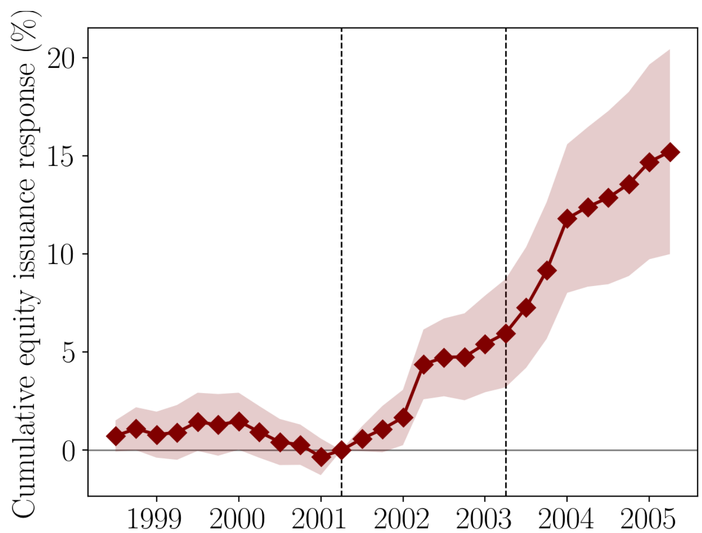
Figure 31: **Alternative measure of equity issuance**
This figure reports the cumulative effect of of the decrease in personal income tax rates passed by the 2001 Economic Growth and Tax Relief Reconciliation Act on log equity issuance by REITs. The outcome variable is log net equity issuance constructed from Compustat as in Jensen et al. (2022). Standard errors are clustered at industry times year level and 95% confidence intervals are provided.



Third, an additional potential concern is that the increase in equity issuance by REITs could have been driven by the real estate bubble that started around the beginning of the century and that culminated with the Great Financial Crisis. To show that this is not the case, I exclude from the dataset any REIT for which I observe a property type (*ptype*) equal to 6, 7, or 8. These are REITs that invest in mortgages, mortgage-backed securities, and residential real estate, respectively. I then repeat the estimation using the net equity issuance measure from Boudoukh et al. (2007) and Belo et al. (2019) as the outcome variable. The cumulative effect decreases only slightly from 16.60 percentage points in the baseline regression to 15.21 percentage points, suggesting that the real estate bubble was not a driver behind the increase in equity issuance by REITs.

A second reform that changed the tax rate on dividends paid by REITs was the 2017 Tax Cut and Jobs Act. However, this reform was accompanied by several other provisions and, in particular, by the largest decrease in corporate income tax rates ever passed in the U.S. history. This change likely increased equity issuance by corporations relative to REITs for two reasons: (i) it may have created an incentive for REITs to accept the payment of the corporate tax on non-distributed income and to increase the use of retained earnings to finance their investments; (ii) it may have redirected investors demand away from REITs

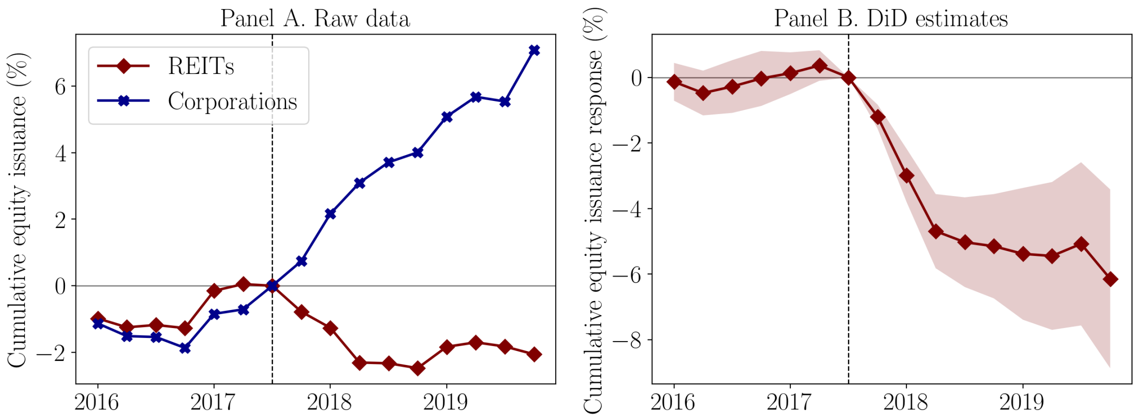
Figure 32: **Effect excluding REITs that invest in mortgages, MBS, and residential real estate**
This figure reports the cumulative effect of of the decrease in personal income tax rates passed by the 2001 Economic Growth and Tax Relief Reconciliation Act on log equity issuance by REITs. I drop from the dataset REITs that invest in mortgages, MBS, and residential real estate. Standard errors are clustered at industry times year level and 95% confidence intervals are provided.



and towards corporations subject to the corporate tax rate on all of their taxable income.

Figure 33 reports estimates of the baseline regression in (1) around the 2017 Tax Cut and Jobs Act. This reform had a slight negative effect on equity issuance by REITs and, at the same time, a strong positive effect on equity issuance by corporations. Estimates suggest that equity issuance by corporations increased by 6.14 percentage points relative to equity issuance by REITs. This is consistent with the response to the decrease in corporate income tax rates more than offsetting the response to the decrease in personal income tax rates.

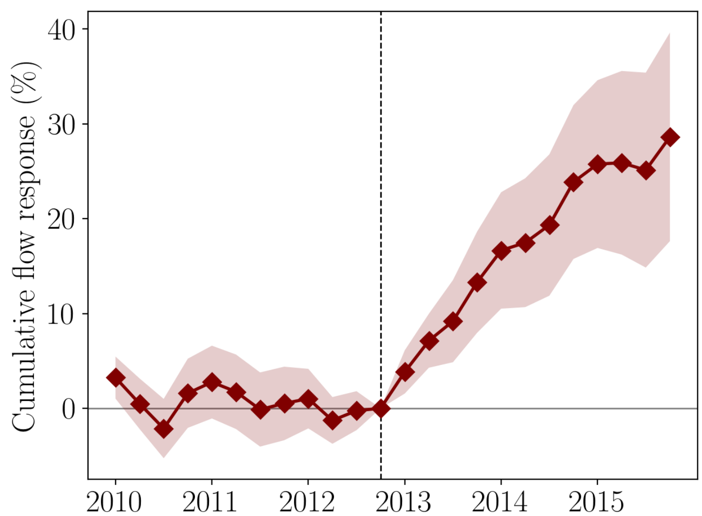
Figure 33: **Effect of the 2017 Tax Cut and Jobs Act on equity issuance by REITs**
Panel A reports the cumulative average log equity issuance by REITs and corporations in raw data around the passage of the 2017 Tax Cut and Jobs Act. Panel B provides the cumulative effect on log equity issuance by REITs, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\gamma}_u$, where γ_t is defined in equation (1). In Panel B, standard errors are clustered at industry times year-quarter level and 95% confidence intervals are provided.



D.4 ETFs

To ensure that results are not driven by composition effects or by unobservables at share class level, I first repeat the estimation of the baseline equation in (1) after restricting the dataset to ETFs and mutual fund share classes that appear in the dataset throughout the estimation window (2010Q1-2015Q4). Results are reported in Figure 34. The cumulative effect is 28.63 percentage points, virtually identical to the cumulative effect of 28.11 percentage points obtained in the baseline specification.

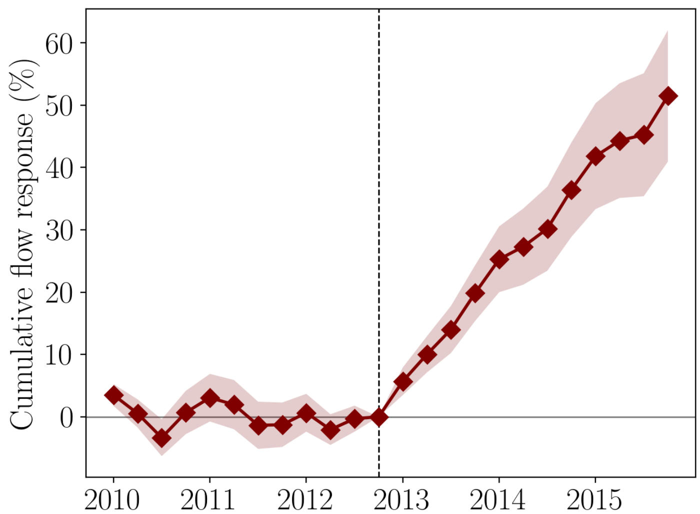
Figure 34: **Effect in a balanced panel**
This figure reports the cumulative effect of the increase in capital gain tax rates passed by the 2012 American Taxpayer Relief Act on log flows to ETFs. The dataset is balanced and restricted to ETFs and mutual fund share classes that appear in the dataset throughout the estimation window (2010Q1-2015Q4). Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



Second, I modify the baseline specification in (1) to include unit α_i and year-quarter α_t fixed effects instead of style times year-quarter fixed effects. The cumulative effect increases substantially to 51.50 percentage points suggesting that, if anything, the baseline estimate of 28.11 percentage points should be considered as a conservative lower bound of the true effect.

Third, if the increase in capital flows to ETFs was indeed driven by the increase in capital gain taxes, we would expect the effect on flows and therefore on fees to be stronger in styles where stocks have lower dividend yield on average (i.e., a large part of the returns come in the form of capital gains rather than dividend distributions). The reason is that, in these styles, redemptions faced by mutual funds can trigger the realization of larger capital

Figure 35: **Share class fixed effects**
This figure provides the cumulative effect of the increase in capital gain tax rates passed by the 2012 American Taxpayer Relief Act on log flows to ETFs. The dataset is balanced and restricted to ETFs and mutual fund share classes that appear in the dataset throughout the estimation window (2010Q1-2015Q4). Moreover, I include security α_i and year-quarter fixed effects α_t. Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



gains and therefore larger tax expenses for investors. To test this hypothesis, I estimate the following extension of equation (1) separately for ETFs and mutual funds in each style s

$$y_{it} = \alpha_{s(i)t} + \delta_{s(i)}\mathbb{I}\left(i \in Treatment\right) + \gamma_{s(i)}\mathbb{I}\left(i \in Treatment\right)\mathbb{I}\left(t > \text{2012Q4}\right) + \beta'_{s(i)}X_{it} + \varepsilon_{it}, \tag{12}$$

where $\mathbb{I}\left(t > \text{2012Q4}\right)$ denotes an indicator equal to one in all year-quarters after 2012Q4, when the reform was passed. Equation (12) is still a DiD design in which $\gamma_{s(i)}$ identifies the average effect of the reform on y_{it} across all year-quarters in the post-reform period.

I report estimates of $\gamma_{s(i)}$ from equation (12) in Table 13 for cap-based styles (Large Cap, Mid Cap, Small Cap) and income-based styles (Income, Growth). The advantage behind these two groups is that styles in each group can be clearly ranked in terms of the average dividend yield of stocks in which mutual funds and ETFs invest. The third row reports estimates $\widehat{\gamma}_{s(i)}$ when using the log flow, $\log\left(1 + f_{it}\right)$, as the outcome variable in equation (12). The fourth row provides estimates $\widehat{\gamma}_{s(i)}$ when using the expense ratio as the outcome variable in equation (12).

To visualize the ranking between styles in terms of dividend yield, the second row provides the average dividend yield of each style across stocks. This is computed by first taking a value-weighted average of dividend yields across stocks in the portfolio of each mutual fund and ETF, with weight given by the portfolio share on each stock. The dividend yield is then average across all mutual funds and ETFs in each style.

Results in Table 13 show that the increase in capital flows was stronger for small cap and mid cap ETFs and lower for large cap ETFs. This ranking mirrors the ranking of these three styles in terms of dividend yield, which is on average lower for small cap and mid cap stocks, and higher for large cap stocks. Similarly, the effect was stronger for growth ETFs compared to income ETFs. These results confirm that the increase in capital flows to ETFs after 2012Q4 was indeed a response to the increase in capital gain taxes. Additionally, the effect on fees was higher in styles that experienced the largest effect on capital flows. This confirms that the increase in fees charged by ETFs after 2012Q4 was indeed a response to the increase in demand.

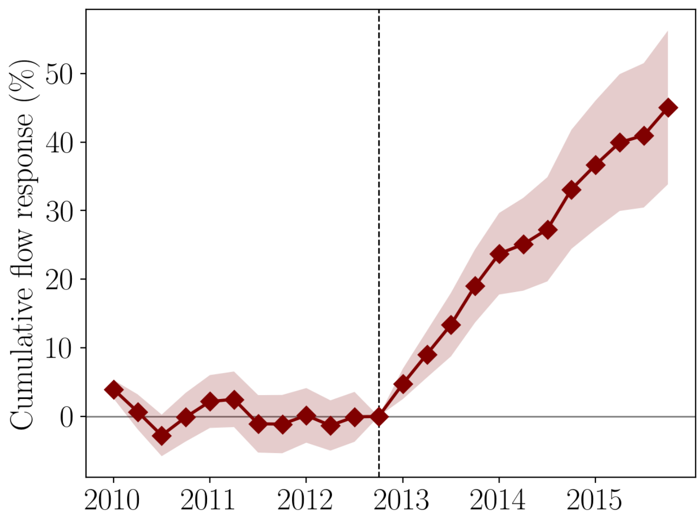
Table 13: **Heterogeneity across styles**
This table provides the effect of the increase in capital gain tax rates passed by the 2012 American Taxpayer Relief Act on log flows to ETFs and fees charged by ETFs in different styles. The third row provides estimates $\widehat{\gamma}_{s(i)}$ from equation (12) when the outcome variable is represented by $\log(1 + f_{it})$. The fourth row provides estimates $\widehat{\gamma}_{s(i)}$ from equation (12) when the outcome variable is represented by the expense ratio charged by mutual funds and ETFs. Standard errors are clustered at year-quarter level.

	Large Cap	Mid Cap	Small Cap	Income	Growth
Average d/p (%)	2.071	1.176	1.196	2.457	1.403
Flows (%)	-0.526	3.961***	3.655**	-1.065	2.561***
	(1.970)	(1.115)	(1.452)	(1.530)	(0.925)
Fees (bps)	0.2432	7.619***	8.018***	4.130*	9.255***
	(2.888)	(0.965)	(1.260)	(2.222)	(1.276)

Standard-errors clustered at year-quarter level
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

If the increase in capital flows to ETFs after 2012Q4 was indeed driven by the increase in capital gain taxes, we would also expect the effect to be stronger for passive ETFs. The reason is that passive ETFs are more likely to be held by investors with longer investment horizon. At the same time, compounding the lower tax expenses generated by ETFs over multiple years can have substantial implications for the long-term after-tax performance. To test this hypothesis, I restrict the dataset to passive ETFs and mutual funds and I repeat estimation of the baseline specification in equation (1).[54] Consistently with the increase in capital flows to ETFs being driven by the increase in capital gain taxes, I find that the cumulative effect increases from 28.11 percentage points in the baseline specification to 45.03 percentage points for passive ETFs.

Figure 36: **Effect on log flows to index ETFs**
This figure provides the cumulative effect of the increase in capital gain tax rates passed by the 2012 American Taxpayer Relief Act on log flows to index ETFs. Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



As for retirement share classes, I also exploit the fact that, between 2010Q1 and 2015Q4, 46 mutual funds were offering at the same time an ETF share class and a non-ETF share class. Interestingly, all these mutual funds belonged to one single management company: Vanguard. The reason is that, in 2001, Vanguard obtained a patent which gave it exclusive rights to offer ETF share classes as part of mutual funds offering also non-ETF share classes. Importantly, when a Vanguard mutual fund offers an ETF share class together with non-ETF share classes, part of the tax efficiency of the ETF share class spills over to non-ETF share classes. The reason is that the ETF share class can engage in in-kind redemptions with both authorized participants and with non-ETF share classes. The ETF share class can then receive in-kind securities that non-ETF share classes purchased at low price and, again, exchange them in-kind with the authorized participant. This process removes from the portfolio of non-ETF share classes securities that, if sold, would generate high capital

[54] I identify passive ETFs and mutual funds as those with index_fund_flag in CRSP equal to "D". These are funds that CRSP defines as "Pure Index Funds" with the "objective to match the total investment performance of a publicly recognized securities market index. The fund will hold virtually all securities in the noted index with weightings equal to those in the index".

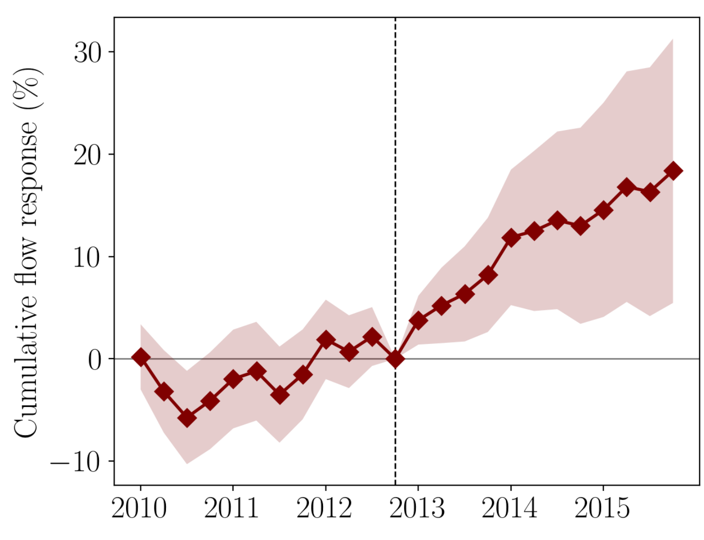
gain tax expenses for investors.

Given the spillover effect from ETF to non-ETF share classes, the study of Vanguard is an interesting one. In fact, because non-ETF share classes are also in some sense tax-efficient, then we would expect that the effect of the reform on capital flows to Vanguard ETF share classes is lower compared to the effect on capital flows to the average ETF. To test this hypothesis, I restrict the dataset to mutual funds that offered both an ETF and a non-ETF share class, and I estimate the following variant of the baseline DiD design

$$\log\left(1+f_{it}\right)=\alpha_{m(i)}+\alpha_{t}+\gamma_{t}\mathbb{I}\left(i\in Treatment\right)+\beta^{\prime}X_{it}+\varepsilon_{it}, \tag{13}$$

where $\alpha_{m(i)}$ denotes mutual fund fixed effects. Controlling for mutual fund fixed effects implies that I am comparing ETFs and non-ETF share classes offered by the same mutual fund and therefore identical in terms of portfolio allocation and portfolio manager. I report results in Figure (37). I find that the cumulative effect on log flows to ETFs is 18.37 percentage points. While statistically significant, it is lower than the baseline estimate of 28.11 percentage points. This is expected if the increase in capital flows to ETFs after 2012 was driven by households responding to the increase in capital gain taxes.

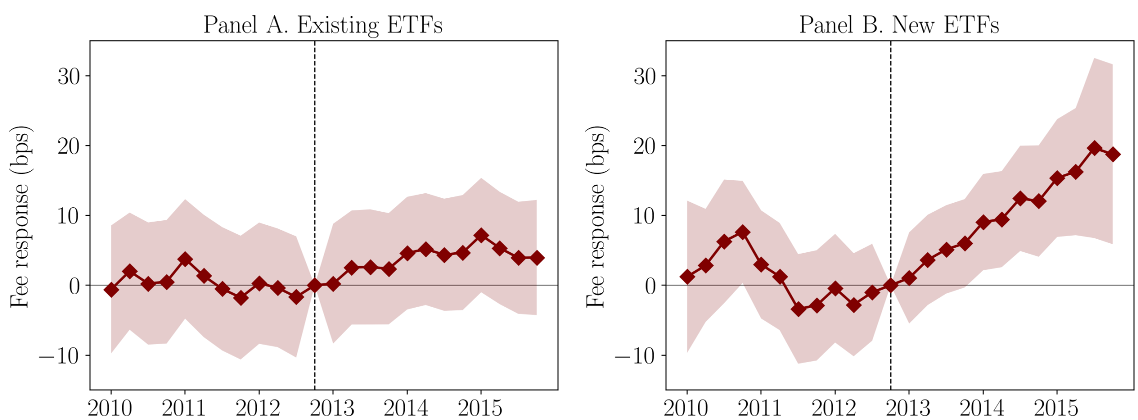
Figure 37: **ETFs and non-ETFs mutual funds**
This figure provides the cumulative effect of the increase in capital gains passed by the 2012 American Taxpayer Relief Act on log flows to ETFs, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t}\widehat{\gamma}_{u}$, where γ_t is defined in equation (13). Standard errors are clustered at mutual fund times year-quarter level and 95% confidence intervals are provided.



Finally, I report in Figure 38 the effect of the 2012 reform on the expense ratio charged by existing and new ETFs. Specifically, I estimate the baseline specification in equation (1) first on a sample of ETFs and mutual funds that are alive throughout the estimation window (2010Q1-2015Q4) and then on a sample of ETFs and mutual funds that enter the dataset for the first time in any of the year-quarters covered by the estimation window.

Consistent with the fact that mutual funds infrequently increase their expense ratio, I find that the increase in fees was insignificant for existing ETFs. On the contrary, it was mostly driven by ETFs introduced on the market after the reform was passed, which increased their expense ratio by 19 bps relative to new mutual fund share classes.

Figure 38: **Effect on the expense ratio charged by existing and new ETFs**
Panel A provides the effect of the increase in capital gain tax rates passed by the 2012 American Taxpayer Relief Act on the expense ratio charged by ETFs after restricting the dataset to share classes available throughout the estimation window (2010Q1-2015Q4). Panel B provides the effect of the increase in capital gain tax rates passed by the 2012 American Taxpayer Relief Act on the expense ratio charged by ETFs that were offered for the first time on the market in any of the year-quarter covered during the estimation window (2010Q1-2015Q4). Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



E Details on the construction of elasticities

Table 14 provides all the inputs used to construct the elasticities reported in Table 1 of the main text. I focus on the elasticities of capital flows but the same logic applies to the elasticities of fees and market entry.

The first column provides the policy parameter changed by each reform used in Section 3. The second column reports the institutional sector affected by the change in the policy parameter. The third column reports the effect of each reform on capital flows. I remark that, for the purpose of computing elasticities, I focus on the effect on capital flows obtained in a balanced panel of institutions. Focusing on a balanced panel allows to isolate the response of household demand and avoids that the elasticities also capture the response from the financial sector, especially on the extensive margin.

The fourth column reports the percentage change in the policy parameter between the last fiscal year before the reform was passed and the last fiscal year covered in the estimation of the DiD.[55] Finally, the last column reports the implied elasticities, calculated as the absolute value of the ratio between the effect on capital flows (third column) and the percentage change in the relevant policy parameter (fourth column). These elasticities coincide with the ones reported and discussed in the main text of the paper.

Table 14: **Inputs for the calculation of elasticities**
This table summarizes the inputs used to construct the elasticities of capital flows discussed in the main text. The first column provides the policy parameter changed by each reform used in Section 3. The second column reports the institutional sector affected by the change in the policy parameter. The third column reports the effect of each reform on capital flows. The fourth column reports the percentage change in the policy parameter. The last column reports the implied elasticities, calculated as the absolute value of the ratio between the effect on capital flows (third column) and the percentage change in the relevant policy parameter (fourth column). The third and fourth columns are reported in percentage points.

Policy parameter	**Institutional sector**	**Effect**	**% change in policy parameter**	**Implied elasticity**
Contribution limit	Defined contribution	30.05	36.29	0.83
Income tax rate	Life insurance	17.80	-11.15	1.60
Dividend tax rate	REITs	15.68	-9.88	1.59
Capital gain tax rate	ETFs	28.63	18.33	1.56

[55] For additional details on the calculation of the percentage changes in the policy parameters, please refer to Appendix A.

For completeness, I report in Tables 15 and 16 two alternative elasticities that are frequently observed in the macro-finance and public finance literatures. Table 15 reports the elasticities of capital flows with respect to the average keep rate, defined as one minus the average tax rate. Table 16 provides the semi-elasticities of capital flows, obtained by rescaling the effect on capital flows by the change in the average tax rate. Notice that these alternative measures of elasticities are not properly defined for changes in contribution limits on defined contribution plans, which are therefore omitted.

Table 15: **Elasticities of capital flows with respect to average keep rates**
This table summarizes the inputs used to construct the elasticities of capital flows with respect to the average keep rate, defined as one minus the average tax rate. The first column provides the policy parameter changed by each reform used in Section 3. The second column reports the institutional sector affected by the change in the policy parameter. The third column reports the effect of each reform on capital flows. The fourth column reports the percentage change in the average keep rate. The last column reports the implied elasticities, calculated as the absolute value of the ratio between the effect on capital flows (third column) and the percentage change in the relevant policy parameter (fourth column). The third and fourth columns are reported in percentage points.

Policy parameter	**Institutional sector**	**Effect**	**% change in keep rate**	**Implied elasticity**
Income tax rate	Life insurance	17.80	3.89	4.58
Dividend tax rate	REITs	15.68	3.59	4.37
Capital gain tax rate	ETFs	28.63	-2.53	11.34

Table 16: **Semi-elasticities of capital flows**
This table summarizes the inputs used to construct the semi-elasticities of capital flows. The first column provides the policy parameter changed by each reform used in Section 3. The second column reports the institutional sector affected by the change in the policy parameter. The third column reports the effect of each reform on capital flows. The fourth column reports the change in the average tax rate. The last column reports the implied semi-elasticities, calculated as the absolute value of the ratio between the effect on capital flows (third column) and the change in the average tax rate (fourth column). The third and fourth columns are reported in percentage points.

Policy parameter	**Institutional sector**	**Effect**	**Change in tax rate**	**Implied semi-elasticity**
Income tax rate	Life insurance	17.80	-2.88	6.18
Dividend tax rate	REITs	15.68	-2.63	5.96
Capital gain tax rate	ETFs	28.63	2.22	12.90

F Conceptual framework to interpret the DiD estimates

A potential concern about the DiD estimates discussed in Section 3 is that they might overestimate the true effect if households reallocate capital from the control to the treatment groups in response to fiscal reforms. In this section, I use a standard portfolio choice model to quantify the bias, if any, in the DiD estimates when households reallocate capital from the control to the treatment group.

Consider a representative investor with mean-variance preferences over her portfolio return and suppose that two assets are traded. The two assets should be thought of as two aggregate institutional sectors, for instance mutual funds and ETFs. The first asset has expected return which consists of a capital gain μ_1 and a dividend or capital gain distribution δ_1. In addition, the investor may derive non-pecuniary benefits ψ_1 from holding the asset.

Similarly, the expected return of the second asset consists of a capital gain μ_2, a dividend or capital gain distribution δ_2, and a non-pecuniary benefit ψ_2. Let Σ denote the variance-covariance matrix and suppose that there exists a risk-free asset with rate normalized to zero without loss of generality. Finally, let θ_1 and θ_2 denote the portfolio weights on assets 1 and 2, respectively.

While the first asset is subsidized by the government, the second asset is not. In particular, any distribution δ_2 made by the second asset is taxed at rate τ_δ. Moreover, the investor has to pay an income tax rate τ_y before investing in asset 2. Because asset 1 is subsidized by the government, suppose that there is a maximum limit $\overline{\theta}$ on the portfolio wight that the investor can allocate to asset 1.

The investor chooses the vector of portfolio weights θ to solve

$$\max_{\theta} \theta' \left(\mu + \delta + \psi\right) - \frac{\gamma}{2}\theta'\Sigma\theta,$$

subject to $\theta_1 \leq \overline{\theta}$, where

$$\theta \equiv \begin{bmatrix} \theta_1 \\ \theta_2 \end{bmatrix}, \ \mu \equiv \begin{bmatrix} \mu_1 \\ \mu_2\left(1-\tau_y\right) \end{bmatrix}, \ \delta \equiv \begin{bmatrix} \delta_1 \\ \delta_2\left(1-\tau_\delta\right)\left(1-\tau_y\right) \end{bmatrix}, \ \psi \equiv \begin{bmatrix} \psi_1 \\ \psi_2 \end{bmatrix}, \ \Sigma \equiv \begin{bmatrix} \sigma_1^2 & \rho\sigma_1\sigma_2 \\ \rho\sigma_1\sigma_2 & \sigma_2^2 \end{bmatrix}.$$

Let λ denote the Lagrange multiplier on the constraint $\theta_1 \leq \overline{\theta}$. The solution to the portfolio problem is

$$
\begin{aligned}
\theta_1 &= \frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\left[\sigma_2^2\left(\mu_1+\delta_1+\psi_1-\lambda\right)-\rho\sigma_1\sigma_2\left(\mu_2\left(1-\tau_y\right)+\delta_2\left(1-\tau_\delta\right)\left(1-\tau_y\right)+\psi_2\right)\right],\\
\theta_2 &= \frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\left[\sigma_1^2\left(\mu_2\left(1-\tau_y\right)+\delta_2\left(1-\tau_\delta\right)\left(1-\tau_y\right)+\psi_2\right)-\rho\sigma_1\sigma_2\left(\mu_1+\delta_1+\psi_1-\lambda\right)\right],\\
\lambda &= \max\left\{0,\, \mu_1+\delta_1+\psi_1-\frac{\rho\sigma_1}{\sigma_2}\mu_2\left(1-\tau_y\right)+\delta_2\left(1-\tau_\delta\right)\left(1-\tau_y\right)+\psi_2-\gamma\sigma_1^2\left(1-\rho^2\right)\overline{\theta}\right\}.
\end{aligned}
$$

Let's first consider a change in the tax rate τ_δ. This change reflects the reforms used for REITs and ETFs, both of which changed the tax rate on their distributions. Because neither REITs nor ETFs are subject to investment limits, suppose $\overline{\theta}\to\infty$ so that $\lambda=0$. The capital flows to assets 1 and 2 in response to the change in the tax rate are

$$
\begin{aligned}
\frac{d\theta_1}{\theta_1} &= \frac{1}{\theta_1}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\rho\sigma_1\sigma_2\delta_2\left(1-\tau_y\right)d\tau_\delta,\\
\frac{d\theta_2}{\theta_2} &= -\frac{1}{\theta_2}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\sigma_1^2\delta_2\left(1-\tau_y\right)d\tau_\delta.
\end{aligned}
$$

While the tax rate change increases capital flows to asset 1 by $\frac{d\theta_1}{\theta_1}$, the DiD estimate is $\frac{d\theta_1}{\theta_1}-\frac{d\theta_2}{\theta_2}$. Let χ denote the effect on the treatment group and $\hat{\chi}$ the DiD estimate. The fraction of DiD estimate explained by the effect on the treatment group is then

$$
\begin{aligned}
\frac{\chi}{\hat{\chi}} &= \frac{\frac{d\theta_1}{\theta_1}}{\frac{d\theta_1}{\theta_1}-\frac{d\theta_2}{\theta_2}}\\
&= \frac{\frac{1}{\theta_1}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\rho\sigma_1\sigma_2\delta_2\left(1-\tau_y\right)d\tau_\delta}{\frac{1}{\theta_1}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\rho\sigma_1\sigma_2\delta_2\left(1-\tau_y\right)d\tau_\delta+\frac{1}{\theta_2}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\sigma_1^2\delta_2\left(1-\tau_y\right)d\tau_\delta}\\
&= \frac{\theta_2\rho\sigma_2}{\theta_2\rho\sigma_2+\theta_1\sigma_1}. \qquad (14)
\end{aligned}
$$

Consider next a change in the tax rate τ_y and, as before, suppose that the constraint $\theta_1\leq\overline{\theta}$ is not binding. This change reflects the reform used for variable annuities, which decreased personal income tax rates. The capital flows to assets 1 and 2 in response to the change in the tax rate are

$$
\begin{aligned}
\frac{d\theta_1}{\theta_1} &= \frac{1}{\theta_1}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\rho\sigma_1\sigma_2\mu_2 d\tau_y,\\
\frac{d\theta_2}{\theta_2} &= -\frac{1}{\theta_2}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\sigma_1^2\mu_2 d\tau_y.
\end{aligned}
$$

Recall that the DiD design in Section 3 used variable annuities as treatment group and retirement share classes as control group. Because contributions to variable annuities are generally taxed at income tax rates, then variable annuities should be thought of as the asset that is not subsidized, i.e. asset 2. The DiD estimate is then $\frac{d\theta_2}{\theta_2} - \frac{d\theta_1}{\theta_1}$ and the fraction of DiD estimate represented by the effect on the treatment group is

$$\begin{aligned}
\frac{\chi}{\widehat{\chi}} &= \frac{\frac{d\theta_2}{\theta_2}}{\frac{d\theta_2}{\theta_2} - \frac{d\theta_1}{\theta_1}} \\
&= \frac{-\frac{1}{\theta_2}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\sigma_1^2\mu_2 d\tau_y}{-\frac{1}{\theta_2}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\sigma_1^2\mu_2 d\tau_y - \frac{1}{\theta_1}\frac{1}{\gamma}\frac{1}{\sigma_1^2\sigma_2^2}\frac{1}{1-\rho^2}\rho\sigma_1\sigma_2\mu_2 d\tau_y} \\
&= \frac{\theta_1\sigma_1}{\theta_1\sigma_1 + \theta_2\rho\sigma_2}. \qquad (15)
\end{aligned}$$

Finally, suppose that the limit on asset 1 binds, i.e.$\theta_1 = \overline{\theta}$, and consider a change in the contribution limit $\overline{\theta}$. This change reflects the reform used for retirement share classes, which increased contribution limits on defined contribution plans. The capital flows to assets 1 and 2 in response to the change in the contribution limit are

$$\begin{aligned}
\frac{d\theta_1}{\theta_1} &= \frac{d\overline{\theta}}{\theta_1}, \\
\frac{d\theta_2}{\theta_2} &= -\frac{1}{\theta_2}\rho\frac{\sigma_1}{\sigma_2}d\overline{\theta}.
\end{aligned}$$

The fraction of DiD estimate explained by the effect on the treatment group (i.e., retirement share classes) is then

$$\begin{aligned}
\frac{\chi}{\widehat{\chi}} &= \frac{\frac{d\theta_1}{\theta_1}}{\frac{d\theta_1}{\theta_1} - \frac{d\theta_2}{\theta_2}} \\
&= \frac{\frac{d\overline{\theta}}{\theta_1}}{\frac{d\overline{\theta}}{\theta_1} + \frac{1}{\theta_2}\rho\frac{\sigma_1}{\sigma_2}d\overline{\theta}} \\
&= \frac{\theta_2\sigma_2}{\theta_2\sigma_2 + \theta_1\rho\sigma_1}. \qquad (16)
\end{aligned}$$

To understand the economics behind these results, it is useful to consider a simple but realistic case in which $\rho \to 1$ and $\sigma_1 = \sigma_2$. In this case, the effect on the treatment group becomes

$$\chi = \widehat{\chi}\left(1 - \omega\right), \qquad (17)$$

where ω denotes the weight on the treatment group in the portfolio of risky assets. If the treatment group is sufficiently small relative to the control group, then any dollar outflow from the control group is significantly smaller, once rescaled for total assets, compared to the same dollar inflow into the treatment group. As a result, the estimates are virtually unbiased.

For each DiD experiment, Table 17 provides an estimate of $\frac{\chi}{\tilde{\chi}}$. I use equation (14) to compute $\frac{\chi}{\tilde{\chi}}$ for REITs and ETFs. I use equation (15) to compute $\frac{\chi}{\tilde{\chi}}$ for variable annuities. I use equation (16) to compute $\frac{\chi}{\tilde{\chi}}$ for retirement share classes.

I estimate θ_1/θ_2 as the ratio between total assets in the treatment and control group in the last year-quarter before each reform became effective. All other moments are computed using the time-series of value-weighted returns in the treatment and control groups over the entire sample (1999Q1-2023Q4). Results suggest that the true effect accounts for most of the DiD estimates, ranging from 82.4% for ETFs to 99.2% for retirement share classes.

Table 17: **Correction of DiD estimate**s
This table provides estimates of $\frac{\chi}{\tilde{\chi}}$ for all institutional sectors studied in the paper. Rows 2 to 5 provide the inputs required to compute $\frac{\chi}{\tilde{\chi}}$, which is reported in the last row.

	Retirement share classes	**Variable annuities**	**REITs**	**ETFs**
θ_1/θ_2	0.006	0.052	0.104	0.200
ρ	0.969	0.937	0.610	0.988
σ_1	0.062	0.070	0.091	0.092
σ_2	0.047	0.062	0.098	0.087
$\frac{\chi}{\tilde{\chi}}$	0.992	0.947	0.864	0.824

G Long-term growth explained by fiscal policy

G.1 Details on extrapolation

The policy parameters studied in the paper experienced changes other than the ones directly tested in Section 3.

A second reform that changed the tax rate on dividends paid by REITs was the 2017 Tax Cut and Jobs Act. In fact, this reform not only decreased personal income tax rates but it also introduced a 20% deduction on dividends paid by REITs. As discussed in Chodorow-Reich et al. (2024), however, the 2017 reform was accompanied by several other provisions and, in particular, by the largest decrease in corporate income tax rates ever passed in the U.S. This change likely increased equity issuance by corporations relative to REITs, which I confirm in Appendix D.3. Although the contemporaneous decrease in corporate income tax rates represents a key confounding effect, the decrease in personal income tax rates and, especially, the 20% deduction likely increased demand for REITs.

Turning to variable annuities, the 2001 Economic Growth and Tax Relief Reconciliation Act decreased personal income tax rates which likely increased capital flows to variable annuities relative to retirement share classes. However, this effect is not testable in the data because the 2001 Economic Growth and Tax Relief Reconciliation Act contemporaneously increased contribution limits on defined contribution plans. As discussed in Section 3.2, this increase in contribution limits strongly increased capital flows to retirement share classes.

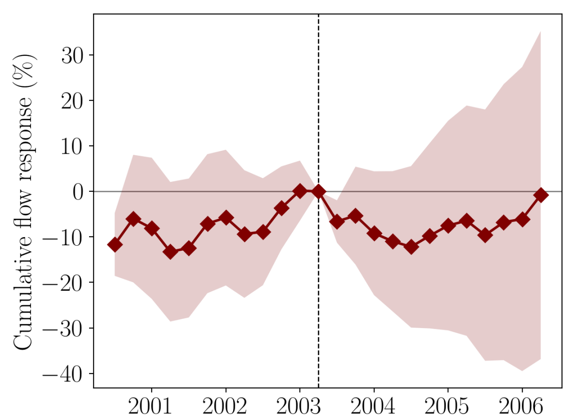
Defined contribution plans also presumably benefited from the constant increase in contribution limits that took place after 2005. However, this increase in contribution limits was not an unanticipated shock like in 2001. On the contrary, it was a result of the 2001 Economic Growth and Tax Reconciliation, which, for years after 2005, indexed the increase in contribution limits to annual inflation rates.

As for ETFs, a second reform that changed capital gain tax rates was the 2003 Jobs and Growth Tax Relief Reconciliation Act. This reform decreased capital gain tax rates across all income brackets, which may have decreased capital flows to ETFs relative to mutual funds. While the Jobs and Growth Tax Relief Reconciliation Act also introduced a large decrease in tax rates on qualified dividends, ETFs do not offer any advantage in terms of dividend taxation relative to mutual funds. Because, to the best of my knowledge, no other confounding shock affected ETFs and mutual funds differently around 2003, I can directly test whether the 2003 reform affected capital flows to ETFs relative to mutual funds. Figure 39 provides the estimates of the baseline DiD design in equation (1) around

2003. The treatment group is represented by ETFs while the control group is represented by mutual funds.

Estimates suggest that the 2003 reform did not affect capital flows to ETFs relative to mutual funds. This is reasonable since ETFs are close substitutes to mutual funds. After the decrease in capital gain taxes, ETFs were still more tax efficient than mutual funds, and investors that were holding ETFs to begin with did not reallocate from ETFs to mutual funds. At the same time, investors that were holding mutual funds to begin with have even stronger incentives to hold mutual funds after capital gain tax rates decreased. Because no other reform affected capital gain tax rates between 2000 and 2020, this lack of effect from the 2003 Jobs and Growth Tax Relief Reconciliation Act suggests that no extrapolation is needed for ETFs.

Figure 39: **Effect of the 2003 Jobs and Growth Tax Relief Reconciliation Act on capital flows to ETFs**
This figure provides the cumulative effect of the 2003 Jobs and Growth Tax Relief Reconciliation Act on log flows to ETFs, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\gamma}_u$, where γ_t is defined in equation (1). Standard errors are clustered at style times year-quarter level and 95% confidence intervals are provided.



On the contrary, for retirement share classes, variable annuities, and REITs, I summarize the inputs used to estimate the extrapolated effect in Table 18. Specifically, the first row reports the initial fraction of long-term growth explained by fiscal policy. These numbers coincide with those reported in Figure 13, Panel A. The second row reports the percentage change in the policy parameter. For REITs and variable annuities, I use the percentage change in the average keep rates. Results are identical if I use the percentage change in the average tax rates. The third row reports the implied elasticity, computed as the first row divided by the second row.

The fourth row provides the percentage change in the policy parameters not included in the initial $\frac{\widehat{g}}{g}$. For defined contribution plans, this coincide with the percentage change in total contribution limits on 401(k) plans and IRAs between 2020 and 2005. For life insurance companies, it coincides with the percentage change in the average keep rate on personal income observed between 2001 and 2005 plus the percentage change in the average keep rate on personal income observed between 2012 and 2015. For REITs, it coincides with the percentage change in the average keep rate on dividends paid by REITs observed between 2012 and 2015 plus the percentage change in the average keep rate on dividends paid by REITs observed between 2017 and 2020.

The fifth row reports the extrapolated effect which is equal to the third row multiplied by the fourth row. Finally, the last row provides the updated value of $\frac{\widehat{g}}{g}$, obtained as the sum of both the baseline effect (first row) and the extrapolated effect (fifth row). The last row coincides with the numbers reported in Figure 13, Panel B.

Table 18: **Inputs for extrapolation**
This table summarizes the inputs used to extrapolate the effects of fiscal reforms on capital flows. Numbers in all rows but the third one are reported in percentage points.

	Defined contribution	**Life insurance**	**REITs**
Initial $\frac{\widehat{g}}{g}$	21.25	30.84	15.60
% change in policy parameter	36.29	3.89	3.59
Implied elasticity	0.59	7.93	4.35
% change in policy parameter not tested	31.45	2.01	8.57
Extrapolated effect	18.41	15.90	37.31
Final $\frac{\widehat{g}}{g}$	39.66	46.73	52.91

G.2 Long-term growth explained under homogeneous treatment

For each institutional sector, Figure 40 compares $\widehat{g}$ estimated under the assumption of both homogeneous and heterogeneous treatment with the actual growth rate in aggregate assets $\log\left(\frac{\sum_i A_{iT}}{\sum_i A_{i0}}\right)$ observed around the reform. As discussed in Section 4, the estimates under homogeneous treatment are virtually identical to the estimates under heterogeneous treatment, suggesting that fiscal reforms increased capital flows to aggregate financial sectors regardless of the specific characteristics of individual intermediaries.

Figure 40: **Estimated and actual growth of institutional sectors around fiscal reforms**
This figure reports the estimated effect of fiscal reforms on the growth rate of aggregate assets managed by different institutional sectors. Estimates are computed under the assumption of homogeneous and heterogeneous treatment. For each institutional sector, the actual growth rate of aggregate assets is also reported.

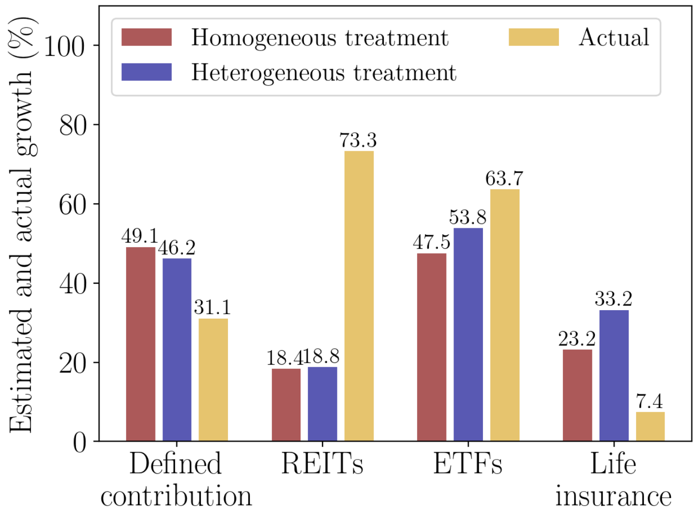
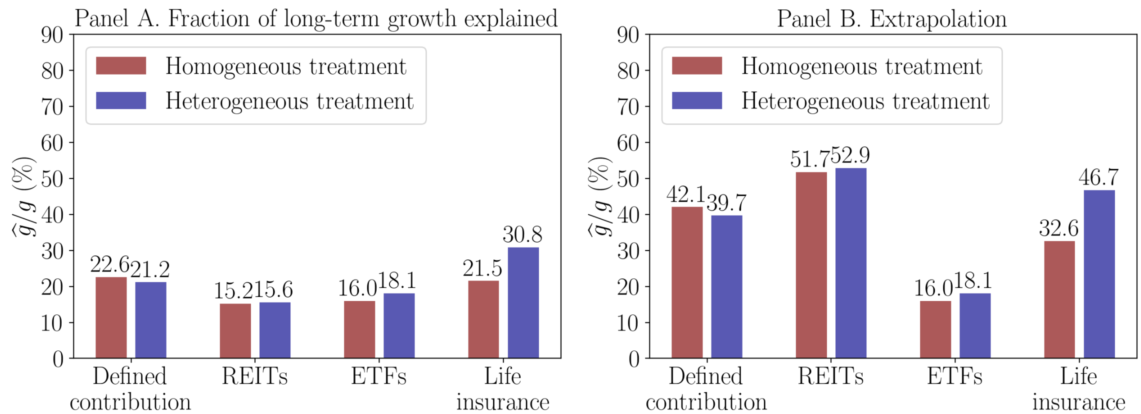


Figure 41 compares instead the effects estimated under the assumptions of homogeneous and heterogeneous treatment with the long-term growth of aggregate institutional sectors, defined as the cumulative value-weighted flow $g = \sum_t \sum_i \frac{A_{it}}{\sum_i A_{it}} \log(1 + f_{it})$ observed between 2000Q1 and 2020Q4. For each estimate $\widehat{g}$ presented in Figure 40, Panel A of Figure 41 reports $\frac{\widehat{g}}{g}$. Panel B of Figure 41 reports instead the effects extrapolated to account for any change in the policy parameters observed between 2000Q1 and 2020Q4. As expected given the result in Figure 40, results under the assumptions of homogeneous and heterogeneous treatment are virtually identical.

Figure 41: **Fraction of long-term growth explained by fiscal policy**
Panel A reports the fraction of long-term growth of aggregate institutional sectors explained by fiscal policy, where the long-term growth of each institutional sector is computed as the cumulative value-weighted flow observed between 2000Q1 and 2020Q4. Panel B uses the elasticities discussed in Section 3.6 to extrapolate the effect of changes in the relevant policy parameter that occurred outside the reforms studied in Section 3.



H Additional results on asset pricing effects

In this section, I provide additional results on the effect of the 2012 increase in capital gain tax rates on asset returns.

First, the baseline equation estimated in Section 5 uses $m_{a2012Q4}$ as continuous treatment. However, if ETFs tilted ex-ante their holdings towards stocks more likely to benefit from the increase in capital gain tax rates then the effect of the reform on asset returns would be overestimated. I then estimate the following variant of equation (6)

$$r_{at} = \alpha_{c(a)t} + \gamma m_{a\tau} + \delta m_{a\tau} \mathbb{I}\left(t > 2012Q4\right) + \psi' \left(X_{a,t-1}\right) R_t + \varepsilon_{at}, \tag{18}$$

where $\mathbb{I}\left(t > 2012Q4\right)$ denotes an indicator equal to one in all year-months after the reform was passed in 2012Q4. Equation (18) is still a DiD design in which δ identifies the average effect of the reform on r_{at} across all year-months in the post-reform period.

I estimate equation (18) using $m_{a\tau}$ constructed in multiple year-quarters τ. I use 2011Q2, 2011Q4, 2012Q2, and 2012Q4. I report the results in Table 19 where each column corresponds to a different year-quarter τ used to construct $m_{a\tau}$. The estimates of δ remain virtually identical regardless of the year-quarter τ used to construct $m_{a\tau}$ suggesting that ETFs did not tilt their portfolios to stocks more likely to benefit from the reform.

The effect estimated in Section 5 is based on a DiD design with continuous treatment. Because DiD designs with continuous treatment are based on the stronger assumption of parallel trends between any pair of values that the treatment can take, I report also results from a discrete treatment specification. In particular, I estimate the following variant of equation (6)

$$r_{at} = \alpha_{c(a)t} + \delta_t \mathbb{I}\left(m_{a2012Q4} > p_{2012Q4}\right) + \psi' \left(X_{a,t-1}\right) R_t + \varepsilon_{at}, \tag{19}$$

where p_{2012Q4} denotes the median of $m_{a2012Q4}$ across stocks. Equation (19) is identical to equation (6) except for the treatment assignment which is now discrete and includes all stocks with $m_{a2012Q4}$ above the cross-sectional median. Figure 42 provides estimates of $\sum_{u=-T}^{t} \delta_u$ from equation (19). Stocks with $m_{a2012Q4}$ above the cross-sectional median outperform stocks with below-median $m_{a2012Q4}$ by 6.97 percentage points annually in the three years after the reform was passed. Overall, the discrete treatment specification confirms the large and persistent effect of fiscal policy on the cross-section of asset returns as emphasized in the main body of the paper.

I further check that the effects are robust across several specifications of the baseline

Table 19: **Sensitivity of asset pricing effects to sorting year-quarter**
This table provides the effect on asset returns when the mismatch measure is constructed based on year-quarters other than 2012Q4. The estimation window is from January 2010 to December 2015. Standard errors are clustered at industry times year-month level.

	2012Q4	2012Q2	2011Q4	2011Q2
$m_{a\tau}$	-0.254***	-0.294***	-0.247***	-0.287***
	(0.052)	(0.053)	(0.072)	(0.071)
$m_{a\tau}\mathbb{I}\left(t > 2012Q4\right)$	0.349***	0.380***	0.315***	0.364***
	(0.074)	(0.080)	(0.098)	(0.098)
Industry × year-quarter	Yes	Yes	Yes	Yes
Observations	109,232	110,452	109,248	107,413
R^2	0.321	0.321	0.323	0.327
Within R^2	0.191	0.193	0.195	0.194

Clustered standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

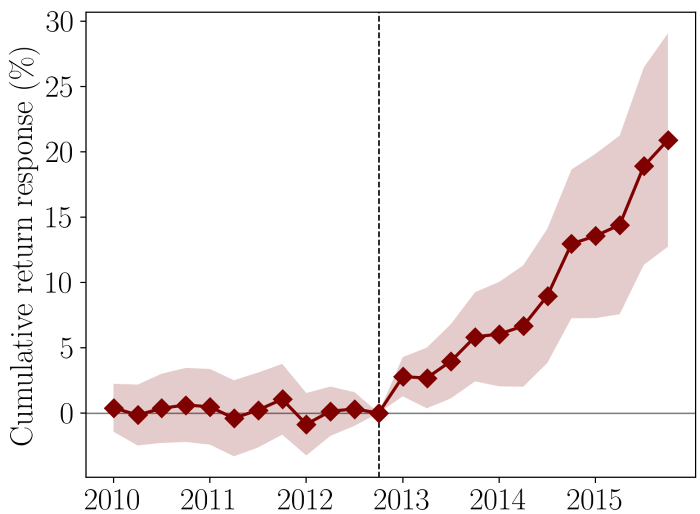
econometric design. To this end, I estimate several versions of equation (18) which I progressively saturate with additional fixed effects. I use $m_{a2012Q4}$ constructed in year-quarter 2012Q4 as continuous treatment. I report the results from this exercise in Table 20. I first estimate equation (18) with no fixed effects and I keep micro-cap stocks which are excluded in any of the estimate discussed so far. In the second column, I exclude micro-cap stocks from the estimation. In the third column, I include industry fixed effects. In the fourth column, I include year-quarter fixed effects. In the last column, I include industry times year-quarter fixed effects as in the baseline specification. In Table 21, I report the results of similar exercises estimated under discrete treatment.

One final concern about the estimates discussed in Section 5 is that I do not control for stock fixed effects. To address this concern, for each stock in the dataset, I estimate the following factor model

$$r_{at} = \alpha_a + \Delta\alpha_a \mathbb{I}\left(t > 2012Q4\right) + \gamma_a R_t + \varepsilon_{at}. \tag{20}$$

Equation (20) is a standard factor model where R_t includes the market return, the three Fama-French factors, momentum, and the average return across stocks in the same industry as stock a. However, I also include an indicator $\mathbb{I}\left(t > 2012Q4\right)$ equal to one in

Figure 42: **Discrete treatment**
This figure provides the cumulative effect on asset returns, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\delta}_u$, where δ_t is defined in equation (19). The treatment (control) group includes securities with mismatch $m_{a2012Q4}$ above (below) the median across stocks as of 2012Q4. Standard errors are clustered at industry times year-month level and 95% confidence intervals are provided.



all year-months after the reform was passed. The coefficient $\Delta\alpha_a$ therefore captures the change in alpha experienced by stock a after the reform was passed. Because I estimate equation (20) stock by stock, then I am implicitly controlling for unobservables at stock level. In particular, I estimate equation (20) stock by stock and over a time window that ranges from January 2010 to December 2015.

Figure 43 reports the average of $\widehat{\Delta\alpha_a}$ across stocks grouped in five quintiles based on $m_{a2012Q4}$. Figure 43 shows that unobservables at stock level are unlikely to explain the large and persistent effect of the reform on asset prices. The change in annual alpha increases monotonically in $m_{a2012Q4}$ and ranges from -6.67 percentage points for stocks in the bottom quintile to 5.26 percentage points for stocks in the top quintile.

Finally, in order to compute the implied multiplier, I estimate the following variant of equation (6)

$$\Delta q_{at} = \alpha_{c(a)t} + \delta_t m_{a2012Q4} + \psi' \left(X_{a,t-1}\right) R_t + \varepsilon_{at}, \tag{21}$$

where Δq_{at} denotes the percentage change in the number of shares of stock a held by all mutual funds and ETFs in year-quarter t. Results are reported in Figure 44. To evaluate how long it takes for the effect of reform to dissipate, I increase the post-reform period up to 2020. The left panel provide estimates of $\sum_{u=-T}^{t} \delta_u$ from equation (21) while the right panel reports estimates of $\sum_{u=-T}^{t} \delta_u$ from equation (21) when r_{at} is used as the outcome variable. To facilitate interpretation of the magnitudes, I multiply all estimates by $m_{a2012Q4} = 1\%$ so that they can be interpreted as the effect on demand and returns of a 1% increase in m_{2012Q4}. As of 2015Q4 (the last year-quarter considered in the baseline specification), I find that a 1 percentage point increase in m_{2012Q4} increases shares held by mutual funds and ETFs by 3.69% and, at the same time, returns increase by 7.54 percentage points. The ratio between the two estimates reveal a multiplier of 2.04, as discussed in the main text. Moreover, the effect dissipates and the increase in returns flattens around 2019.

Table 20: **Sensitivity of asset pricing effects**
This table provides the effect on asset returns across different specifications of the continuous treatment design. The estimation window is from January 2010 to December 2015. Standard errors are clustered at industry times year-month level.

	(1)	(2)	(3)	(4)	(5)
$m_{a2012Q4}$	-0.243***	-0.286***	-0.282***	-0.266***	-0.254***
	(0.048)	(0.053)	(0.052)	(0.052)	(0.052)
$m_{a2012Q4}\mathbb{I}\,(t > 2012Q4)$	0.324***	0.449***	0.437***	0.403***	0.349***
	(0.068)	(0.076)	(0.075)	(0.074)	(0.074)
Micro-caps	Yes	No	No	No	No
Industry	No	No	Yes	No	No
Year-quarter	No	No	No	Yes	No
Industry × year-quarter	No	No	No	No	Yes
Observations	217,508	109,232	109,232	109,232	109,232
R^2	0.138	0.244	0.249	0.245	0.321
Within R^2			0.245	0.173	0.191

Clustered standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 21: **Sensitivity of asset pricing effects under discrete treatment**
This table provides the effect on asset returns across different specifications of the discrete treatment design. The estimation window is from January 2010 to December 2015. Standard errors are clustered at industry times year-month level.

	(1)	(2)	(3)	(4)	(5)
$\mathbb{I}\left(m_{a2012Q4} > p_{2012Q4}\right)$	0.000	-0.001	-0.001	-0.005***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$\mathbb{I}\left(m_{a2012Q4} > p_{2012Q4}\right) \mathbb{I}\left(t > 2012Q4\right)$	0.001	-0.000	-0.000	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Micro-caps	Yes	No	No	No	No
Industry	No	No	Yes	No	No
Year-quarter	No	No	No	Yes	No
Industry × year-quarter	No	No	No	No	Yes
Observations	217,508	109,232	109,232	109,232	109,232
R^2	0.138	0.244	0.249	0.245	0.321
Within R^2			0.245	0.173	0.191

Clustered standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Figure 43: **Effect on alpha**
This figure provides the average change in alpha for stocks in different quintiles of $m_{a2012Q4}$. For each stock a, I annualize the change in alpha by multiplying $\widehat{\Delta\alpha}_a$ by 12.

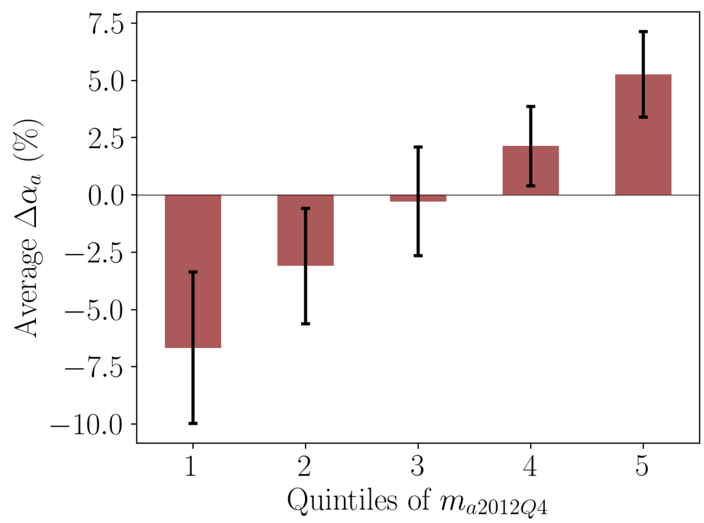
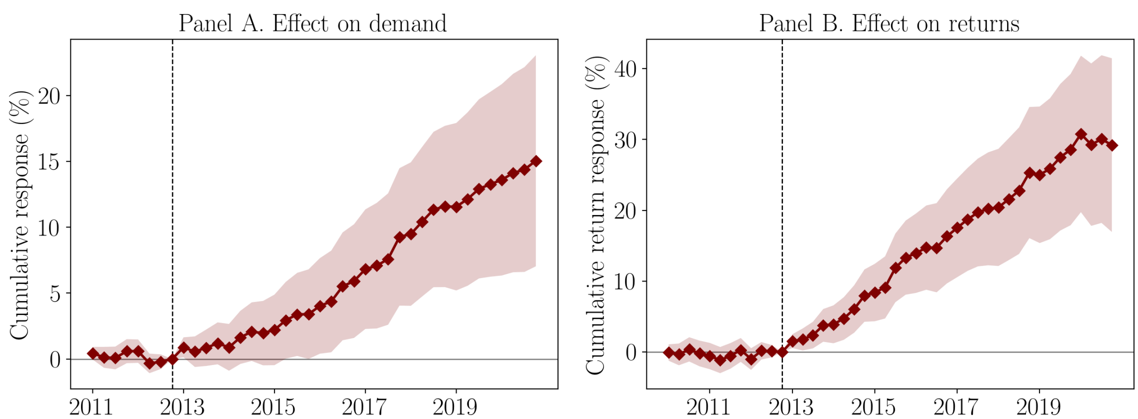


Figure 44: **Effect on demand and asset prices**
Panel A reports the cumulative effect of the 2012 reform on demand of mutual funds and ETFs, summarized for each year-quarter t by the estimate $\sum_{u=-T}^{t} \widehat{\delta}_u$, where δ_t is defined in equation (21). Panel B reports the cumulative effect of the reform on the return of assets held by mutual funds and ETFs. Standard errors are clustered at industry times year-quarter level and 95% confidence intervals are provided.



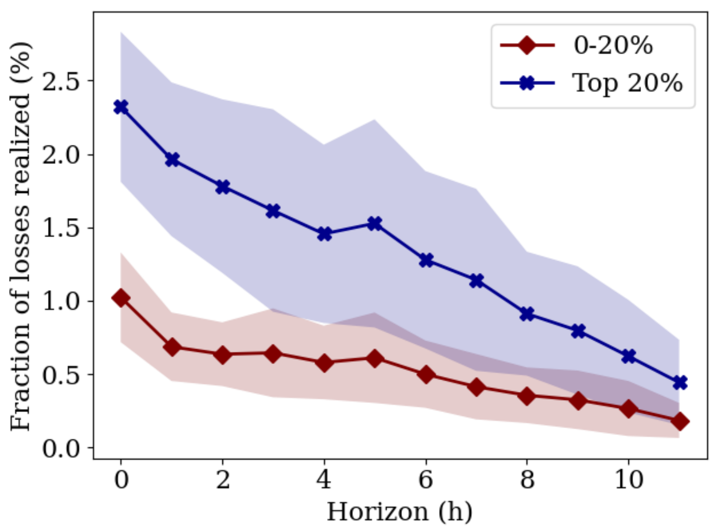
I Additional results on individual households

In this section, I report additional results from the Addepar dataset.

I.1 Robustness of baseline results

To visualize the difference in the fraction of losses realized over time by households in the top and bottom 20%, I report in Figure 45 the same estimates discussed in Panel B of Figure 18 but I include 95% confidence intervals. The difference in the fraction of losses realized by the two groups remain statistically significant up to $h = 5$ year-months. For $h \geq 6$, I cannot reject any more that the two estimates are equal at 95% confidence level.

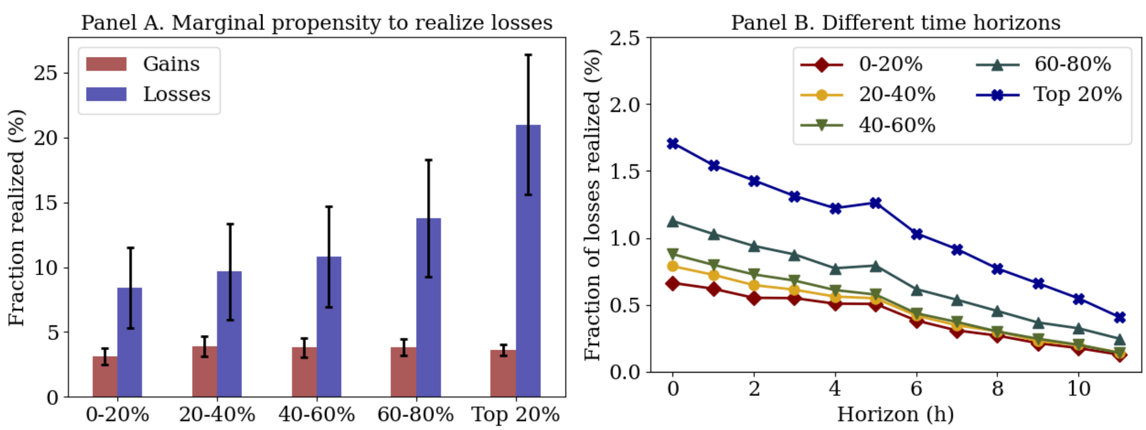
Figure 45: **The fraction of losses realized by top and bottom 20%**
This figure compares the fraction of losses realized by the top and bottom 20% of the wealth distribution at different horizons $h \in \{0, 1, .., 11\}$. For each wealth group, standard errors are clustered at year-month level.



One concern about the estimates discussed in Section 6 is that households at the lower end of the wealth distribution have a larger fraction of assets invested in qualified accounts (e.g. 401(k) plans and IRAs). Because capital gains realized in these accounts are not taxed, then less-wealthy households may have less incentives to realize capital losses. While the Addepar dataset does not flag whether assets are held within or outside retirement accounts, more than 95% of the assets in retirement accounts in the U.S. are allocated to mutual funds (including balanced funds, target-date funds, and money market funds).

I then repeat the estimation of the results in Figure 18 after restricting the dataset to directly held equity and fixed-income securities and report the results in Figure 46. I include in the estimation directly held government bonds, corporate bonds, convertible bonds, municipal bonds, ABS/MBS, common stocks, preferred stocks, convertible stocks. I also include depository receipts on equity and debt that represent stocks and bonds issued by foreign corporations and publicly traded in U.S. markets. I find that results are unchanged. Households in the bottom 20% still realize 8.40% of unrealized losses compared to 21.00% for households in the top 20%. Moreover, I cannot reject the null hypothesis that the new estimates are all equal to the original ones.

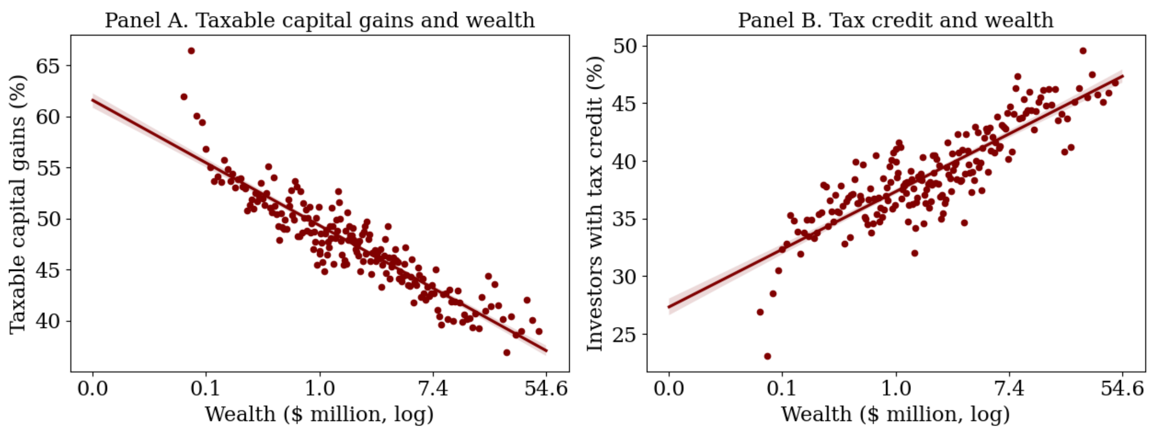
Figure 46: **The fraction of gains and losses realized outside retirement accounts**
Panel A provides the fraction of unrealized losses that households in each wealth quintile g realize on average, summarized by the estimate $\widehat{\gamma}_g$ in equation (7). I also report the fraction of unrealized gains that households realize on average, which are obtained from estimating equation (7) with rg_{it} as the outcome variable and $ug_{i,t-1}$ on the right-hand side. Panel B reports estimates of equation (7) with the outcome variable being $rl_{i,t+h}$ for different horizons $h \in \{0, 1, .., 11\}$. In Panel A, standard errors are clustered at wealth quintile times year-month level and 95% confidence intervals are provided. The dataset is restricted to only include directly held equity and fixed-income securities.



Using the dataset restricted to directly held equity and fixed-income securities, I further report in Figure 47 the fraction of taxable gains (Panel A) and the fraction of households with zero tax expense (Panel B) as a function of the initial log wealth $\log A_{i,t-1}$. Like in the baseline results, taxable capital gains decrease strongly in wealth from 55% of total realized gains for households with wealth around \$100,000 to 35% for households with wealth around \$50 million. This result implies that 65% of realized gains are not taxable on average for households at the top of the wealth distribution. Moreover, around 45% of households with wealth around \$50 million report zero taxable capital gains compared to 30% of households with wealth around \$100,000.

These results confirm that differences in the response to tax incentives across the wealth distribution are not explained by the fact that less-wealthy households have a larger share of assets in retirement accounts. On the contrary, they are unconditionally less responsive and this has substantial implication for heterogeneity in after-tax performance.

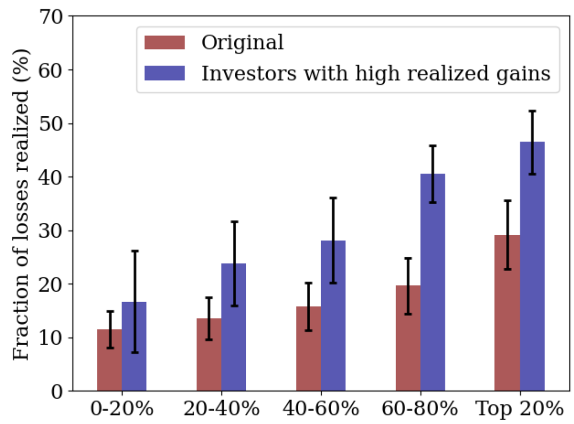
Figure 47: **Taxable capital gains outside retirement accounts**
For each household-year in the dataset, Panel A provides a binned scatterplot of the taxable capital gains over total realized gains on the y-axis against the log of total wealth observed, for each household, at the beginning of each year. Panel B provides instead the fraction of household-years for which I observe zero taxable gains or, equivalently, a tax credit that can be carried forward to future years. The dataset is restricted to only include directly held equity and fixed-income securities.



I.2 Tax-loss harvesting in December

If wealthier households are indeed more responsive to tax incentives, we would expect that they realize a larger fraction of losses in December when they have realized substantial gains until November. To test this hypothesis, I first compute for which household total capital gains realized in each fiscal year until November net of realized losses. I then estimate equation (7) after I restrict the dataset only to households in each wealth quintile that are above the median of net gains realized until November. I further restrict the dataset to include only household-months observed in December of every year. I report the estimates in Figure 48 together with the original estimates discussed in Section 6.3. Confirming that wealthier households are more responsive to tax incentives, the increase in the fraction of losses realized is larger for households in the fourth quintile and in the top 20%. Moreover, only for these two quintiles, the new estimates are statistically different than the original ones.

Figure 48: **The fraction of losses realized**
This figure provides the fraction of losses realized in December by households that are above the median of net realized gains as of November of each fiscal year (blue). The figure also provides the original estimates discussed in Figure 18 (red). Standard errors are clustered at wealth quintile times year-month level and 95% confidence intervals are provided.



I.3 Sales and purchases around zero unrealized returns

The higher propensity to realize losses estimated for the top 20% of the wealth distribution suggests that households in this group should also exhibit a higher propensity to sell losing securities, consistent with tax-loss harvesting strategies (Constantinides, 1983).[56] To test this hypothesis, I investigate whether the trading behavior of individual households display any discontinuity in a narrow interval around zero unrealized returns. The empirical strategy exploits, for a given household, variation between the sales of securities with slightly positive unrealized returns (unrealized gains) and the sales of securities with slightly negative unrealized returns (unrealized losses). Identification relies on the assumption that, in a sufficiently narrow interval around zero, whether a security exhibits an unrealized gain or a loss is as good as random.

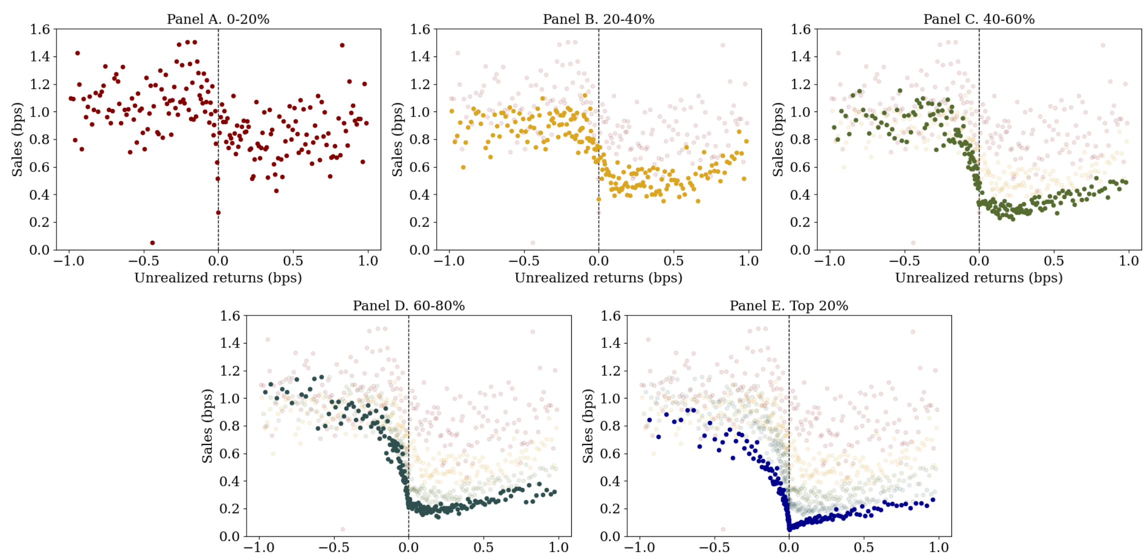
To visualize the variation used in the estimation, I start by reporting in Figure 49 the relation between sales and unrealized returns across securities and households in each quintile of the wealth distribution. Following Gabaix et al. (2024), I construct sales as $f_{iat}^{(s)} = -\frac{F_{iat}\mathbb{I}(F_{iat}<0)}{A_{i,t-1}^{DH}}$, where F_{iat} denotes dollar flows of household i to security a in year-month t, $\mathbb{I}\left(F_{iat} < 0\right)$ is an indicator equal to 1 if $F_{iat} < 0$, and the denominator is initial

[56]If true, this result would suggest that theories of loss aversion (Kahneman and Tversky (1979); Kahneman et al. (1991); Tversky and Kahneman (1991); Benartzi and Thaler (1995); Barberis et al. (2001)) are better suited to describe the behavior of less-wealthy compared to wealthier households.

wealth constructed as in Davis and Haltiwanger (1992).[57] Let also $ur_{iat} = \frac{UR_{iat}}{A_{it}}$ denote the unrealized return on security a held by household i in year-month t rescaled by total assets. I focus on March 2023, the last year-month covered in the dataset, and I restrict attention to positions that in March 2023 reported an initial unrealized return lower than 1bps, i.e. $|ur_{ia,t-1}| < 1$bps.

Figure 49 provides two key takeaways. On the one hand, sales decreases monotonically in wealth, possibly due to excessive rebalancing of less-wealthy households (Barber and Odean, 2000). On the other hand, the slope of the relation exhibits a strong discontinuity around zero for households at the higher end of the wealth distribution but not so much for less-wealthy households. This is in line to what we would expect if wealthier households are more responsive to tax incentives and therefore sell a larger fraction of securities with larger rather than lower initial unrealized losses. Quantitatively, for households in the top 20% of the wealth distribution, the ratio of sales to unrealized gains is 0.2 while the ratio of sales to unrealized losses is close to 1.

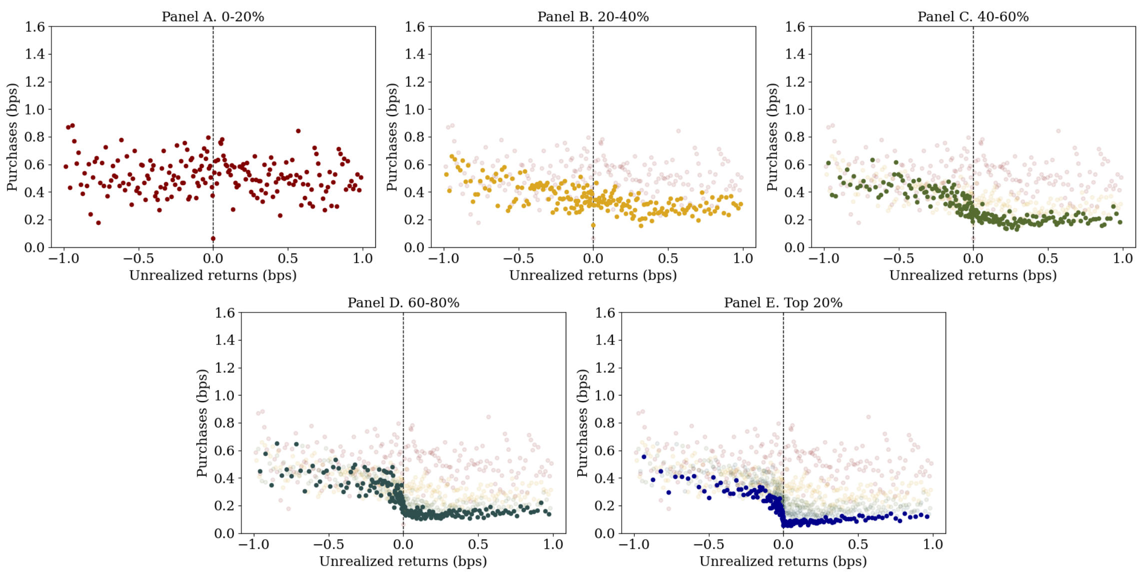
Figure 49: **Relation between unrealized returns and sales around zero**
Each panel reports a binned scatterplot of sales against unrealized returns in a narrow interval around zero for a given quintile of wealth distribution. To better visualize the relation across wealth quintiles, each panel further includes in the background the relation between sales and unrealized returns observed in lower wealth quintiles. The dataset is restricted to securities for which I observe $|ur_{iat}| < 1$bps.



[57] Specifically, $A^{DH}_{i,t-1} \equiv \frac{1}{2}\sum_a A_{iat} + \frac{1}{2}\sum_a \left(A_{iat} - R^{\$}_{iat}\right)$ is the average of initial and final total wealth, where final wealth is adjusted for valuation effects summarized by the dollar return $R^{\$}_{iat}$ gained by household i on security a in year-month t.

If the discontinuity in the slope around zero is indeed due to a response to tax incentives, we would expect such discontinuity to disappear (or at least to be significantly less pronounced) if purchases instead of sales are reported on the y-axis. This is indeed what I observe in Figure 50, where I repeat the exercise in Figure 49, but I now plot purchases instead of sales against unrealized returns.[58] While some discontinuity is still observable for households in the top 20%, it is significantly lower than the one observed for sales. Moreover, I do not find that purchases increase for assets with positive unrealized returns, which would be consistent with households in the top 20% trading momentum. At the same time, it is still true that households in the bottom 20% purchase more than wealthier households regardless of the level of unrealized returns.

Figure 50: **Relation between unrealized returns and purchases around zero**
Each panel reports a binned scatterplot of purchases against unrealized returns in a narrow interval around zero for a given quintile of wealth distribution. To better visualize the relation across wealth quintiles, each panel further includes in the background the relation between purchases and unrealized returns observed in lower wealth quintiles. The dataset is restricted to securities for which I observe $|ur_{iat}| < 1$bps.



Informed by these facts, I quantify more formally the change in the slope by estimating the following regression in each year-month in the dataset and for each wealth quintile g around zero unrealized returns

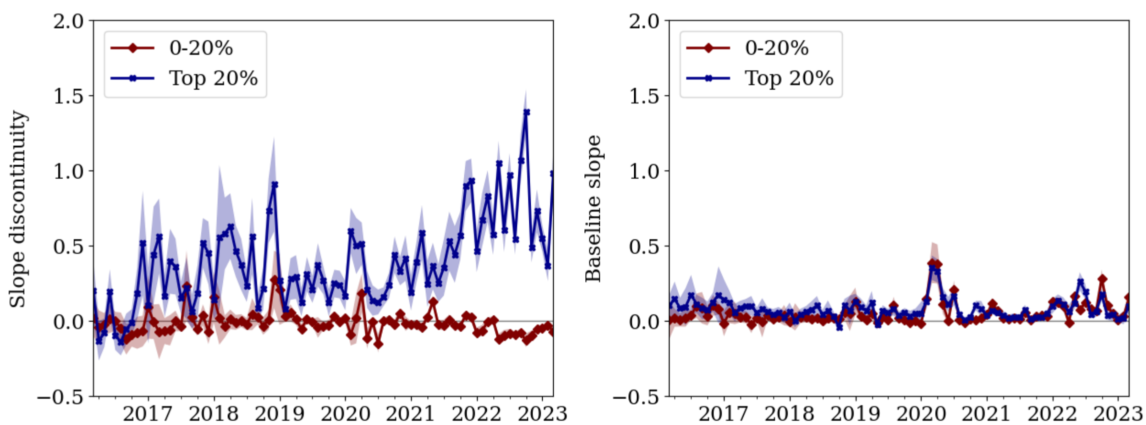
$$f^{(s)}_{iat} = \alpha_{it} + \delta_{g(i)t}\mathbb{I}\left(ur_{ia,t-1} < 0\right) + \beta_{g(i)t} ur_{ia,t-1} - \gamma_{g(i)t} ur_{ia,t-1}\mathbb{I}\left(ur_{ia,t-1} < 0\right) + \varepsilon_{iat} \quad (22)$$

[58] Similarly to sales, I define purchases of security a by household i in year-month t as $f^{(b)}_{iat} = \frac{F_{iat}\mathbb{I}(F_{iat}>0)}{A^{DH}_{i,t-1}}$.

where $\mathbb{I}\left(ur_{ia,t-1} < 0\right)$ is an indicator equal to 1 if $ur_{ia,t-1} < 0$. I control for household fixed effects α_{it} to exploit only variation across securities held by the same household. Because the slope is negative for $ur_{ia,t-1} < 0$ and positive for $ur_{ia,t-1} > 0$, I invert the sign of $ur_{ia,t-1}$ in constructing the interaction with $\mathbb{I}\left(ur_{ia,t-1} < 0\right)$. This ensures that $\gamma_{g(i)t}$ captures, for each wealth quintile and year-month, the change in the magnitude of the slope which I will refer to as marginal propensity to sell losers or MPSL.[59]

Panel A of Figure 51 compares γ_{gt} estimated separately for households in the top and bottom 20%. The estimated MPSL is higher for households in the fifth wealth quintile in every year-month of the sample. The difference is statistically and economically significant going from virtually zero for households in the bottom 20% to 0.48 on average for households in the top 20%. As a sanity check, I report in Panel B estimates of $\beta_{g(i)t}$ which capture the slope of the relation between sales and unrealized gains. Consistent with tax incentives explaining the heterogeneous discontinuities around zero, the difference in the baseline slopes $\beta_{g(i)t}$ estimated for the bottom and top 20% are statistically insignificant in virtually every year-month of the sample.

Figure 51: **Slope discontinuity around zero**
Panel A provides the time-series of $\gamma_{g(i)t}$, estimated in each year-month separately for households in the first and fifth quintiles of the wealth distribution. Panel B provides instead the time-series of estimates for $\beta_{g(i)t}$. The dataset is restricted to securities for which I observe $|ur_{iat}| < 1$bps. Standard errors are clustered at household-level and 95% confidence intervals are provided.



[59] Because the source of variation used for estimation is across securities held by a given household in a given month, I restrict attention to household and year-months for which I observe both 10 securities with positive unrealized returns and 10 securities with negative unrealized returns.

J A framework of wealth accumulation and allocation

I provide additional intuition on the difference between the elasticities discussed in Section 3.6 and the elasticity of taxable wealth in Jakobsen et al. (2020). I use their model with the main exception that I allow households to trade multiple assets. To ensure that households have incentive to hold all assets, I allow returns to be random. These simple extensions will allow me to evaluate both household response to changes in a wealth tax and household response to changes in a capital gain or dividend tax on one of the assets. Quantitatively, I will show that the extended framework can closely match both the elasticity of taxable wealth to the wealth tax estimated in Jakobsen et al. (2020) and the elasticities of capital flows to changes in dividend and capital gain tax rates discussed in Section 3.6 of this paper. The intuition is that households can only respond to a decrease in the wealth tax by increasing wealth accumulation and reducing consumption. The magnitude of this response is mainly governed by the elasticity of intertemporal substitution and by bequest motives. On the contrary, when households are allowed to trade two assets, one of which is taxed at a preferential tax rate, they can reallocate wealth across assets to take advantage of tax incentives. The magnitude of this response is mainly governed by the correlation between the returns on the two assets.

J.1 Model

Time t is continuous and households live for T periods. They can trade two assets indexed by $a \in \{1,2\}$ with returns given by

$$\frac{dP_{at}}{P_{at}} = \mu_a \left(1 - \tau_{\mu a}\right) dt + \sigma_a' dB_t,$$

where $\mu_a \left(1 - \tau_{\mu a}\right)$ is the after-tax expected return on asset a, σ_a' is a 2×1 diffusion vector, and dB_t is a 2×1 vector of independent Brownian motions.[60] Let C_t denote consumption at time t, θ_t a 2×1 vector of portfolio weights on the two assets, and W_T terminal wealth. Households maximize

$$\max_{\{C_t, \theta_t\}_{t=0}^{T}} \mathbb{E} \left[\int_0^T e^{-\rho t} \frac{C_t^{1-\gamma}}{1-\gamma} + e^{-\rho T} A^{\gamma} \frac{W_T^{1-\gamma}}{1-\gamma} \right]$$

[60] The tax rate $\tau_{\mu\alpha}$ should be interpreted as any tax rate that affects the expected return of an asset without affecting is volatility like, for instance, a tax rate on distributions made by ETFs, dividends paid by REITs, or distributions made by variable annuities, that are typically proportional to the starting value of the position.

subject to

$$dW_t = W_t \left[\theta_t' (\mu - r) + r - \tau_w\right] dt - C_t dt + W_t \theta_t' \Sigma dB_t,$$

where $\Sigma \equiv \begin{bmatrix} \sigma_1' \\ \sigma_2' \end{bmatrix}$ is a 2×2 matrix, $\mu \equiv \begin{bmatrix} \mu_1 (1 - \tau_{\mu 1}) \\ \mu_2 (1 - \tau_{\mu 2}) \end{bmatrix}$, r is a constant risk-free rate, and τ_w is a proportional tax on wealth.[61] The above problem admits a well-known analytical solution which, for completeness, is summarized in the following proposition.

Proposition. Optimal consumption and portfolio weights are given by

$$C_t^* = \frac{1}{f(t)} W_t, \tag{23}$$

$$\theta^* = \left(\Sigma' \Sigma\right)^{-1} \frac{\mu - r}{\gamma}, \tag{24}$$

where $\theta_t^* = \theta^*$ does not depend on time t and

$$f(t) = \frac{\kappa_2}{\kappa_1} e^{-\kappa_1 (T - t)} + \frac{1}{\kappa_1}, \tag{25}$$

$$\kappa_1 = \frac{\rho}{\gamma} - \frac{1 - \gamma}{\gamma} \left[\theta' (\mu - r) + r - \tau_w\right] + \frac{1}{2} (1 - \gamma) \theta' \Sigma \Sigma' \theta,$$

$$\kappa_2 = \kappa_1 A - 1.$$

Proof. Let $V(t, W)$ denote the value function. The Hamilton-Jacobi-Bellman equation is

$$\rho V = \max_{C, \theta} V_t + V_W \left\{W \left[\theta' (\mu - r) + r - \tau_w\right] - C\right\} + \frac{1}{2} V_{WW} W^2 \theta' \Sigma \Sigma' \theta + \frac{C^{1-\gamma}}{1 - \gamma}, \tag{26}$$

where I omitted the arguments of the value function $V(t, W)$ for simplicity and where I used V_x and V_{xx} to denote the first and second derivatives of V with respect to a generic argument x. The boundary condition is

$$V(T, W) = A^{\gamma} \frac{W^{1-\gamma}}{1 - \gamma}. \tag{27}$$

[61]The terminal condition $A^{\gamma} \frac{W_T^{1-\gamma}}{1-\gamma}$ in the household problem can be interpreted as a utility of bequests. Jakobsen et al. (2020) specify it as $A^{\alpha} \frac{W_T^{1-\alpha}}{1-\alpha}$, with α denoting the inverse of the bequest elasticity and A governing the intensity of the bequest motive. I restrict $\alpha = \gamma$ as it simplifies the analytical solution. In practice, Jakobsen et al. (2020) estimate that the elasticity of bequest and the elasticity of intertemporal substitution to be fairly similar in their data. As a consequence this restriction is likely to have minor implication for the purpose of the results discussed in Section J.2.

The first-order condition for θ implies

$$\theta_t = -\left(\Sigma\Sigma'\right)^{-1} \frac{V_W(t,W)}{WV_{WW}(t,W)} (\mu - r), \tag{28}$$

and the first-order condition for C implies

$$C_t = V_W(t,W)^{-\frac{1}{\gamma}}. \tag{29}$$

Guess that $V(t,W)$ satisfies

$$V(t,W) = f(t)^{\gamma} \frac{W^{1-\gamma}}{1-\gamma}. \tag{30}$$

Then

$$\begin{aligned} \theta_t &= \left(\Sigma\Sigma'\right)^{-1} \frac{\mu - r}{\gamma}, \\ C_t &= \frac{1}{f(t)} W_t. \end{aligned} \tag{31}$$

Using (30) and (31) in (26), it follows that $f(t)$ satisfies the following ODE

$$f'(t) = \kappa_1 f(t) - 1, \tag{32}$$

where

$$\kappa_1 = \frac{\rho}{\gamma} - \frac{1-\gamma}{\gamma}\left[\theta'(\mu - r) + r - \tau_w\right] + \frac{1}{2}(1-\gamma)\theta'\Sigma\Sigma'\theta.$$

Rewrite (32) as

$$\frac{\kappa_1 f'(t)}{\kappa_1 f(t) - 1} = \kappa_1,$$

so that

$$\frac{\partial \log(\kappa_1 f(t) - 1)}{\partial t} = \kappa_1.$$

Integrating both sides

$$f(t) = \frac{\kappa_2}{\kappa_1} e^{-\kappa_1(T-t)} + \frac{1}{\kappa_1},$$

for some constant κ_2 that is pinned down by the boundary condition in (27)

$$\kappa_2 = \kappa_1 A - 1$$

$\square$

Using this framework, I turn in the next section to simulate a change in the wealth tax rate τ and a change in the tax rates $\tau_{\mu 1}$, $\tau_{\mu 2}$ on the returns of the two risky assets.

J.2 Effect of tax changes

I rely on Jakobsen et al. (2020) (Section V.B) to calibrate all common parameters. The average elasticity of intertemporal substitution and elasticity of bequests across all their specifications is equal to 2.78. I then set $\gamma = 0.36$ to match the inverse of their average estimate. I set A to match their average estimate across specifications, which I find equal to 17.46, and $\rho = 0.036$ to match their average discount rate. I set the expected returns on the two assets μ_1 and μ_2 equal to their average rate return of 0.06. Finally, I set the wealth tax rate $\tau = 0.022$ and $T = 30$ years.

Turning to the parameters that are not available in Jakobsen et al. (2020), I calibrate the entries of the variance-covariance matrix Σ to match the volatility of annual returns earned by the ETF and mutual fund sectors (assets $a = 1$ and $a = 2$ respectively) between 2000 and 2020, as well as their correlation. This results in

$$\Sigma = \begin{bmatrix} 0.147 & 0.095 \\ 0.095 & 0.145 \end{bmatrix},$$

which implies a volatility of annual returns for ETFs equal to 0.175, a volatility of annual returns for mutual funds equal to 0.1733, and a correlation equal to 0.914. Finally, I set the tax rate $\tau_{\mu 1} = \tau_{\mu 2} = 0.11$ to match the average capital gain tax rate just before the 2012 American Taxpayer Relief Act was passed, as discussed in Section A.4.

Using this calibration, I simulate the model for thirty years at annual frequency (i.e. $dt = 1$), starting from $W_0 = 1$. At each time t, I use equation (23) to solve for optimal consumption C_t. I then find next-period wealth using

$$W_{t+1} = W_t \left[\theta' \left(\mu - r\right) + r - \tau_w\right] - C_t, \tag{33}$$

where θ is pinned down by equation (24). At each time t, I further obtain dollar holdings on both risky assets as $W_{1t} = \theta_1 W_t$ and $W_{2t} = \theta_2 W_t$.

I then proceed to evaluate how households in the model reallocate across assets in response to a change in $\tau_{\mu 2}$. Specifically, I consider an increase in $\tau_{\mu 2}$ from 0.11 to 0.132, where 0.132 is chosen to match the increase in average capital gain tax rates introduced by the 2012 American Taxpayer Relief Act. I assume that the increase only affects mutual funds (i.e., asset 2). For ETFs (i.e., asset 1), I keep $\tau_{\mu 1} = 0.11$. Using these new tax rates, I solve the model once more and I obtain a new time-series of dollar holdings $W_{1t}^{(\mu)}$ on asset 1. To isolate the behavioral effect on dollar holdings, I still use $\tau_{\mu 2} = 0.11$ in (33) when computing next-period wealth. This avoids that dollar holdings on asset 1 are higher simply due to a mechanical wealth effect.

The second line of Table 22 reports the behavioral response to the change in τ_{μ_2}, computed as $\frac{\log\left(W_{1t}^{(\mu)}/W_{1t}\right)}{\log(0.11/0.132)}$. I report results for t ranging from 1 to 5 years. I find the model-implied elasticities to be just above 2. These numbers should be compared with the estimated elasticities over two to four years discussed in Section 3.6, which are just around 1.6. Overall, although this framework was designed to match household response to changes in the wealth tax rate, it can already match quite closely the reallocation of wealth within the financial system when households can optimize taxation across different assets.

For completeness, I also evaluate the response to a change in the wealth tax rate from 0.022 to 0.01. I set the wealth tax rate $\tau = 0.01$ and I derive a new time-series of wealth $W_t^{(w)}$. I then compute the implied elasticity with respect to the keep rate $1 - \tau$ as $\frac{\log\left(W_t^{(w)}/W_t\right)}{\log(0.99/0.978)}$. This elasticity can be compared with the elasticities reported in Jakobsen et al. (2020) (Table II). Focusing on the same horizon of $t = 8$ years, I find the model-implied elasticity to be 6.062, in line with the estimated values that mostly vary between 5 and 9. The third line of Table 22 also reports the elasticity of wealth with respect to the wealth tax rate τ, computed over $t = 1$ to $t = 5$ years. Comparing the second and third lines, households appear more active when they can optimize taxation within the financial system and more reluctant to reduce wealth accumulation in response to changes in the wealth tax rate. That said, the gap between the two-elasticities decrease in the long term. At $t = 30$ years, the elasticity of W_{1t} with respect to $\tau_{\mu 2}$ is -1.517, while the elasticity of W_t with respect to τ is -0.225.

Table 22: **Model-implied elasticities**
This table summarizes the model-implied elasticities of W_{1t} with respect to the tax rate τ_{μ_2} and of W_t with respect to the wealth tax rate τ. Elasticities are reported for $t = 1$ to $t = 5$ years and for $t = 30$ years. For each t, the second line reports $\frac{\log\left(W_{1t}^{(\mu)}/W_{1t}\right)}{\log(0.11/0.132)}$ and the third line reports $\frac{\log\left(W_t^{(w)}/W_t\right)}{\log(0.01/0.022)}$.

Time horizon (t)	1	2	3	4	5	30
$\frac{\log\left(W_{1t}^{(\mu)}/W_{1t}\right)}{\log(0.11/0.132)}$	-2.069	-2.048	-2.028	-2.008	-1.987	-1.517
$\frac{\log\left(W_t^{(w)}/W_t\right)}{\log(0.01/0.022)}$	-0.0096	-0.019	-0.0287	-0.038	-0.048	-0.225