Private Capital Markets and Inequality*

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Abstract

This paper studies the relationship between the growth in private capital markets and the rise in economic inequalities over the last two decades in the U.S. First, we document that the share of financing raised by early-stage companies from U.S. high-net-worth individuals (HNWIs) tripled from 2004 to 2022. Second, exploiting both company- and state-level variation in exposure to the expanded federal capital gains tax exclusion on qualified small business stock (QSBS), we find that QSBS-eligible companies' probability of staying private increased by 3.6 percentage points, and that the average income gap between HNWIs and other income earners increased by 7.2%. Third, we show that this rise in income concentration appears to have been driven by HNWIs' excess returns on their early-stage investments relative to public stock market returns. Finally, using counterfactual simulations, we find that HNWIs' excess returns on these investments accounted for 28% of the growth in the top 0.5% wealth share between 2010 and 2022. Our paper has important insights in light of the current policy debate of democratizing private markets to retail investors.

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1 Introduction

Two important stylized facts have marked the last four decades in the U.S. On the one hand, the concentration of income and wealth has steadily risen (Piketty et al., 2018; Saez and Zucman, 2020; Smith et al., 2023; Auten and Splinter, 2024). On the other hand, private capital markets have expanded, while public stock market listings have fallen (Stulz, 2020; Ewens and Farre-Mensa, 2022). The aim of this paper is to examine whether high-net-worth individuals (HNWIs) have increased their participation in private capital markets and, if so, whether their growing presence in these markets could be behind these two key macroeconomic trends.[1]

The increasing participation of HNWIs in private capital markets could be related to both the growth of these markets and the rise in economic inequalities for two main reasons. First, only the wealthiest individuals are generally able to invest in private companies (Jensen et al., 2017; Mikhail, 2022), which leads private business wealth to be highly concentrated at the top of the income and wealth distribution (Kopczuk and Zwick, 2020). As a result, if the returns on private companies were also larger than the returns on public companies (Kartashova, 2014; Brown and Kaplan, 2019; Balloch and Richers, 2023), then, all else equal, income and wealth inequality would increase. Second, to the extent that HNWIs' investments represent a novel valuable source of financing for private companies, these companies may choose to stay private for longer—if not forever—, thus reinforcing the growth of private capital markets.

We focus on the U.S. for two main reasons. First, U.S. companies consistently account for about half of all the financing raised in global private capital markets (Lerner and Nanda, 2020). Second, the U.S. federal government introduced tax breaks after the 2008 financial crisis to incentivize HNWIs to invest in early-stage private companies (Polsky and Yale, 2023). These reforms represent a quasi-exogenous shock to HNWIs' participation in private capital markets, which we exploit to study how HNWIs' early-stage investments shaped both the decisions of private companies to stay private and the dynamics of inequality in the U.S.[2] To rationalize the effects on inequality, we further estimate the returns that HNWIs earned on their early-stage investments, comparing them to the returns that they would have instead earned in public stock markets or other alternative investments.

[1] By HNWIs, we refer to individuals who satisfy the U.S. Securities and Exchange Commission's (SEC) definition of accredited investors: those whose combined net worth with their spouse (excluding the value of their primary residence) exceeds $1 million, or whose combined (individual) income has exceeded $300,000 ($200,000) for the last two years. The SEC's website provides further details about this definition: https://www.sec.gov/resources-small-businesses/capital-raising-building-blocks/accredited-investors.

[2] Throughout this paper, we use "early-stage investments" interchangeably with "investments in startup companies." By these, we refer to investments in companies that Pitchbook categorizes as in either the pre-seed, seed, early, or late stage of their development.

To carry out our analyses, we mainly use data on private capital market activity from Pitchbook, which includes information on the financing received by private companies, the investors participating in each deal, and the changing valuation of each company across deals. For our analyses of inequality, we complement this information from Pitchbook with distributional wealth and income statistics from the Survey of Consumer Finances, the Internal Revenue Service (IRS) Statistics of Income (SOI Tax Stats), and the Forbes 400 rich lists.

We provide four sets of results. First, we show that U.S. HNWIs' participation in U.S. private capital markets has grown considerably in recent decades. This growth has been driven mainly by their investments in early-stage companies, which have increased from about $0.4 billion in 2004 to almost $15 billion in 2022, rather than by their investments in more mature private companies. Early-stage financing is the focus of the paper as this is the sub-asset class within private capital markets which seen growing participation by HNWIs. HNWIs' early-stage investments have increased not only in absolute but also in relative terms. In particular, the share of financing raised by U.S. startups from U.S. HNWIs tripled from about 2% to 6% over the 2004-2022 period.[3] We further document that the accumulated value of HNWIs' early-stage investments since 2004 reached almost $490 billion by 2022, which was more than double their counterfactual value had HNWIs instead invested in either the NASDAQ 100, S&P 500, or Russell 2000 public stock market index or in the Barclay Hedge Fund Index. HNWIs have thus, on average, earned excess returns on their early-stage investments relative to public stock markets or other alternative investments.[4]

Second, we study the relationship between the increase in HNWIs' early-stage investments and the probability that companies stay private. To do so, we exploit the expansion of an existing federal tax exclusion on long-term capital gains from the sale of qualified small business stock (QSBS). This tax exclusion is meant to incentivize HNWIs to invest in early-stage companies, since it applies only to QSBS purchased from companies that satisfy the following three conditions. First, the issuing company must be a U.S. C corporation; second, at least 80% of the company's assets must be actively used in qualified—mainly non-professional and non-extractive—trades and businesses; and, third, the company's gross assets must have never exceeded $50 million, inclusive of the financing raised from the investor purchasing the QSBS. At the exclusion's first introduction in 1993, investors

[3] Although HNWIs account for a relatively small share of overall early-stage financing, the growth in HNWIs' early-stage investments could still have important implications for economic inequalities, given that the ownership of private companies is highly concentrated at the top of the income and wealth distribution (Kopczuk and Zwick, 2020).

[4] We estimate the returns on each company based on the history of its valuation, as recorded by Pitchbook each time the company raises new financing.

who held QSBS for at least five years could exclude 50% of the first $10 million of the associated capital gains. However, in response to the 2008 financial crisis, the federal government temporarily expanded the exclusion rate to 75% in 2009, and to 100% in 2010. The 100% exclusion was made permanent eventually in 2015. Exploiting company-level variation in QSBS eligibility as part of a difference-in-differences design, we find that the QSBS reforms made QSBS-eligible companies 2.6 percentage points more likely to raise financing from U.S. HNWIs and 3.6 percentage points more likely to stay private post the reform relative to QSBS-ineligible companies. Third, we further exploit the QSBS reforms to establish a link between the increase in HNWIs' early-stage investments and the rise in U.S. income inequality over the last two decades. We follow a two-step approach, estimating the effects of the QSBS reforms on HNWIs' early-stage investments in the first step, and estimating the effects of these investments on income inequality in the second step. In both steps, we exploit state-level variation in exposure to the federal reforms, based on the number of HNWIs who resided in each state in 2008 (i.e., the year immediately prior to these reforms). To avoid additional regulatory burdens, the only individuals from which startups generally raise money are HNWIs. Thus, the reforms should increase HNWIs' investments more in states where the ex-ante number of resident HNWIs is higher. This setting has two main threats for identification. On the one hand, HNWIs may choose to settle in certain states specifically to get access to exclusive new investment opportunities (e.g., aspiring venture capitalists moving to California). On the other hand, resident HNWIs' early-stage investments may be biased toward local startups, which happens to be true in our setting. In this latter case, if the startups in states with more resident HNWIs are exposed to different economic shocks than those in states with fewer resident HNWIs, then startup investments by HNWIs residing in one state may grow faster or slower relative to those by HNWIs residing in another state, and for reasons entirely unrelated to the reforms.

In the first step of our state-level analysis, we therefore compare how the QSBS reforms affected resident HNWIs' in-state investments in early-stage companies relative to the investments in those same companies by other types of investors—namely, resident institutional investors, non-resident institutional investors, and non-resident HNWIs. This comparison allows us to control for interacted state-year fixed effects common to all investor types. We estimate that the expansion of the QSBS tax exclusion explained, on average, 21.4% of the overall growth in U.S.-based HNWIs' investments in early-stage companies between 2004-2008 and 2009-2022. In the second step, we use a similar state-level specification to the one that we use for investments, comparing how the QSBS reforms affected the average income gap between HNWIs and other income earners. We find that the increase in HNWIs' early-stage investments increased the average income gap between the top 0.5% and bottom 99.5% by 7.2% in the post-reform period.

Fourth, we document that HNWIs' excess returns on their early-stage investments (relative to public stock market returns) can be associated with the rise in U.S. income concentration over the last two decades. To that end, we first decompose our state-level income measures into three components: realized capital gains, other capital income, and labor income. We then show, using the same state-level design, that the rise in the average income gap between the top 0.5% and bottom 99.5% in the post-reform period was mainly due to a disproportionate rise in the capital gains of the top 0.5%. We further show that this rise in the average capital gains gap between the top 0.5% and the bottom 99.5% is strongly associated with an increase in HNWIs' excess returns from their early-stage investments. We also document, by means of counterfactual simulations, that these excess returns account for 28% of the overall nationwide growth in the top 0.5% shares of post-tax income and wealth, respectively, in the post-reform period. The effects are stronger for billionaires than millionaires. Finally, we show that the rise in economic inequalities can lead to a further increase in HNWIs' participation in private capital markets, suggesting the existence of a feedback loop between the two phenomena. Our paper has important insights in light of the current policy debate of democratizing private markets to retail investors and it's implications for inequality. If more individual investors have access to private capital markets, it can increase or decrease inequality depending on the deals they have access to and outperformance of private relative to public markets.

This paper contributes to three main strands of the literature. First, we contribute to the growing theoretical and empirical literature on the dynamics of income and wealth inequality, which—in addition to savings, bequests, risk-sharing, interest rates, and labor income—has emphasized asset prices and returns as important determinants of those dynamics (De Nardi, 2004; Jones, 2015; Saez and Zucman, 2016; De Nardi and Fella, 2017; Kuhn et al., 2017; Feiveson and Sabelhaus, 2018; Bach et al., 2020; Fagereng et al., 2020; Cioffi, 2021; Greenwald et al., 2021; Hubmer et al., 2021; Irie, 2024; Mian et al., 2021; Xavier, 2021; Bauluz et al., 2022; Meeuwis, 2022; Andersen et al., 2023; Blanchet and Martínez-Toledano, 2023; Martínez-Toledano, 2023; Nekoei and Seim, 2023; Gomez and Gouin-Bonenfant, 2024; Gomez, 2025). While confirming the importance of return heterogeneity as one such determinant, we also identify a new channel to explain it—namely, the differences across the income and wealth distribution in individuals' access to and participation in private capital markets.

In this way, we further contribute to the separate literature that focuses on measuring the returns to different asset classes and providing explanations for the heterogeneity in those returns across investors. Several theoretical papers have suggested that such heterogeneity can be driven by differences in entrepreneurial ability (Lucas, 1978), information (Peress, 2004), or sophistication (Kacperczyk et al., 2019). Recent empirical studies have instead emphasized the return heterogeneity within a particular asset class, including household

wealth (Bach et al., 2020; Fagereng et al., 2020; Xavier, 2021; Balloch and Richers, 2023), bank deposits (Deuflhard et al., 2019), stocks (Calvet and Fisher, 2007; Campbell et al., 2019), and retirement accounts (Bretscher et al., 2021). Our paper is most closely related to prior and contemporaneous studies examining differences in returns between public and private companies (Moskowitz and Vissing-Jørgensen, 2002; Kartashova, 2014; Brown and Kaplan, 2019; Brown et al., 2021; Balloch et al., 2025), most of which have focused on documenting either the under- or outperformance of buyout funds or overall private business wealth. We instead put the focus on early-stage markets and document HNWIs' recent outperformance in these markets relative to public stock markets.

Finally, we contribute to the literature on investors in private capital markets. Most existing studies have focused on institutional investors like pension funds and endowment plans (Lerner and Schoar, 2004; Lerner et al., 2007; Sørensen, 2007; Robinson and Sensoy, 2013; Maurin et al., 2022; Mittal, 2024). Following the recent literature's growing interest in angel investors (Lindsey and Stein, 2020; Bach et al., 2022; Karlsen et al., 2023; Canipek, 2024), we focus on HNWIs' participation in private capital markets, especially on their investments in early-stage companies. Though other papers have also studied the effects of tax breaks that incentivize investments in early-stage companies (Edwards and Todtenhaupt, 2020; Denes et al., 2023; Chen and Farre-Mensa, 2024; Campello and Junqueira, 2025; Fairlie et al., 2025), we are, to the best of our knowledge, the first to do so with the aim of understanding the implications of the increasing participation of HNWIs in private capital markets for income and wealth inequality.

The rest of this paper is organized as follows. Section 2 first describes the data that we use, while Section 3 documents key stylized facts. Section 4 then analyzes the effects of the QSBS reforms on HNWIs' early-stage investments, companies' decisions to stay private, and income inequality. Section 5 studies the implications of these investments for wealth inequality by means of counterfactual simulations. Section 6 documents the existence of a feedback loop between the recent rise in private capital markets and economic inequalities. Finally, Section 7 concludes.

2 Data

This section describes the different datasets that we use and our respective validations of them. Section 2.1 first describes how we use data on private capital market activity from Pitchbook to identify high-net-worth individuals' investments in private capital market deals and calculate the returns that they earned on them. Section 2.2 then describes how we use data on inequality from various sources to identify the number of HNWIs residing

in each U.S. state as well as construct the income and wealth distributions.

2.1 Private Capital Markets

Pitchbook. Our main source for data on private capital market activity is Pitchbook, a commercial data provider which collects data on the deals through which companies raise financing and the investors in these deals. For the period from 2004 to 2022, Pitchbook's data contains information on 455,926 deals for 208,436 U.S. companies, corresponding to 3,181,224 investments by 129,578 investors. Pitchbook collects this data from various sources, including press releases and regulatory filings by companies, Freedom of Information Act requests to public pension funds, and voluntary requests to the general and limited partners of private investment funds (Cumming and Monteiro, 2023).[5]

First, we use the data from Pitchbook to identify investors's investments in private capital market deals, including both investments directly into companies and those intermediated by private investment funds. We classify private capital market deals into four categories: early stage (equity investments in startups), private equity (equity investments in more mature companies), private debt (debt investments by non-bank lenders, other than in the form of bonds), and real asset (acquisitions of real estate, infrastructure, or natural resources).[6] We also classify investors into two categories: high-net-worth individuals (individuals, angel groups, and family offices) and institutional investors (pension funds, endowment plans, insurance companies, and other investors). Since we observe the exact amounts invested by only some of the investors in each deal, we distribute the remainder equally across all the other investors, similarly distributing the investments intermediated by each fund across all its limited partners. Appendix A contains further details on our procedure to clean Pitchbook's data on private capital market activity.[7]

[5] For further details, see Pitchbook's website: https://www.pitchbook.com/research-process.

[6] We follow the categorization used by private investment professionals (see https://www.preqin.com/academy/lesson-2-private-capital/what-is-private-capital). We prefer "early-stage" to "venture capital," since the latter usually refers only to intermediated investments in startups.

[7] Investments in private capital market deals are difficult to systematically observe in the U.S. because, to the best of our knowledge, there is no comprehensive administrative dataset on them. We choose to rely on Pitchbook's data since it is the National Venture Capital Association's preferred source of information on private capital market activity (see https://www.nvca.org/document-category/nvca-yearbook). We show in Appendix Table A1 that Pitchbook has better coverage of private capital market activity—especially of high-net-worth individuals' investments, which is the focus of this paper—than Preqin, an alternative data provider (see also Pitchbook's website: https://www.pitchbook.com/compare/pitchbook-vs-preqin). As a further validation, we compare Pitchbook's coverage of angel investments in U.S. companies to that of the other alternative data providers (Crunchbase, VentureXpert, and VentureSource) used by Denes et al. (2023), based on a definition of angel investments similar to theirs (i.e., early-stage investments that were made as part of capitalization, angel, crowdfunding, accelerator/incubator, or seed deals or that were made by individuals, angel groups, or accelerators/incubators). For the period from 1988 to 2018, Denes et al. (2023) identify 206,885 angel investments in their data, while we identify 249,898 in ours.

Second, we use the data from Pitchbook on companies' valuations to calculate the returns that investors earned on their investments.[8] Although returns are often calculated at the fund level (Korteweg and Nagel, 2025), we instead calculate investment-level returns, given that 82% of the early-stage investments made by U.S. high-net-worth individuals in U.S companies from 2004 to 2022 that we observe were direct rather than intermediated (see Appendix Table A2). We take a similar approach to that of Korteweg and Sorensen (2010), using information on the changing valuation of each company across deals to calculate the rate of return yielded by its equity. Specifically, we compare the company's new pre-money valuation (before accounting for the equity financing that it raised as part of its new deal) to its previous post-money valuation (after accounting for the equity financing that it raised as part of its previous deal). This comparison accounts for any dilution of existing investors' shares between deals, differentiating between valuation growth due to new equity financing and that due to organic growth. In this way, we first calculate the history of the rate rate of return yielded by each company's equity. We then calculate the return yielded by each investment in each year, allowing the value of the investment to evolve over time according to the company's return history.[9] Since we do not observe each company's valuation as part of each of its deals, we impute its missing valuations based on the evolution of similar companies' valuations. Similarly, for each year in which the company did not raise financing, we impute its missing valuation based on the dynamic selection model described in Korteweg and Sorensen (2010). The model yields estimates of these missing valuations that account for selection bias—namely, the fact that better-performing companies are more likely to raise financing and therefore report valuations that we can observe. Appendix B contains a detailed description of this imputation procedure, as well as of other aspects of our procedure to clean Pitchbook's data on valuations and our return methodology.

Finally, we also use the data from Pitchbook to distinguish between eligible and ineligible issuers of QSBS and to thereby evaluate the effects of the QSBS reforms on high-net-worth individuals' early-stage investments and companies. We infer each U.S. company's QSBS eligibility—that is, whether it was a C corporation, was active primarily in a qualified trade or business, and had no more than $50 million in gross assets—based on its legal name, primary industry code, and financing history. We also identify the years during which it was an active private company using information on when it was founded and when it first went bankrupt, became publicly listed, or was acquired by another company. Appendix C contains further details about our procedure to determine QSBS eligibility.

[8] Reassuringly, the evolution of private business wealth in the U.S. based on Pitchbook is broadly in line with its evolution based on alternative data sources (see Appendix Figure B1).

[9] As a further validation, Appendix Figure B2 shows that our calculated returns for U.S. venture capital funds based on Pitchbook closely resemble those of the Cambridge Associates U.S. Venture Capital Index.

2.2 High-Net-Worth Individuals and Inequality

Geographic Wealth Inequality Database. To conduct the state-level regression analyses, we require a measure of the total number of HNWIs residing in each U.S. state. For that, we rely on the Geographic Wealth Inequality Database (GEOWEALTH-US) built by Suss et al. (2024). This database provides estimates of the number of HNWIs residing in each state in every year from 2005 to 2022. To obtain these series, the authors first estimate the relationship between wealth and other observable characteristics on the sample of individuals who appear in the Survey of Consumer Finances (SCF). They then predict the wealth of individuals sampled in U.S. population surveys, in which those same characteristics—other than wealth—are also observable.

Based on observable income and estimated wealth, Suss et al. (2024) define HNWIs to resemble the U.S. Securities and Exchanges Commission's (SEC) definition of accredited investors.[10] In the view of the SEC, only accredited investors are sophisticated enough to invest in unregistered securities like the QSBS issued by early-stage private companies. Private investment funds and companies that raise financing from non-accredited investors are therefore required to register their securities with the SEC.[11]

In Appendix D.1, we validate the number of accredited investors residing in each state as measured in GEOWEALTH-US with alternative estimates from Phoenix Marketing International/MarketCast Wealth and Affluent Monitor, Forbes 400 rich lists, Credit Suisse, and the Survey of Consumer Finances (SCF). Our baseline measure based on GEOWEALTH-US appears to be consistent with these alternative sources, both across states and over time.

IRS Statistics of Income. To conduct the state-level inequality analyses, we build state-level income inequality series based on the personal income tax statistics from the Statistics of Income (SOI Tax Stats) database provided by the U.S. Internal Revenue Service (IRS). Specifically, we use the historical data tables that provide information on a range of personal income tax items, which are aggregated by state and adjusted gross income (AGI) bracket for each year from 2004 to 2022. AGI refers to income from all sources, including labor income, investment income, business profits, and retirement

[10] For this definition, see footnote 1. Suss et al. (2024) estimate the wealth of individuals who appear in cross-sectional rather than longitudinal population surveys. As a result, they only consider whether an individual's household income exceeds $300,000 in the current year for the income test. Furthermore, they consider the individual's wealth excluding the value of their primary residence for the wealth test.

[11] Private funds and companies can raise financing from up to 35 non-accredited investors before triggering SEC registration: https://www.sec.gov/resources-small-businesses/exempt-offerings/private-placements-rule-506b. In practice, however, this number is so low—and the amount that can be raised from them so limited—that non-accredited investors have generally been excluded from private capital markets; the only exception happens when private companies raise financing via crowdfunding (Jensen et al., 2017).

income, adjusted for tax deductions.

We apply the method of generalized Pareto interpolation (GPI) developed by Blanchet et al. (2022). GPI is a non-parametric approach that avoids the assumptions of a Pareto approximation, which are often violated by empirical data. For every state in each year, we construct the state-level income distribution across individuals using data on IRS tax filing units, assuming that the reported household income of couples filing jointly is shared equally between spouses. Our series are consistent with those of Sommeiller and Price (2018), who build state-level income inequality series for the U.S. using personal income tax tabulations from 1917 to 2015 (Appendix Figure D1). Using the information available on the composition of income for each tax bracket, we further decompose the aggregate income in each income group into labor income, realized capital gains, and capital income (i.e., dividends, interest income, and other income from investments).

We also aggregate our state-level income inequality series at the national level and build nationwide post-tax income distribution series to implement the counterfactual simulations of U.S. income inequality described in Appendix Section D.2.2.[12]

Survey of Consumer Finances. We also construct a series for the nationwide wealth distribution to implement the counterfactual simulations of U.S. wealth inequality in Section 5.2. For that, we rely on the Survey of Consumer Finances (SCF), which provides a representative picture of the structure of the incomes, assets, and debts of U.S. households. The SCF oversamples individuals at the top of the wealth distribution, enabling a more accurate measurement of the wealth of the wealthiest individuals. The survey is updated every three years and is available between 1989 and 2022. We build the wealth distribution for every wave of the survey between 2004 and 2022 using a measure of net wealth—that is, the sum of private business wealth, public equity, real estate, interest-earning assets, and other financial and non-financial assets, minus all liabilities.[13] To complement our analysis of HNWIs' returns on early-stage investments based on Pitchbook, we also rely on the SCF to compute returns on both private and public equity over the 2004-2022 period using the methodology developed by Moskowitz and Vissing-Jørgensen (2002) and Kartashova (2014).

[12] Appendix D.2.1 explains in more detail the methodology used to build the state- and federal-level pre- and post-tax income distribution series.

[13] Appendix D.3.1 explains in more detail the methodology used to build the wealth distribution series.

3 Descriptive Evidence

This section presents descriptive evidence on private capital market activity in the U.S. Section 3.1 documents the increasing participation of U.S. high-net-worth individuals in private capital markets. Section 3.2 characterizes the returns that they earned on their early-stage investments, benchmarking them with respect to public stock market returns.

3.1 HNWIs' Increasing Participation in Private Capital Markets

We first describe the evolution of private capital market activity in the U.S.[14] Figure 1a shows that the total amount of financing raised by U.S. companies from private capital markets grew from $187 billion in 2004 to $645 billion in 2007. This growth was mainly driven by private equity, private debt, and real asset deals, rather than by early-stage deals. With the onset of the 2008 global financial crisis, U.S. private capital market activity collapsed, falling below its 2004 level by 2009. However, it quickly recovered starting in 2010, alongside the broader economic recovery from the crisis. The post-crisis period was marked by the faster growth of early-stage deals relative to that of private equity, private debt, and real asset deals. By the second half of the 2010s, U.S. startups were raising $100-168 billion annually, compared to only $22-42 billion during the 2000s. This trend accelerated during the COVID-19 pandemic, with the total amount of early-stage financing raised by U.S. companies peaking in 2021, before the tightening of U.S. monetary policy dampened startup activity in 2022 (Ma and Zimmermann, 2023; Abreu et al., 2025).[15]

The post-crisis growth in early-stage financing was partly driven by the increasing participation of high-net-worth individuals in these markets. Figure 1b shows that the total amount invested annually by U.S. HNWIs in U.S. startups was relatively stable from 2004 to 2008. However, it grew from $1 billion in 2008 to $25 billion in 2021, before falling to $15 billion in 2022 amid the broader decline in U.S. private capital market activity (see Figure 1a). HNWIs barely increased their investments in private equity, private debt, or real asset deals over the same period.[16] Thus, early-stage investments were the fastest-growing asset class in the private capital portfolios of HNWIs. The increase in

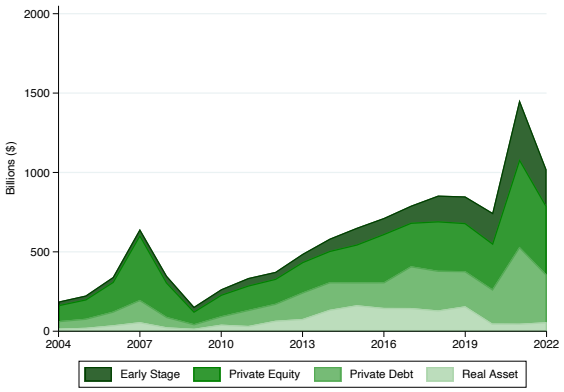
[14] We focus on U.S. companies since they have consistently accounted for about half of the financing raised in private capital markets globally (see Appendix Figure A2).

[15] U.S. private capital market activity has grown not only in absolute terms but also relative to the size of the U.S. economy. Appendix Figure A3 shows that investments in U.S. private capital market deals as a share of U.S. gross domestic product have increased from 1.5% in 2004 to 3.8% in 2022.

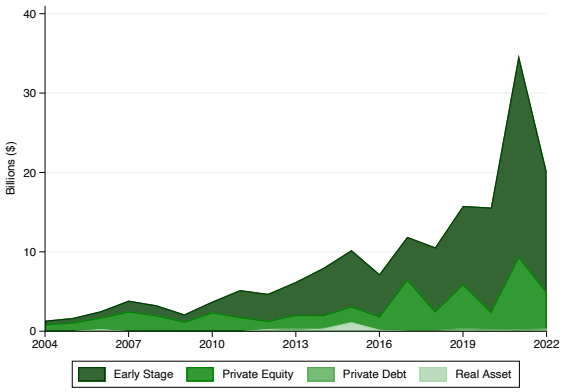
[16] Appendix Figure A4 shows that the early-stage share increased even among HNWIs' intermediated investments in private capital market deals. Nevertheless, private equity remained the largest component of HNWIs' intermediated investments, consistent with the findings of Balloch and Richers (2023).

Figure 1: Investments in U.S. Private Capital Market Deals

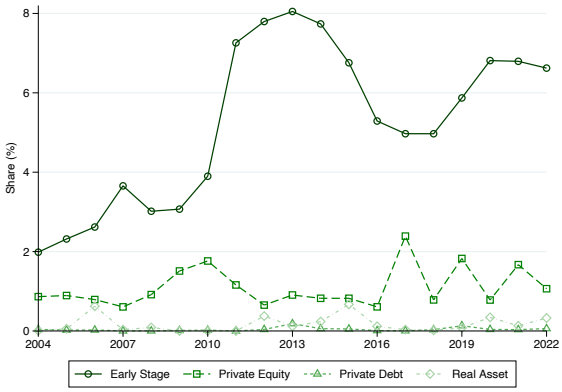
(a) Investments by All Investors



(b) Investments by U.S. HNWIs



(c) U.S. HNWIs' Share of Investments



Source: Pitchbook.
Notes: This figure describes the evolution of investments in private capital market deals for U.S. companies from 2004 to 2022. Panel (a) plots the investments by all investors, Panel (b) plots the investments by U.S. high-net-worth individuals, and Panel (c) plots U.S. HNWIs' share of the investments. The values in Panels (a) and (b) are expressed in nominal terms. Panels (b) and (c) exclude 5 private equity investments by U.S. HNWIs for which the amount invested per investment exceeded $1 billion, which we therefore consider outliers.

HNWIs' early-stage investments was driven primarily by the entry of new participants in early-stage capital markets (Appendix Figure A5a), who accounted for the vast majority of these investments after 2008 (Appendix Figure A5b).[17] Despite this sharp increase in their participation, the sector composition of HNWIs' early-stage investments remained remarkably stable throughout the period (Appendix Figure A7).

Crucially, the growth in HNWIs' early-stage investments outpaced the overall growth of early-stage capital markets. Figure 1c shows that U.S. HNWIs accounted for only 2% of the total amount of financing raised by early-stage U.S. companies in 2004. However, this share spiked to 8% by 2013, before settling at 6-7% during the early 2020s. HNWIs have therefore emerged as a new and important source of financing for U.S. startups. Even though HNWIs' share of total early-stage financing remains relatively small compared to that of institutional investors, private business wealth is distributed highly unequally across households and accounts for a large share of the portfolio of the wealthy (Appendix Figure A8). Thus, a drastic increase in the private business wealth of the wealthy could significantly contribute to a rise in economic inequalities if it consistently yields higher returns than other more widely held forms of wealth. In Sections 4 and 5, we formally explore the link between the increasing participation of HNWIs in private capital markets and the rise in U.S. economic inequalities between 2004 and 2022. We hereafter focus on only early-stage investments, since this is the asset class whose growth has primarily driven the increasing participation of HNWIs in private capital markets.

3.2 HNWIs' Excess Returns on Early-Stage Investments

We next present descriptive evidence on the returns that HNWIs earned on their early-stage investments, as well as the counterfactual returns that they would have earned had they instead invested in public stock markets. As briefly described in Section 2.1 and detailed in Appendix B, we follow a similar approach to Korteweg and Sorensen (2010) and calculate returns by comparing a company's pre-money valuation as part of each new deal to its post-money valuation as part of its previous deal. In our baseline analysis, we consider the valuations and bankruptcies that we observe in the data, as well as the valuations that we compute for companies missing them. Our imputation procedure is based on a regression of the log post-money valuation of each company on year fixed effects interacted with its observable characteristics, namely, the company's primary sector and the number of financing rounds that it previously raised. We use the estimated parameters from this

[17] To ensure that the increase that we find in HNWIs' participation in early-stage capital markets was not merely driven by improvements over time in Pitchbook's data coverage, we corroborate this trend using the Survey of Consumer Finances. Appendix Figure A6 shows that the number of U.S. accredited investors who report having private business wealth more than doubled from 2010 to 2022.

regression to predict the missing valuations. This imputation procedure ensures that we can calculate returns on every early-stage investment by HNWIs, rather than only for those investments in companies whose valuation we observe at least twice. Moreover, it makes the sample of valuations based on which we calculate returns more representative (along observable characteristics) of the companies for which we observe HNWIs' investments.

Figure 2a plots the accumulated end-of-year value of U.S. HNWIs' early-stage investments in U.S. companies from 2004 to 2022 (i.e., the sum of their accumulated initial amounts invested with their accumulated returns). It also plots the accumulated value of their counterfactual investments in various public stock market indices, including the NASDAQ 100, S&P 500, and Russell 2000. By 2022, the accumulated value of HNWIs' early-stage investments was more than double that of their counterfactual investments in any of the indices. HNWIs thus earned excess returns on their early-stage investments, relative to the returns that were available in public stock markets. These excess returns are robust to the use of the subsample of early-stage investments in companies for which we only observe, rather than impute, valuations, as well as alternative samples of valuations, based on either applying haircuts to "unicorn" valuations that exceed $1 billion but are likely overvalued (Gornall and Strebulaev, 2020; Gahng, 2023) or imputing bankruptcies for companies that appear to be inactive (Appendix Figure B2a). The excess returns are also robust to the use of alternative counterfactuals, such as the FANG+ index of high-growth technology companies or the Barclay Hedge Fund Index (Appendix Figure B2b).

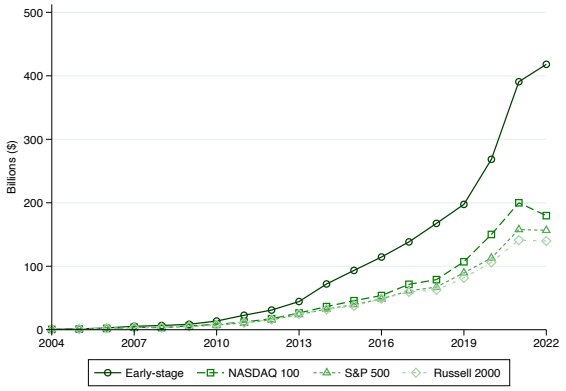
To explain the divergence between the accumulated value of HNWIs' early-stage investments and that of their counterfactual investments, Figure 2b plots the average annual rate of return on both sets of investments. We calculate this average rate as their total value to paid-in capital ratio (TVPI) minus one over each 1-year horizon, comparing the aggregated end-of-year value of HNWIs' investments to their aggregated start-of-year value.[18] In 15 of the 19 years from 2004 to 2022, HNWIs' average rate of return on their early-stage investments exceeded the rate that they would have earned on their counterfactual investments in any of the public indices considered. Consistent with this result, Appendix Figure B3b documents that the returns on private business equity also exceeded those on public equity from 2004 to 2022, based on the methodology using the SCF developed by Moskowitz and Vissing-Jørgensen (2002) and Kartashova (2014).

These average excess returns mask substantial return heterogeneity across HNWIs. Figure

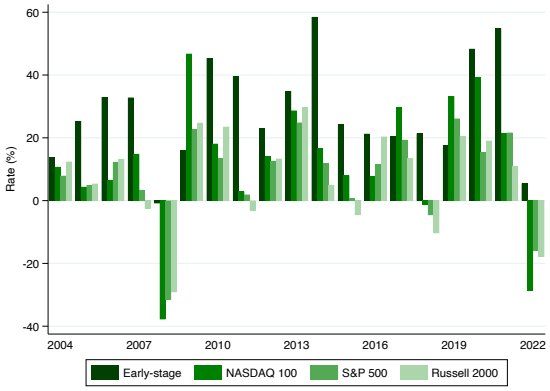
[18] Following Phalippou (2025), Appendix Figure B3a instead plots the internal rate of return (IRR) on HNWIs' pooled investments over each 1-year horizon. This calculation accounts for the fact that HNWIs enter and exit investments at different dates over the course of each year, whereas the TVPI minus one (i.e., the start-of-year value-weighted average of investment-specific annual rates of return) does not. Relative to the TVPI minus one, the IRR simply amplifies any differences between HNWIs' average annual rates of return on their early-stage and counterfactual investments, since, unlike the TVPI minus one, the IRR is an annualized rate.

Figure 2: U.S. HNWIs' Returns on Early-Stage Investments in U.S. Companies

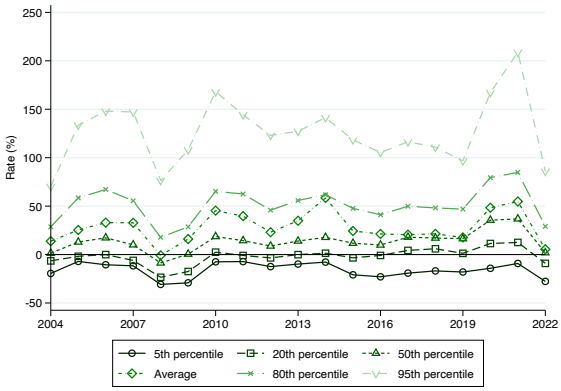
(a) Accumulated value by end of year



(b) Average annual rate of return



(c) Distribution of annual rates of return



Source: Pitchbook, S&P Global.
Notes: This figure describes the returns that U.S. HNWIs earned on their early-stage investments in U.S. companies from 2004 to 2022, as well as the counterfactual returns that they would have earned had they instead invested in public stock markets. Panel (a) compares the accumulated end-of-year value of HNWIs' early-stage investments with counterfactual scenarios where they had instead invested in the NASDAQ 100, S&P 500, or Russell 2000 public stock market index. Panel (b) plots the average annual rate of return that HNWIs earned (would have earned) across their early-stage (counterfactual) investments, calculated as their total value to paid-in capital ratio (TVPI) minus one over a 1-year horizon. Panel (c) plots various percentiles of the distribution across investors of their annual rates of return on their eary-stage investments. The values in Panel (a) are expressed in nominal terms.

2c plots the 5th, 20th, 50th, 80th, and 95th percentiles of the distribution of annual rates of return across HNWIs between 2004 and 2022. Whereas the investor at the 5th percentile consistently lost money, the investor at the 95th percentile consistently more than doubled the start-of-year value of their early-stage portfolio, suggesting that the distribution of returns was highly right-skewed.[19] Appendix Figures B5 and B6 document additional return heterogeneities, showing, among other interesting results, that HNWIs consistently earned more on their direct early-stage investments than on their intermediated ones (Appendix Figure B5a), and that they consistently outperformed U.S. pension funds' early-stage investments in this regard (Appendix Figures B6a and B6b). In Section 5, we formally explore whether these excess returns could be behind the recent rise of economic inequalities in the U.S. and quantify its importance.

HNWIs' excess returns on their early-stage investments could be driven by compensation for systematic risk or by outperformance on their investments relative to public markets on a risk-adjusted basis. To better understand the source of these excess returns, we calculate risk-adjusted returns using the dynamic selection model (DSM) developed by Korteweg and Sorensen (2010). They jointly estimate a model featuring two equations: a valuation equation, which regresses company-level (log) returns on the returns to various market factors, and a selection equation, which regresses a latent selection variable on the (log) valuation and the time since the company's previous financing round. The main aim of their methodology is to correct for sample selection, as valuations of early-stage companies are observed occasionally, and expected to be observed more frequently for well-performing companies. We first replicate Korteweg and Sorensen (2010)'s estimates for their period of analysis, i.e., 1987-2005, using our data from Pitchbook. Appendix Table B1 shows that our estimates of β_M—which measures riskiness of investments relative to the market—and α—which captures performance relative to the market after accounting for risk—are similar to those obtained by Korteweg and Sorensen (2010). They find a β_M of 2.75 for 1987-2005, while with our sample of deals we find a β_M of 2.25. We show a pattern of β_M estimates across different sub periods consistent with that of Korteweg and Sorensen (2010), i.e., a β_M of 2.75 in 1994-2000 and 1.54 in 2001-2005, while their β_M estimates are 2.50, and 1.09 respectively with very few deals and high standard errors in the first sub period. We find an α of 4.3% over 1987-2005, while Korteweg and Sorensen (2010) find 3.3%. For comparison, we show the DSM estimates along with estimates using the OLS and GLS procedures, which reveals selection affects magnitude of estimates.

[19] Appendix Figure B4a shows that the variance of the distribution of rates of return across investments far exceeded that of the same distribution across investors, suggesting that investing in multiple early-stage companies helped HNWIs hedge their investment-specific risks. Appendix Figure B4b further shows that, more so than the median early-stage investor, the median early-stage investment by HNWIs earned close to zero returns, consistent with the findings of Karlsen et al. (2023) and Stanley and Øvrum (2023).

Having re-estimated β(s) and α(s) in the earlier time periods following existing papers lends credibility to focus on our setting of early-stage companies invested in by high-net-worth individuals for our period of analysis (2004-2022). The only modification that we make to the estimation procedure is to weight each company by the amount of early-stage financing they raise from different groups of investors to account for the skewness in the data. To understand if the deals invested in by HNWIs yield different returns relative to other deals, we report α and β estimates (Table 1) from the DSM model using three different sets of deals: early-stage U.S. companies raising financing from 1) all investors (referred to as "All deals"), 2) at least one HNWI (referred to as "HNWI deals"), 3) all investors minus HNWI deals.

Table 1: Risk Adjusted Returns using Dynamic Section Model (DSM) for 2004-2022

	All Deals		HNWIs Deals		All Minus HNWIs Deals	
	One-Factor (1)	Three-Factor (2)	One-Factor (3)	Three-Factor (4)	One-Factor (5)	Three-Factor (6)
β_M	1.760***	1.738***	1.967***	1.816***	1.762***	1.831***
	(0.072)	(0.076)	(0.148)	(0.108)	(0.070)	(0.106)
β_{SMB}		-0.141		-0.271		-0.187*
		(0.135)		(0.199)		(0.113)
β_{HML}		-0.131*		0.174**		-0.1369
		(0.072)		(0.081)		(0.101)
α	0.069***	0.069***	0.080***	0.081***	0.067***	0.068***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)

Notes: The table generates DSM β and α estimates following (Korteweg and Sorensen, 2010) for the one-factor "RMRF" model and Fama-French three-factor model. σ is the estimated standard deviation of the error term. Columns (1) and (2) correspond to all early-stage U.S. company deals in our sample from 2004-2022. Columns (3) and (4) focus on the subset of deals with at least one HNWI investor. Columns (5) and (6) focus on the deals which are the difference between Columns (1)-(2) and Columns (3)-(4). We use valuations on the top 2,500 companies for each of the estimations for computational efficiency, which account for 61% of total financing raised by all investors (Column (1)-(2)), 75% of total financing raised by U.S. HNWIs (Column (3)-(4)), and 62% of total financing raised by other investors (Column (5)-(6)). ***, **, and * denotes whether zero is contained in the 1%, 5%, and 10% confidence intervals, respectively.

Table 1 shows that β_M ranges from 1.97 (one-factor model) to 1.82 (three-factor model) for 2004-2022 for early-stage U.S. company deals which have at least one HNWI investor.[20] These estimates suggest that HNWIs' early-stage investments tend to be riskier than the market, indicating that part of the excess returns we document are explained by compensation for systematic risk. Columns (5)-(6) show that β_M for the market ranges from 1.76 to 1.83 over the 2004-2022 period for deals financed by all other investors. The

[20] We estimate both one-factor and three-factor models. In the former, the only factor is the market return over the risk-free rate, In the latter, we also add the average return gap between and small and large firms, as well as between high and low book-to-market value.

estimated monthly α for the HNWIs sample ranges from 8.0% to 8.1%, which shows that the compensation for excess returns for HNWIs is over and above compensation for risk.[21] This positive and significant alpha could reflect access to good deals, skill, good advisors or investor-specific guidance and support. For comparison, the monthly α estimate for all the deals financed in this period is 6.9%, similar to that of the other investor sample, which ranges from 6.7% to 6.8%. Given that HNWIs obtain on average a higher α relative to all other investors, this suggests that HNWIs appear to invest in better-performing companies. In Section 6, we further explore if skill could potentially be behind the higher alpha among HNWIs by tracking follow-on deals of HNWIs. However, we cannot completely disentangle the source of the positive α and relegate that for future research.

4 QSBS Reforms' Effects on Investments and Inequality

This section exploits the expansion of the U.S. federal capital gains tax exclusion on qualified small business stock (QSBS) in the aftermath of the 2008 global financial crisis as a quasi-exogenous shock to HNWIs' participation in private capital markets. Since these reforms incentivized HNWIs to invest in early-stage companies, we can evaluate the reforms' effects on HNWIs' early-stage investments and, thereby, the implications of these investments for inequality. Section 4.1 first describes the institutional setting of the QSBS capital gains tax exclusion. Section 4.2 then analyzes the QSBS reforms' effects on U.S. HNWIs' early-stage investments and on the U.S. companies in which they invested; to do so, we exploit variation in exposure to the reforms across companies (in terms of their eligibility as issuers of QSBS) and across states (in terms of the number of HNWIs residing in them). Finally, Section 4.3 explores the implications of HNWIs' increased participation in private capital markets for state-level income inequality.

4.1 QSBS Capital Gains Tax Exclusion

The QSBS capital gains tax exclusion was first introduced by the U.S. federal government in 1993. Set forth in Section 1202 of the Internal Revenue Code, it is a personal income tax exclusion on the capital gains realized from the sale of QSBS. The maximum gain eligible for the exclusion is generally limited to $10 million per issuer.[22] For an investor to qualify

[21] Appendix Table B2 shows that the risk-adjusted returns are similar in the post-reform 2009-2022 period.

[22] More precisely, the maximum eligible gain per issuer per year is limited to the greater of (1) $10 million minus the gains previously used for that issuer and (2) 10 times the basis of the QSBS from that issuer

for the exclusion, they need to have held the QSBS for at least five years. For a company to qualify as an eligible issuer of QSBS, it needs to meet the following three requirements: (1) it must be an active business classified as a U.S. C corporation (and therefore taxed separately from its owners); (2) it must have had gross assets of $50 million or less at all times before and immediately after the QSBS was issued; and (3) at least 80% of the company's assets must be used in the active conduct of a qualified trade or business.[23] Companies satisfying these requirements tend to be startups in high-growth sectors (e.g., information technology) attractive to early-stage investors. Indeed, the exemption was explicitly designed to incentivize investments in such startups (Polsky and Yale, 2023).[24]

Although the QSBS capital gains tax exclusion has been in place since 1993, it was only in the aftermath of the 2008 global financial crisis that it became attractive from a tax savings perspective.[25] Figure 3a shows that, from 2004 to 2008, 50% of the first $10 million in capital gains realized from the sale of QSBS was *expected* to be excludable from the federal long-term capital gains tax, while the remaining 50% of gains was expected to be taxed at a fixed 28% rate (i.e., the federal long-term capital gains tax rate at the time of the exclusion's original introduction in 1993).[26] Nevertheless, since the federal long-term capital gains tax rate on capital gains from other investments was itself 15%, the expected federal tax wedge on QSBS capital gains—measured as the difference (in percentage points) between the federal long-term capital gains tax rate and the tax rate on QSBS capital gains—was negligible, as shown in Figure 3b. Investors therefore had little to no incentive to favor QSBS investments over other investments.

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sold in that year. However, the cumulative limit of $10 million per issuer is likely to be the relevant one in practice, since the amount invested in early-stage investments is often small relative to the gain.

[23] Disqualified trades and businesses are determined by the I.R.S. and include companies that: (1) perform services related to health, law, engineering, architecture, accounting, actuarial science, performing arts, consulting, athletics, finance, banking, insurance, leasing, investing, or brokerage; (2) rely on an employee or owner's reputation (i.e., if it endorses products or services, uses an individual's image, or has an employee make appearances at events or on media outlets); (3) produce products, such as fossil fuels, for which percentage depletion (a type of tax deduction) can be claimed; (4) operate a hotel, motel, restaurant, or similar business; or (5) are farming businesses.

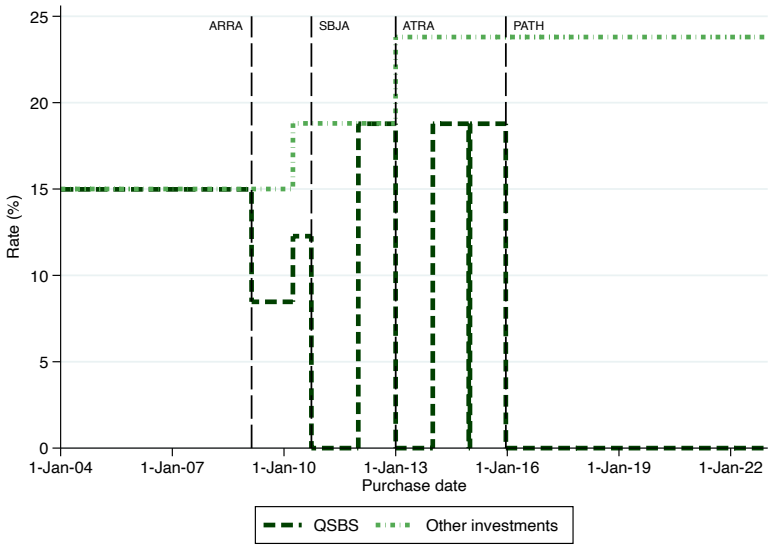
[24] The QSBS exclusion also varies across states due to differences in their state-level long-term capital tax rates or in their decisions to adopt the federal tax rules about the QSBS exclusion for their own state-level tax rules. States either fully conform with federal tax law and apply the same exclusion rate, partially conform and apply a different exclusion rate than the federal rate, or do not conform at all and fully tax QSBS capital gains at the state level. For example, California does not apply the federal exclusion to their own state-level tax rules.

[25] Appendix Figure C1 plots the history of the federal tax wedge on QSBS capital gains all the way back to its original introduction in 1993. The figure shows that cuts to the federal long-term capital gains tax rate in 1997 and 2003 eroded the incentive to favor QSBS over other investments.

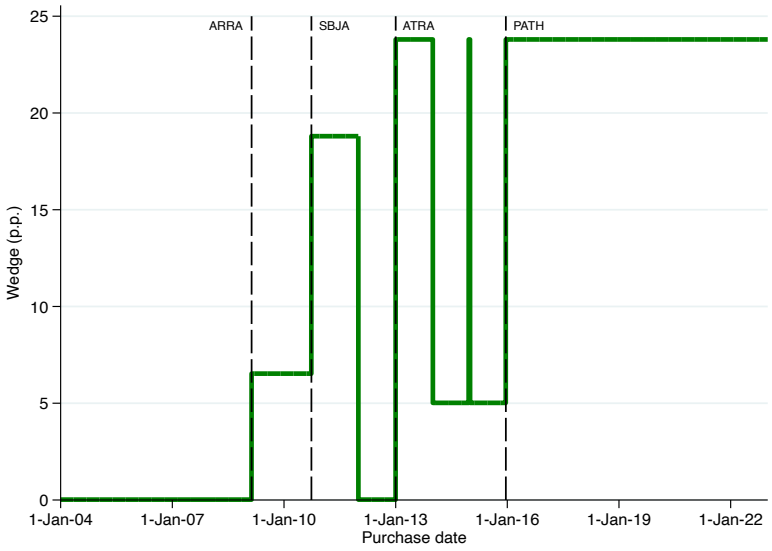
[26] We refer to the *expected* tax rates, since benefiting from the tax exclusion requires at least a five-year gap between the investment date and the exit date, during which investors face uncertainty about the future federal long-term capital gains tax rate that will apply when they eventually exit the investment (in contrast, the exclusion rate is determined as of the investment date.) As a result, the tax wedge on QSBS capital gains can either shrink or expand after the investment decision has already been made.

Figure 3: Recent History of the Federal Tax Exclusion on QSBS Capital Gains

(a) Expected tax rates on QSBS vs. other investments



(b) Expected tax wedge on QSBS



Source: Polsky and Yale (2023).
Notes: This figure compares the evolution of the expected federal tax rate on QSBS capital gains between 2004 and 2022 with that of the expected federal long-term capital gains tax rate on other investments. Panel (a) depicts the evolution of the two expected rates (i.e., on QSBS and other investments) separately, while panel (b) plots the difference between the two rates (i.e., the expected tax wedge, measured in percentage points). Note that the rates plotted are expected (not actual) rates, as those are the rates that individuals were expected to be subject to as of their purchase date, even though the rates may have changed ex-post due to changes in the long-term capital gains tax rates on other investments. The dashed vertical black bars indicate the different Acts that changed QSBS legislation: the American Recovery and Reinvestment Act (ARRA), the Small Business Jobs Act (SBJA), the American Tax Payer Relief Act (ATRA), and the Protecting Americans from Tax Hikes Act (PATH).

With the onset of the crisis in 2008 and the accompanying contraction of credit, the U.S. federal government decided to expand the QSBS tax exclusion to help small private companies raise financing. In particular, in 2009, the American Recovery and Reinvestment Act (ARRA) temporarily raised the excludable share of QSBS capital gains from 50% to 75% until the end of 2010. In 2010, the Small Business Jobs Act (SBJA) temporarily further raised the exclusion rate from 75% to 100% until the end of 2011. These temporary expansions of the QSBS exclusion rate were repeatedly allowed to expire but retroactively extended until 2015, when the Protecting Americans from Tax Hikes Act (PATH) made the 100% exclusion rate permanent. This 100% exclusion rate has therefore effectively been in place since 2010. Given that the federal long-term capital gains tax rate has ranged from 15 to 24% since 2008, the QSBS capital gains tax exclusion has made investments in early-stage companies considerably more attractive to U.S. HNWIs relative to comparable investments in publicly listed stocks or other financial assets.

4.2 HNWIs' Early-Stage Investments and Companies

4.2.1 Effects on Investments

We first study the extent to which the expanded federal tax exclusion on QSBS capital gains increased HNWIs' early-stage investments at the company level. We consider the sample of 14,494 U.S. companies that, as of the end of 2008, had already been founded and that had never before gone bankrupt, become publicly listed, or been acquired. We construct an unbalanced panel of these companies' yearly observations from 2004 to 2022, including each company only from its year of founding to the year of its first bankruptcy, public offering, or acquisition. Treated U.S. companies are those that satisfied the three necessary conditions as of the end of 2008 (i.e., the year before the first QSBS reforms were introduced): (1) they were classified as tax-paying C corporations; (2) they were active primarily in a qualified trade or business; and (3) they had raised no more than $50 million in total financing.[27] The control companies are all other U.S. companies that were active and private as of the end of 2008, but that failed to satisfy at least one of these three conditions. Table C1 reports summary statistics for these companies as of the end of 2008, distinguishing between treated and control companies based on their QSBS eligibility. Relative to ineligible issuers of QSBS, eligible companies were more likely to be younger, operate in the technology sector, and raise early-stage financing from U.S. HNWIs. The inclusion as controls of companies that satisfy some but not all the QSBS eligibility conditions is therefore crucial in overcoming any issues of imbalance.

[27] We use total financing raised as a proxy for gross assets—which is the actual measure used to determine QSBS eligibility—as Pitchbook has information on gross assets only for a limited sample of companies.

We identify the effects of the QSBS reforms on U.S. HNWIs' early-stage investments in treated companies relative to control companies using the following regression specification:

$$Y_{i,t} = \sum_{s \neq 2008} \beta_s \times 1\{t = s\} \times \text{QSBS}_i + \alpha_i + \gamma_{\text{CCorp}(i),t} + \delta_{\text{QTB}(i),t} + \zeta_{\text{50M}(i),t} + u_{i,t} \quad (1)$$

where $Y_{i,t}$ is a dummy variable for whether company i raised financing in year t from at least one U.S. HNWI, and QSBS_i is a dummy variable equal to 1 for treated companies and 0 for control companies. We also control for company fixed effects α_i and separate year fixed effects for each dimension of QSBS eligibility, that is, $\gamma_{\text{CCorp}(i),t}$, $\delta_{\text{QTB}(i),t}$, and $\zeta_{\text{50M}(i),t}$, where $\text{CCorp}(i)$ indicates whether company i was a C corporation, $\text{QTB}(i)$ indicates whether company i was active primarily in a qualified trade or business, and $\text{50M}(i)$ indicates whether company i had raised no more than \$50 million in total financing by the end of 2008. Since we exclude the coefficient β_{2008}, we can interpret the parameter of interest β_s as the percentage-point change since 2008 in the probability that QSBS-eligible companies raised money from at least one U.S. HNWI, relative to the equivalent change for QSBS-ineligible companies. In practice, we first estimate Equation (1) separately on the subsample of the companies founded in each year. We then take a weighted average of the subsample-specific effects for each year, using as weight the share of the companies appearing in that year's sample. We do this to address the issue that a single regression that pools different founding-year cohorts could generate spurious pre-trends due to the unbalancedness of the sample across these cohorts.[28]

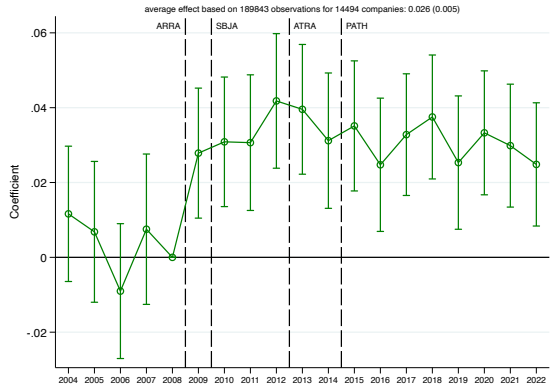
Figure 4a plots our estimates of Equation (1). The estimated coefficients for the years 2004-2007 are close to zero and statistically insignificant, suggesting the absence of pre-trends. Immediately after the first QSBS reform was introduced in 2009, the probability that QSBS-eligible companies raised financing from HNWIs jumped by approximately 3 percentage points, with the effect remaining quite stable until 2022. The average effect over the entire post-reform period was 2.6 percentage points, an estimate which is statistically significant at the 1% level and corresponds to a 62% increase relative to the pre-reform probability of 4.2%. To explore which types of companies drive this average effect, Appendix Figure C2 reports the heterogeneous effects by founding-year cohort, sector, and size. We find a positive effect for all different types of companies, but the heterogeneous effects are only statistically significant different between more-affected younger companies and less-affected older ones. Appendix Figure C3 shows that our analysis is robust to the use of alternative estimators and fixed effects. Appendix Figure C4 also finds a similar effect by considering an auxiliary regression discontinuity design that evaluates the effect of falling below the \$50 million gross assets threshold by the end of 2008 on the small subsample of C corporations active primarily in a qualified trade or

[28] Appendix Section C.2 characterizes this issue with a stylized example.

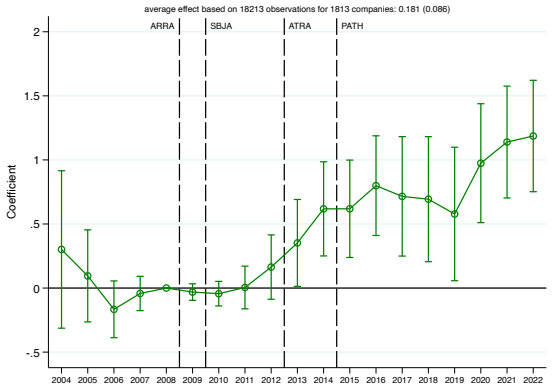
business.

Figure 4: Effects of the QSBS Reforms on Companies

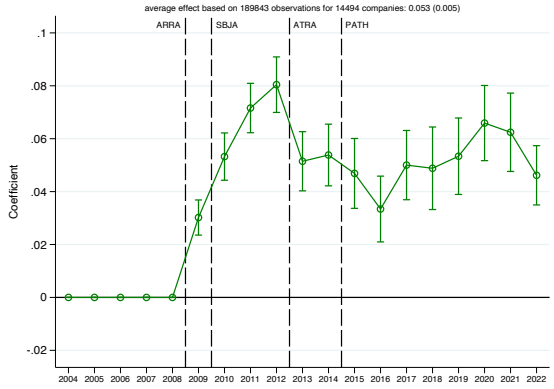
(a) Probability of raising early-stage financing from at least one U.S. HNWI



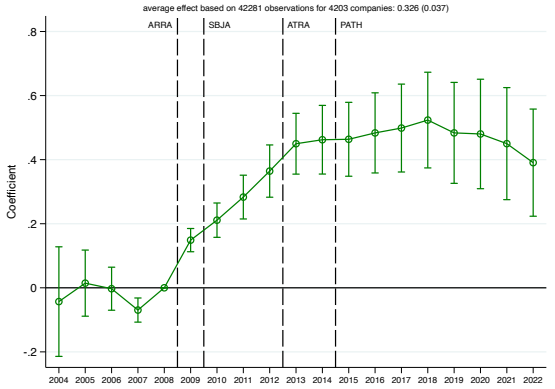
(b) Log accumulated amount of early-stage financing raised from U.S. HNWIs



(c) Probability of staying active and private



(d) Log valuation



Source: Pitchbook.
Notes: This figure depicts the effects of the QSBS reforms on various outcomes at the company level, estimated using Equation (1). Panel (a) plots the reforms' effects on QSBS-eligible companies' probability of raising financing from at least one U.S. HNWI. Panel (b) plots their effects on the log accumulated amount of financing (in millions of U.S. dollars) that QSBS-eligible companies raised from U.S. HNWIs. Panel (c) plots their effects on QSBS-eligible companies' probability of staying private. Finally, Panel (d) plots their effects on QSBS-eligible companies' log valuations, based only on the organic growth in their valuations rather than the growth due to any new equity financing that they raised.

Figure 4b depicts our estimates of Equation (1) when we instead consider the log accumulated amount of early-stage financing raised from U.S. HNWIs as outcome variable. The estimated coefficients for the years 2004-2007 are statistically insignificant. However, the post-reform estimates are statistically significant starting in 2013, suggesting that QSBS-eligible companies' increased likelihood to raise financing from HNWIs after 2008 (see Figure 4a) did not immediately—but did eventually—translate into greater amounts of

financing raised. Indeed, the average effect over the entire post-reform period corresponds to a 18.1% increase that is statistically significant at the 5% level. One may worry that this increase in financing for QSBS-eligible companies was driven by HNWIs reallocating their investments from QSBS-ineligible companies. Appendix Figure C5 shows, however, that QSBS-ineligible companies also saw an increase—albeit a smaller one than QSBS-eligible companies—in their financing raised from HNWIs after the QSBS reforms. Furthermore, most of the new financing from HNWIs was raised from first-time investors in early-stage capital markets (see Appendix Figure A5), so that reallocation is unlikely to be a major threat to our identification strategy.

4.2.2 Effects on Other Company-Level Outcomes

We next study the effects of the QSBS reforms on other outcomes at the company level, namely, their probability of staying active and private as well as their log valuation. We thus first consider as outcome of Equation (1) a dummy variable equal to 1 if company i had never gone bankrupt, become publicly listed, or been acquired and 0 if it experienced one of these events in year t. Figure 4c shows that QSBS-eligible companies became on average 5.3 percentage points more likely to stay active and private relative to QSBS-ineligible companies over the post-reform period. Since 92.4% of the QSBS-ineligible companies in our post-reform sample remained active and private, treated companies experienced a 5.7% increase in this probability relative to control companies. As companies that received more financing from HNWIs became more likely to stay active and private, this result suggests a link between the increasing participation of HNWIs in private capital markets and the growth in the number of active private companies. Appendix Figures C6a-C6c show that most of this effect was explained by a decline in QSBS-eligible companies' probability of being acquired, while the rest can be attributed to declines in their probabilities' of going bankrupt or becoming publicly listed.[29]

Finally, Figure 4d shows that QSBS-eligible companies' valuations also increased after the reforms relative to those of QSBS-ineligible companies. We construct each company's valuation history by comparing its pre-money valuation as part of each of its new deals

[29] Note that the absence of pre-trends in Figure 4c is by construction, since the sample includes only companies that had never before been publicly listed as of the end of 2008. To test for the absence of pre-trends, we consider an alternative regression that evaluates the QSBS reforms' effect on the number of active private QSBS-eligible companies. For that, we first divide companies into cells defined by the triple interaction of their tax classification (C corporation or other), industry (216 finely defined ones), and amount of financing raised prior to each year (10 ranges including \$0-0.5 million, \$0.5-1 million, etc.). We can thus classify each cell as QSBS-eligible or QSBS-ineligible. We then estimate an event study regression of the number of active private companies in each cell on QSBS eligibility, controlling for fine one-dimensional time trends. Appendix Figure C7 reports the results of this regression. Reassuringly, there is a lack of pre-trends, as well as an increase in the number of active private QSBS-eligible companies in the post-reform period, consistent with the results in Figure 4c.

to its post-money valuation as part of its previous deal (see Section 3.2). We thereby only consider the organic growth in its valuation rather the growth due to any new equity financing that it raised. This result suggests that QSBS-eligible companies' pre-reform investors benefited from the QSBS reforms: since companies' valuations increased, so did investors' returns on their investments. Appendix Figure C6c further shows that the reforms led to effectively no increase in the number of workers employed—which we can also observe for a subset of the companies in the data from Pitchbook—at QSBS-eligible companies relative to QSBS-ineligible companies. Taken together, the reforms appear to have created more wealth than jobs, at least in the short to medium run.

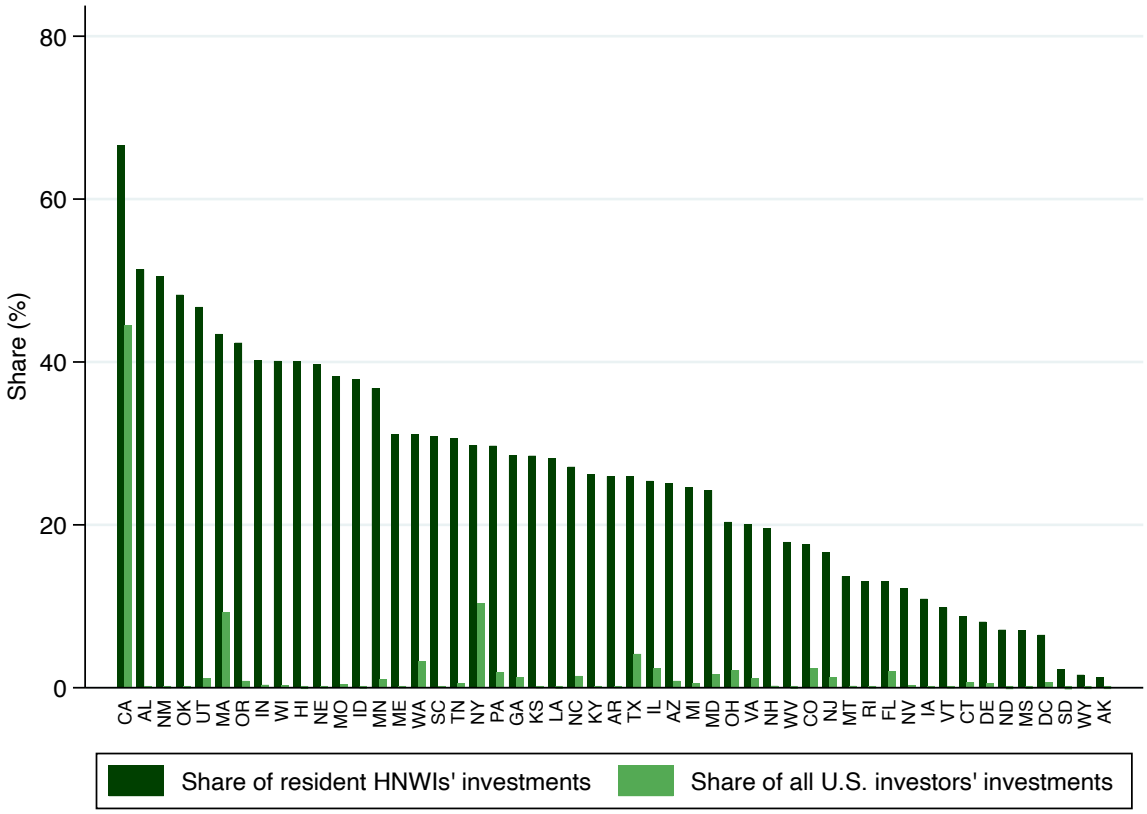
4.3 HNWIs' Early-Stage Investments and Income Inequality

4.3.1 Effects on Investments: State Level

To assess the implications of HNWIs' increased participation in private capital markets for economic inequality, we begin by analyzing the effects of the reforms at the state level. We rely on the following hypothesis: if states more exposed to the QSBS reforms experienced greater increases in investments by their resident HNWIs, then economic inequality should have increased more in those same states due to the excess returns yielded by those investments (Section 3.2). As a first step, we therefore analyze whether HNWIs' early-stage investments increased more after the reforms in states with more resident HNWIs ex ante. The reason the ex-ante number of HNWIs residing in each state represents the state-level exposure to the federal reforms is that any investments induced by the reforms would have been raised from accredited investors, since generally only they can invest in early-stage companies. Given that the increase in HNWIs' early-stage investments was driven by first-time investors (Appendix Figure A5), then states with more resident HNWIs ex ante would have seen more of them enter early-stage capital markets—and also greater increases in their early-stage investments. Appendix Figure C8 visualizes the intuition behind our state-level exposure measure. For both the pre-reform (2004-2008) and post-reform (2009-2022) periods, it plots the log-linear relationship across states between the number of resident HNWIs in 2008 (horizontal axis) and the average across years of both the total annual amount invested by resident HNWIs and the number of resident HNWIs investing (vertical axis). The counterclockwise rotations in the slopes of these relationships between 2004-2008 and 2009-2022 are consistent with our earlier evidence at the company level that the QSBS reforms, which started being introduced in 2009, increased HNWIs' early-stage investment activity (Section 4.2.1).

There are two main factors that could confound the number of resident HNWIs in 2008

Figure 5: In-State Bias of Resident HNWIs' Early-Stage Investments, 2004-2022



Source: Pitchbook.
Notes: This figure compares the share of each state's resident HNWIs' early-stage investments allocated to companies headquartered in that state to the share allocated by all U.S. investors (i.e., both HNWIs and institutions) during the 2004-2022 period.

as a measure of the state-level exposure to the federal QSBS reforms. First, HNWIs may have chosen to settle in certain states to get access to exclusive local investment opportunities (e.g., aspiring angel investors moving to California). Such strategic choice of residence would especially be a concern if HNWIs exhibited home bias in their early-stage investments—that is, if they invested primarily in local startups. Figure 5 shows that this was indeed the case in the U.S. from 2004 to 2022: every state's resident HNWIs allocated a greater share of their early-stage investments to companies headquartered in that state relative to the share allocated by all U.S. investors (i.e., both HNWIs and institutions).[30] Second, given this home bias in HNWIs' early-stage investment portfolios, if startups in states with more resident HNWIs were exposed to different economic shocks than those in states with fewer resident HNWIs, then the early-stage investments by HNWIs residing in the first set of states may have grown faster for reasons unrelated to the QSBS reforms.

To address these potential confounders, we compare resident HNWIs' early-stage investments in companies headquartered in their own state to the investments in those same companies by other investors, including institutional investors headquartered in the same state but that do not pay capital gains taxes and were therefore unaffected by the QSBS reforms. In addition to the comparisons that we make across states and across time, this third dimension of comparison across investor groups ensures that we do not attribute to the QSBS reforms changes in resident HNWIs' early-stage investment activity that we also observe among other "untreated" investors. Appendix Figure C11 visualizes the intuition behind this third dimension of comparison, showing that in-state investments by resident HNWIs grew more than in-state investments by resident institutions after the reforms. Moreover, the two investor groups' investments increasingly diverged as the 100% capital gains tax exclusion was temporarily extended and eventually made permanent.

To formally analyze the effects of the QSBS reforms on resident HNWIs' early-stage investments, we estimate the following regression:

$$\ln Y_{s,g,t} = \sum_{j \neq 2008} \beta_j (\ln X_{s,2008} \times \mathbb{1}_{g=\text{resident HNWIs}}) + \alpha_{s,g} + \gamma_{g,t} + \delta_{s,t} + \zeta_{g,t} W_{s,t} + \epsilon_{s,g,t}, \quad (2)$$

where $\ln Y_{s,g,t}$ is the log millions of dollars invested in startups headquartered in state s by investor group g in year t, as we observe in the data from Pitchbook;[31] $\ln X_{s,2008}$

[30] Appendix Figure C9 shows that HNWIs exhibited similar home bias during the pre-reform period from 2004 to 2008. Appendix Figure C10 further shows that, other than in the companies headquartered in their own state, HNWIs residing in all states tended to additionally invest only in companies headquartered in California (where Silicon Valley is located) and, to a lesser extent, Massachusetts and New York.

[31] We focus on the total amount invested by each investor group rather than their average amount invested, since we observe the total number of *potential* investors in each investor group only for HNWIs (in the GEOWEALTH-US dataset) but not for institutions (for which we observe only those that *actually* invested in the data from Pitchbook).

is the log number of accredited investors residing in state s in 2008, as we observe in the GEOWEALTH-US dataset; and $\mathbb{1}_{g=\text{resident HNWIs}}$ is a dummy variable equal to 1 for resident HNWIs and to 0 for the three other investor groups that we consider: resident institutions, non-resident institutions, and non-resident HNWIs.[32] Since we include state-investor group fixed effects $\alpha_{s,g}$, investor group-year fixed effects $\gamma_{g,t}$, and state-year fixed effects $\delta_{s,t}$, Equation (2) is a triple-difference regression that compares "treated" resident HNWIs to other "untreated" investors. We also include a vector of control variables $W_{s,t}$ that contains both the expected state-level long-term capital gains tax wedge on QSBS and the state-level house price index, whose effects $\zeta_{g,t}$ we allow to vary by both investor group and year.[33] Since we exclude the coefficient β_{2008} when estimating Equation (2), we can interpret the parameter of interest β_j as the change between 2008 and j in the elasticity of resident HNWIs' early-stage investments with respect to the number of resident HNWIs in 2008. Importantly, β_j identifies a relative change in this elasticity, partialing out the average change across the three other "untreated" investors groups and thereby isolating the effect of the QSBS reforms on resident HNWIs' early-state investments.

Figure 6 plots our baseline estimates of β_s from Equation (2) for the 2004-2022 period, replacing $\ln Y_{s,g,t}$ with $\ln(1 + Y_{s,g,t})$ as the outcome variable to ensure a balanced panel.[34] The estimated coefficients for the pre-reform period are never statistically significant and exhibit no pre-trends. We find an immediate increase in resident HNWIs' early-stage investments during 2009-2010, after the initial temporary expansions of the QSBS capital gains tax exclusion. We find even further increases during 2013-2014, when the 100% exclusion was repeatedly—but still only temporarily—renewed. Finally, when the 100% exclusion was made permanent in 2015, our estimated effect reaches its peak, remaining

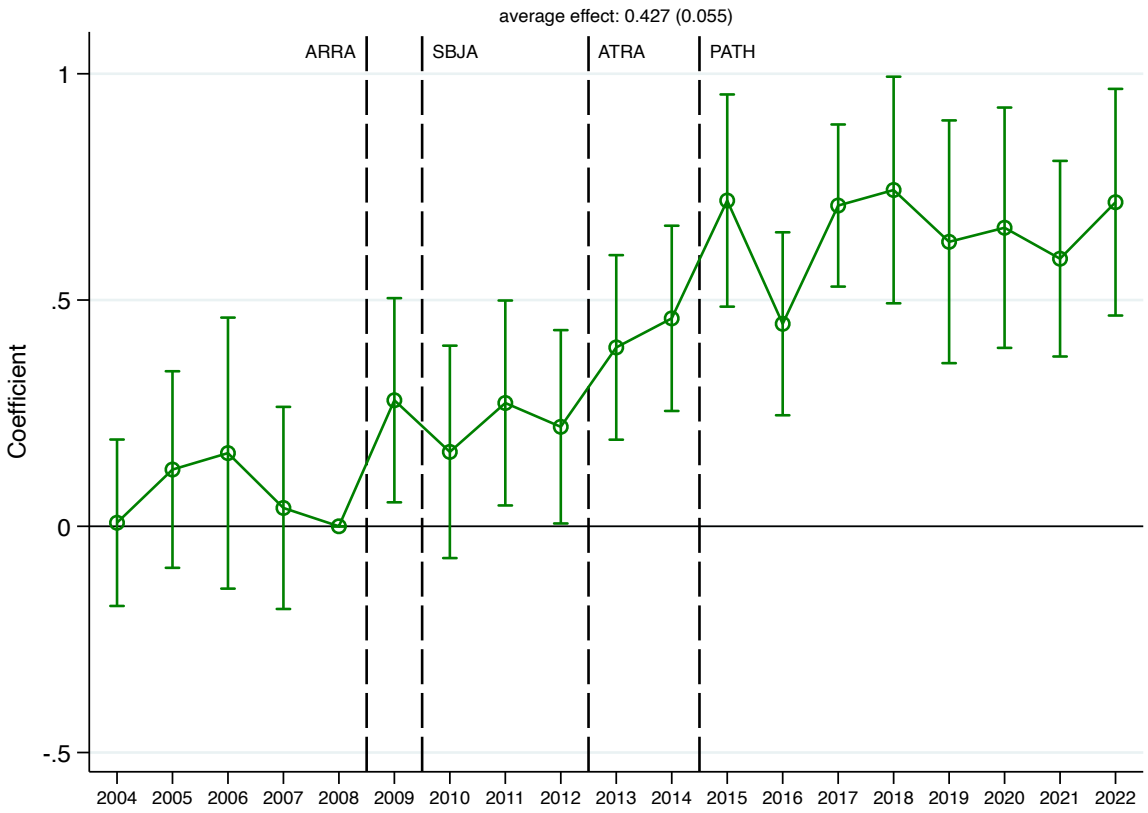
[32] For the same reason resident institutions were unaffected by the QSBS reforms, non-resident institutions were also unaffected. Moreover, the reforms should not have affected the relationship between the number of HNWIs residing in a particular state in 2008 and non-resident HNWIs' early-stage investments in companies headquartered in that state, since the former should predict only changes in resident HNWIs' early-stage investments induced by the reforms. Thus, non-resident institutions and non-resident HNWIs both represent additional types of "untreated" investors for the purposes of our state-level analysis.

[33] The expected state-level long-term capital gains tax wedge on QSBS accounts for heterogeneous exposure across states and investor groups to state-specific QSBS reforms, which may have provided certain states' residents with additional tax incentives to invest in early-stage companies. Similarly, the state-level house price index accounts for heterogeneous exposure to state-specific changes in house prices, which may have affected certain investors' wealth and therefore their investment activity.

[34] Appendix Figures C12a and C12b show that the dynamic pattern of our baseline estimates is robust to estimating Equation (2) using $\ln Y_{s,g,t}$ as the outcome while dropping the 13% of observations for which $Y_{s,g,t} = 0$, as well as to estimating it using a Poisson pseudo-maximum likelihood (PPML) estimator following Chen and Roth (2024). The reason we prefer using $\ln(1 + Y_{s,g,t})$ as the outcome in our baseline analysis is that the PPML estimator implicitly assigns larger regression weights to observations for which $Y_{s,g,t}$ is larger. As a result, even on the subsample of observations for which $Y_{s,g,t} > 0$, the OLS estimates using $\ln Y_{s,g,t}$ as the outcome differ from the PPML estimates primarily because of the implicit weights assigned by PPML, as shown in Appendix Figures C12c and C12d. Since we are interested in the effects of the QSBS reforms on the average state, unweighted OLS estimation using $\ln(1 + Y_{s,g,t})$ is our preferred way to still consider a logged outcome while retaining observations for which $Y_{s,g,t} = 0$.

around this elevated level until 2022. The dynamic effects that we estimate are therefore consistent with the actual timing of the introduction of the reforms. Appendix Figure C13 shows that the increase in resident HNWIs' early-stage investments was driven by an increase in the number of distinct resident HNWIs investing, rather than by an increase in their average amount invested by each, consistent with the entry of first-time investors being the key margin of adjustment (Appendix Figure A5). The results of our state-level analysis are also consistent with the increase in QSBS-eligible companies' probability of raising financing from HNWIs that we documented in Section 4.2.1.

Figure 6: Effects of the QSBS Reforms on Resident HNWIs' Early-Stage Investments



Source: Pitchbook, GEOWEALTH-US.
Notes: This figure plots the triple-difference estimates (with 95% confidence intervals) of Equation (2) for the effects of the QSBS reforms on resident HNWIs' early-stage investments in companies headquartered in their own state. The regression is based on $51 \times 4 \times 19 = 3{,}876$ state-investor group-year observations, with standards errors clustered at the state level. To ensure a balanced panel, we replace $\ln Y_{s,g,t}$ with $\ln(1 + Y_{s,g,t})$ as the outcome. The average effect refers to the estimate of Equation (2) after replacing the dynamic coefficient β_j with the static coefficient $\beta_{j:j>2008}$.

Our baseline estimates are robust to the use of alternative vectors of control variables (Appendix Figure C14a) and alternative measures of the number of HNWIs residing in each state in 2008, such as the number of residents ranked in the Forbes 400 rich list (Appendix Figure C14b). Our results are also robust to dropping California and eight other states

containing cities considered to be major technology hubs (Appendix Figure C14c), as well as to extending Equation (2) to further consider resident HNWIs' early-stage investments in companies headquartered in states other than their own (Appendix Figure C14d). Reassuringly, the increase in the triple-difference parameters identified in Equation (2) was driven by an increase in the difference-in-difference parameters for resident HNWIs, rather than by a decrease in the difference-in-difference parameters for other investor groups (Appendix Figure C15a)—exactly as we would have expected, given the nature of the QSBS reforms. Our triple-difference estimates are lastly robust to considering resident institutions as the only group of other "untreated" investors (Appendix Figure C15b).

Finally, we use our estimates to quantify the scale of the effects of the QSBS reforms relative to the overall growth in HNWIs' early-stage investments. We start by replacing the dynamic coefficient β_j from Equation (2) with the static coefficient $\beta_{j:j>2008}$ to obtain an average effect of 0.427. With 6.4 million accredited investors residing in the U.S. in 2008, this estimate implies that the QSBS reforms explain $(6.4 \times 10^6)^{0.427} \approx \$800$ million $= \$0.8$ billion of the increase in HNWIs' early-stage investments between the average pre-reform year and the average post-reform year. Since the overall increase was \$6.9 billion (from \$0.9 billion on average in the pre-reform period to \$7.8 billion on average in the post-reform period), we can attribute $0.8/6.9 \approx 12\%$ of the increase in HNWIs' early-stage investments after 2008 to the QSBS reforms.[35]

4.3.2 Implications for State-Level Income Inequality

In the previous subsection, we show as a first step that HNWIs' early-stage investments increased more after the reforms in states with more resident HNWIs ex ante. In this subsection, we next explore as a second step whether economic inequality increased by more after 2008 in states where HNWIs' early-stage investments increased more after the reforms, consistent with the excess returns that they earned on their investments in these markets (Section 3.2). Ideally, we would analyze the implications of these investments for state-level wealth inequality, since HNWIs tend to accumulate their returns on these investments for several years before realizing them as capital gains income (Appendix Figure A2). However, there is no state-level wealth inequality data available for the U.S. that is derived directly from observed wealth. We thus instead focus on the implications

[35] We also consider an alternative quantification, exponentiating the number of HNWIs residing in each state with respect to an appropriate estimate of the average effect and then taking the sum of the exponentiated numbers across states. If we use the PPML estimate of 0.285 for the average effect from Appendix Figure C12b—the larger regression weights assigned by the PPML estimator to observations for which $Y_{s,g,t}$ is larger become appropriate in this case, since we are quantifying how the effect for each state contributes to the nationwide increase—then the share of the overall increase in HNWIs' investments attributable to the QSBS reforms rises to 18%. Thus, the two quantifications yield similar results.

for state-level income inequality, which we can measure using data from the IRS Statistics of Income database.

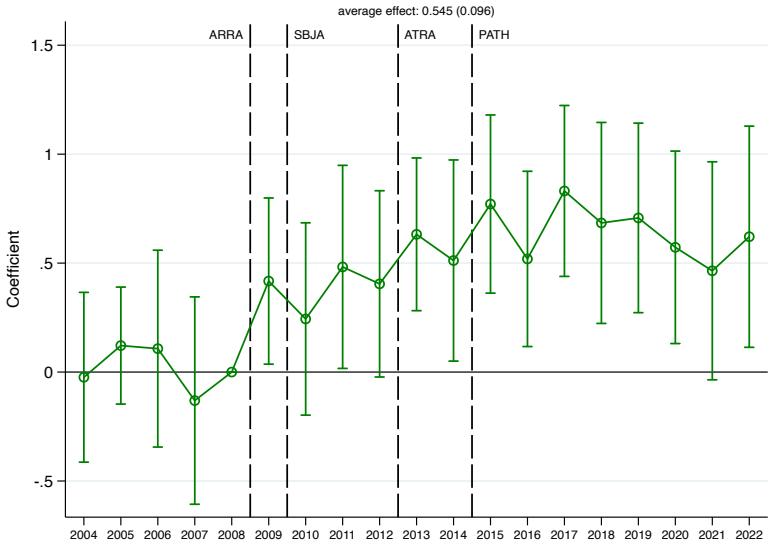
Before turning to the state-level income inequality analysis, we first confirm that the HNWIs' early-stage investments induced by the QSBS reforms and directly observable in Pitchbook did actually generate income for these investors. Using the same regression specificed in Equation (2), we consider an additional outcome: the log millions of dollars of capital gains income realized by investor group g from the investments that they made in startups headquartered in state s in year t.[36] We attribute all the realized capital gains to the investment year t. This allows us to evaluate whether the same increase in resident HNWIs' early-stage investments since 2008 that we document in Section 4.3.1 eventually translated into an increase in their capital gains income. In our baseline analysis, we consider realized capital gains income based on both observed and imputed exits. For investments that we do not observe as exited, we impute exits after five years, the mean of the observed years to exit distribution. Reassuringly, Figure 7a shows that the dynamic pattern of estimated effects on resident HNWIs' early-stage capital gains income is similar to that of investments in Figure 6. Appendix Figures C16b and C16c show that this increase in capital gains income is robust to considering only the investments that we directly observe as exited, as well as to alternative imputations of times to exit for those investments that we do not observe as exited.

We then validate this same dynamic pattern in the IRS data while also more directly considering the implications for income inequality. For that, we rely on the taxable income distribution that we construct for every state in every year using the IRS Statistics of Income database (see Section 2.2 and Appendix Section D.2.1), from which we can also decompose the total income earned by individuals ranked in each range of the distribution into realized capital gains income and other income. We can divide the state-level income distribution into up to 127 income groups: 99 groups for each percentile from the 1st to the 99th, 9 groups for each tenth of a percentile from the 99.1st to the 99.9th, 9 groups for each hundredth of a percentile from the 99.91st to the 99.99th, and 10 groups for each thousandth of a percentile from the 99.991st to the 100th. For each income group $g \in \{1, \ldots, 99, 99.1, \ldots, 99.9, 99.91, \ldots, 99.99, 99.991, \ldots, 99.999, 100\}$ in state s in year t, we calculate the average thousands of dollars of each type of taxable income $Y_{s,g,t}$ earned

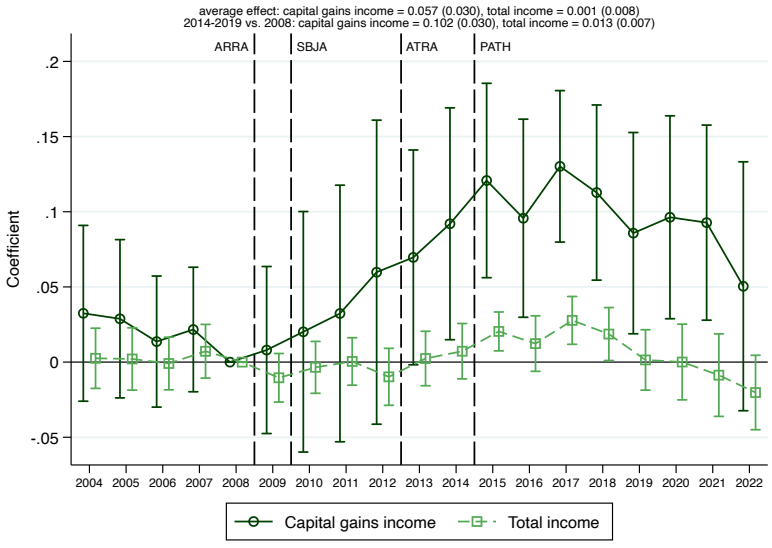
[36] Since capital gains income is negative for 11% of observations, in which case it cannot be logged, we instead consider the transformation $\ln[(1 + (Y_{s,g,t}, 0)]$ as our baseline outcome. Our results are robust to considering the alternative inverse hyperbolic sine transformation (Appendix Figure C16a) that can also handle negative values.

Figure 7: Implications of the QSBS Reforms for Income Inequality

(a) Resident HNWIs' early-stage capital gains income



(b) Average income among top 0.5% of earners



Source: Pitchbook, IRS Statistics of Income, GEOWEALTH-US.
Notes:

by the individuals belonging to that group.[37] We ultimately run the following regression:

$$\ln Y_{s,g,t} = \sum_{j \neq 2008} \beta_j (\ln X_{s,2008} \times \mathbb{1}_{g>99.5}) + \alpha_{s,g} + \gamma_{g,t} + \delta_{s,t} + \zeta_{d(g),t} W_{s,t} + \epsilon_{s,g,t} \quad (3)$$

where $\ln X_{s,t}$ is the log number of accredited investors residing in state s in 2008, just as in Equation (2); $\mathbb{1}_{g>99.5}$ is a dummy variable equal to 1 for income groups $g > 99.5$ in the top 0.5% of the state-level income distribution and 0 otherwise; $W_{s,t}$ is a vector of control variables whose effects $\zeta_{d(g),t}$ are the same for all income groups g within decile d of the state-level income distribution; and $\alpha_{s,g}$, $\gamma_{g,t}$, $\delta_{s,t}$ are state-income group, income group-year, and state-year fixed effects, respectively. The reason we split the state-level income distribution at the 99.5th percentile is that this is the highest-earning income group for which, in every state in every year, the average income of the individuals belonging to it exceeded $200,000—the individual income threshold for qualifying as an accredited investor (Section 2.2). We can therefore be reasonably confident that, across all states and years, individuals ranked in the top 0.5% of their own state's income distribution were always accredited investors. Since we exclude the coefficient β_{2008} when estimating Equation (3), we can interpret the parameter of interest β_j as the change between 2008 and j in the average (log) income gap between the top 0.5% and bottom 99.5% of the state-level income distribution.

Figure 7b plots our baseline estimates of β_s from Equation 3 for both realized capital gains income and total income for the 2004-2022 period. The average capital gains income of the top 0.5% began to gradually increase after the first QSBS reforms were introduced in 2008.[38] This trend continued until 2015, when the capital gains income of the top 0.5% reached a multi-year peak—one that was large enough to also statistically significantly increase their total income in 2014-2019 relative to 2008.[39] The changes in capital gains income in 2014-2019 dominated any changes in other income (Appendix Figure C17),

[37] Taxable income only includes QSBS realized capital gains income, as long as it is not excluded due the QSBS capital gains tax exemption. As we discuss in Section 4.1, only the first $10 million of the capital gains realized from the sale of QSBS issued by a particular issuer are generally excludable from the long-term capital gains tax. However, as we discuss in Appendix Section A.5, the excludable share of all capital gains income earned by HNWIs from their QSBS was only 21% from 2004 to 2022. We therefore expect the majority of QSBS capital gains to be classified as taxable income.

[38] Note that, unlike in Figure 7a, there is no sudden sharp increase in 2009 in Figure 7b. This is because the former attributes the capital gains income to the year of investment, while the latter attributes it to the year of realization.

[39] In contrast, the average effect on the total income of the top 0.5% in 2009-2022 was not statistically significant. We focus on the 2014-2019 period for two main reasons: (1) we should only expect an effect happening after 2014, given that capital gains income is attributed to the year of realization and investors need to hold the stock for at least five years to qualify for the QSBS exclusion; and (2) we exclude the statistically insignificant estimates from the COVID-19 period, as these null estimates are driven by a decline in the other income of the top 0.5% that cannot straightforwardly be attributed to the QSBS reforms.

with the increase in both capital gains income and total income being even larger for the top 0.1% compared to only the top 0.5% (Appendix Figure C18). We can thus conclude that, first, the average income gap between the top 0.5% and the bottom 99.5% increased by more after 2008 in the same states where resident HNWIs' early-stage investments increased by more after the QSBS reforms, and, second, this expanding gap was driven by an increase in realized capital gains among the top 0.5%. Appendix Figure C19 shows our findings are robust to alternative transformations of the outcome variable. Furthermore, Appendix Figure C20 corroborates that our results are driven by an increase in the underlying difference-in-difference parameters for the top 0.5%, consistent with the nature of the reforms.

Finally, we should stress that the state-level income inequality analysis in this section has several limitations and results should be interpreted with caution. First, since we do not directly observe the QSBS realized taxable capital gains in the IRS Statistics of Income database, we rely on the total realized taxable capital gains. Hence, there may be some changes in the total realized capital gains that may not necessarily be attributed to the QSBS reforms. Reassuringly, we show by means of Figure 7a that the HNWIs' early-stage investments induced by the QSBS reforms and directly observable in Pitchbook did actually generate income for these investors. Second, one may worry about other shocks that (e.g. COVID-19) differentially affected the income of the top 0.5% relative to the bottom 99.5% and were correlated with the ex-ante number of accredited investors residing in a state. Nevertheless, with the exception of the COVID-19 period, we do find significant and robust results in line with the excess return mechanism that we hypothesized. To assess the overall effect of this same mechanism, we next run counterfactual simulations to quantify how U.S. nationwide wealth inequality would have evolved without these excess returns.

5 Wealth Inequality Counterfactuals

The reduced-form analyses in Section 4.3.2 assess the implications of the growing participation of HNWIs in private capital markets on state-level income inequality. However, the effects of the excess return channel on income inequality only operate through realized capital gains and miss unrealized capital gains. To understand the total effect of the excess return channel, we need to consider both realized and unrealized gains, and hence focus on wealth rather than income inequality. In this section, we run partial equilibrium counterfactual simulations to quantify how the increase in the participation of HNWIs in early-stage markets has shaped nationwide U.S. wealth inequality over the last two decades. In Appendix D.2.2, we also assess the implications of the excess return channel

and the tax savings associated to the QSBS reforms for nationwide U.S. income inequality by means of counterfactual simulations.

5.1 Methodology

To carry out the counterfactual simulations for wealth inequality, we estimate the baseline wealth distribution series following the Distributional Financial Accounts (DFA) methodology developed by Batty et al. (2020), which combines the Financial Accounts (FA) with the Survey of Consumer Finances (SCF) to build U.S. wealth distribution series consistent with macroeconomic aggregates. We also follow Batty et al. (2020) and improve the SCF's ability to capture the top of the wealth distribution by adjusting the very top with the Forbes 400 rich lists. To ensure consistency with the state-level income inequality analysis in Section 5.1, the baseline counterfactual analyses are based on the top 0.5% wealth group. Appendix Table D8 reports summary statistics for U.S. top wealth groups in 2010 and 2022. The top 0.5% wealth group had an average wealth of 31.33 million USD and concentrated 21.74% of total net wealth in 2010. The wealth share went up to 23.33% in 2022 and its average wealth grew substantially reaching 48.47 million USD.

We run the counterfactual simulations for the post-reform 2010-2022 period using the private and counterfactual public gains—both realized and unrealized—derived from Pitchbook and described in Appendix B.[40] In particular, we re-estimate the wealth inequality series replacing the cumulated private capital gains by the counterfactual cumulated gains had HNWIs invested this money in the NASDAQ 100. We rely on the NASDAQ as the baseline public stock market index, since it is the one that puts more weight on tech companies and thus it is closer to the portfolio of early-stage companies (Appendix Figure A6). The baseline counterfactual analyses are based on the top 0.5% wealth group, but we also present results for the top 10% wealth group and different subgroups within the top 10%. The reason is that 90% of the individuals to whom we assign capital gains appear to belong to this wealth group.

The main challenge we face when implementing the counterfactuals is how to distribute the actual and counterfactual cumulated gains along the wealth distribution in the SCF. For the Forbes 400 individuals, this is not an issue, as we can match the individuals on the list to their Pitchbook profiles, as explained in Appendix Section A.3. We can thus directly attribute on an individual basis to each Forbes 400 individual the accumulated capital gains they earn on these investments. For assigning the remaining Pitchbook private business capital gains, we need to rely on an imputation procedure as—with the

[40] We start the simulations in 2010, since it is the first available post-reform year for which there is a wave of the SCF available.

exception of the Forbes 400 individuals—we cannot match the identities of the Pitchbook investors to the identities of the anonymous households in the SCF. For that, we rank the population of accredited investors who are full or partial owners of a C corporation or a partnership in the SCF into 100 percentiles and distribute the cumulated private capital gains proportionally to business wealth on an annual basis. This methodology ensures heterogeneity in returns coming from the direct assignment of capital gains to the Forbes 400 individuals and from the differences in returns across the private business wealth distribution. Appendix D.3.2 explains in more detail the methodology used to carry out the wealth inequality simulations.

Although Pitchbook provides a very rich snapshot of HWNIs private investments and its returns, it does not capture the universe of HWNIs private investments in early-stage companies. Thus, we miss part of the private capital gains HNWIs earn in this market. This is particularly relevant in this section, as we aim to quantify the overall effect of the increasing participation of HNWIs in early-stage markets on wealth inequality. To overcome this challenge, we rescale the private and counterfactual public capital gains we derive from Pitchbook to get a more realistic aggregate number for capital gains. For that, we use as a multiplier the gap we observe between the total excluded QSBS realized gains (152 billion USD) officially reported by the IRS (Abdulrauf et al., 2025), and the estimate of excluded QSBS gains (28 billion USD) we derive from Pitchbook over 2012-2022 (see Appendix Figure D3. This implies an approximate rescaling of 5.4 times the Pitchbook capital gains. In what follows, we present results with and without rescaling capital gains. Appendix D.3.2 explains in more detail the methodology used to carry out the rescaling of capital gains.

5.2 Results

Figure 8 compares the baseline top 0.5% wealth share to the counterfactual top 0.5% wealth share over the period 2010-2022 under the counterfactual scenario without excess returns that is, under the scenario where they would have invested their early-stage investments in the public stock market instead. Given that wealth shares are slow-moving variables, the difference between the baseline top 0.5% wealth share and the counterfactual top 0.5% wealth share is not very large in absolute terms—less than 1 percentage point difference in 2022—even for the version using rescaled capital gains. However, the effects are much larger when quantifying the contribution of this return channel for the overall growth of the wealth share over the 2010-2022 period. Table 2 compares the baseline growth rate of the top 0.5% wealth share between 2010 and 2022, with the counterfactual growth rate of the top 0.5% wealth share. As expected, the growth rate of the top 0.5%

wealth share is lower in the counterfactual scenario in which HNWIs would have invested their money in the public stock market (6.46% without rescaling, 5.04% with rescaling), than the baseline scenario in which they invested their money in early-stage markets (6.80%). Private capital gains from early-stage investing thus account for 5% (25.88%) of the overall growth of the top 0.5% wealth share over the period 2010-2022 without (with) rescaled capital gains.

Figure 8: Top 0.5% wealth share: Baseline vs. Counterfactuals



Source: Survey of Consumer Finances, Pitchbook.
Notes: This figure compares the baseline top 0.5% wealth share to the counterfactual top 0.5% wealth share over the period 2010-2022 under the counterfactual scenario of no private capital gains but with counterfactual public capital gains, using NASDAQ 100 as the public stock market index. We present the counterfactual wealth shares with and without rescaling capital gains. The baseline wealth distribution series are derived by combining the Survey of Consumer Finances (SCF) with the Financial Accounts (FA) and the Forbes 400 rich lists following the Distributional Financial Accounts methodology developed by Batty et al. (2020).

The heterogeneity in private returns across the wealth distribution exacerbates the effect, as with homogeneous returns private capital gains from early-stage investing would only account for 0.88% (10%) of the overall growth of the top 0.5% wealth share over the period 2010-2022 without (with) rescaled capital gains (Appendix Table D2). Our results are robust both in terms of direction and magnitude to the use of other public stock market indices, as well as the Barclay Hedge Fund and the FANG+ index (Appendix Table D3). Taken together, these findings are consistent with private business wealth being highly concentrated at the top of the wealth distribution and the return premium over public markets we document in Section 3.2.

We further analyze whether the contribution of the excess return channel to the overall growth of the wealth share over the 2010-2022 period is heterogeneous across the wealth distribution. Figure 9 shows that indeed the excess return channel is more important in

Table 2: Counterfactuals: Growth of Top 0.5% Wealth Share (2010-2022)

		W/o excess returns NASDAQ	
	Baseline	W/o rescaling	W/ rescaling
Absolute growth rate	6.80%	6.46%	5.04%
Share of growth rate	/	5.00%	25.88%

Sources: Pitchbook, SCF, FA, Forbes 400, Bloomberg.
Notes: This table compares the baseline growth rate in the top 0.5% wealth share between 2010 and 2022, to the counterfactual growth rate in the top 0.5% wealth share, had HNWIs invested the money in the NASDAQ 100 and not in early-stage markets. The table also reports the share of growth in the top 0.5% wealth share explained by excess returns. This share is derived as one minus the ratio between the counterfactual growth rate in the wealth share without excess returns and the baseline growth rate in the wealth share. We present the counterfactual growth rates with and without rescaling capital gains.

explaining the growth in the wealth share of the billionaires, than that of the millionaires. In particular, for the top 10 to 0.5% and top 0.5 to 0.1% wealth groups, private capital gains account for 1% and 3% (4% and 11%) of the overall growth of their wealth share over the 2010-2022 without (with) rescaled capital gains, respectively. Instead, for the top 0.1% to Forbes 400 and for the Forbes 400 wealth groups, private capital gains account for approximately 4% (15%) of the overall growth of their wealth share over the 2010-2022 without (with) rescaled capital gains, respectively. Our results are robust both in terms of direction and magnitude to the use of other public stock market indices, as well as the Barclay Hedge Fund and the FANG+ index (Appendix Tables D4 and D5).

Finally, we want to emphasize that the increasing participation of HNWIs in private capital markets has contributed to the rise in wealth concentration over the 2010-2022 period because of the return premium over public markets we uncover in Section 3.2. If instead public markets would have outperformed private capital markets during that same period of time, wealth concentration would have—all else equal—declined. In Table 3, we provide supporting evidence for this argument by running an alternative counterfactual simulation to the one we run in Table 2. In particular, we now assume that HNWIs would have earned between 2010 and 2022 the excess returns of 2023 relative to the NASDAQ 100 and thus replace the actual private capital gains by the counterfactual private capital gains such that the excess returns are like the ones in 2023. We pick the gains of 2023 to run this alternative counterfactual, as this is the first year since 2010 in which early-stage markets clearly underperformed public stock markets. We find that under this alternative scenario private capital gains from early-stage investing would have instead slowed down by 2.65% without rescaled capital gains (15.15% with rescaled capital gains) the overall growth of the top 0.5% wealth share over the 2010-2022 period. These analyses thus illustrate that the increasing participation of HNWIs in private capital markets would—all else equal—increase or decrease economic inequalities, depending on

Figure 9: Heterogeneity in Share of Growth Explained by Excess Returns across Top Wealth Groups (2010-2022)



Source: SCF, Pitchbook, FA, Forbes, Bloomberg.
Notes: This figure depicts how much of the 2010-2022 growth rate in the wealth share of each of the wealth groups within the top 10% is accounted for by excess returns (excluding private capital gains and adding counterfactual public gains based on the NASDAQ 100 public stock market index) with and without rescaling capital gains. The share of growth for every wealth group is derived as one minus the ratio between the counterfactual growth rate in the wealth share without excess returns and the baseline growth rate in the wealth share. Note that whenever the growth rates are negative (in this case, the top 1-0.5% and the top 0.5-0.1%), we report the absolute value of one minus the ratio to ensure consistency. The baseline wealth distribution series are derived by combining the Survey of Consumer Finances (SCF) with the Financial Accounts (FA) and the Forbes 400 rich lists following the Distributional Financial Accounts methodology developed by Batty et al. (2020). Appendix D.3.2 provides more details about the methodology used to estimate the baseline wealth distribution series and carry out the counterfactuals.

whether these markets outperform or underperform public markets.

Table 3: Counterfactuals: Growth of Top 0.5% Wealth Share (2010-2022, assuming 2023 excess return)

		W/o excess returns NASDAQ	
	Baseline	W/o rescaling	W/ rescaling
Absolute growth rate	6.80%	6.98%	7.83%
Share of growth rate	/	-2.65%	-15.15%

Sources: Pitchbook, SCF, FA, Forbes 400, Bloomberg.
Notes: This table compares the absolute baseline growth rate in the top 0.5% wealth share between 2010 and 2022, with the counterfactual growth rate in the top 0.5% wealth share, had HNWIs invested the money in the NASDAQ 100 and not in early-stage markets (assuming they would have obtained the excess returns of 2023 in every single year between 2010 and 2022). We present the counterfactual growth rates with and without rescaling capital gains. The table also reports the share of growth in the top 0.5% wealth share explained by excess returns. This share is derived as one minus the ratio between the counterfactual growth rate in the wealth share without excess returns and the baseline growth rate in the wealth share. Note that because in this case the counterfactual growth rate is higher than the baseline, the shares are negative.

6 Feedback Loop

To what extent is the relationship between HNWIs' increasing participation in private capital markets and rising inequality self-reinforcing? We consider this question in this last section. In particular, we evaluate whether early-stage investors who benefited from higher returns that increased their wealth, also invested disproportionately more in this market, contributing to the self-reinforcing loop.

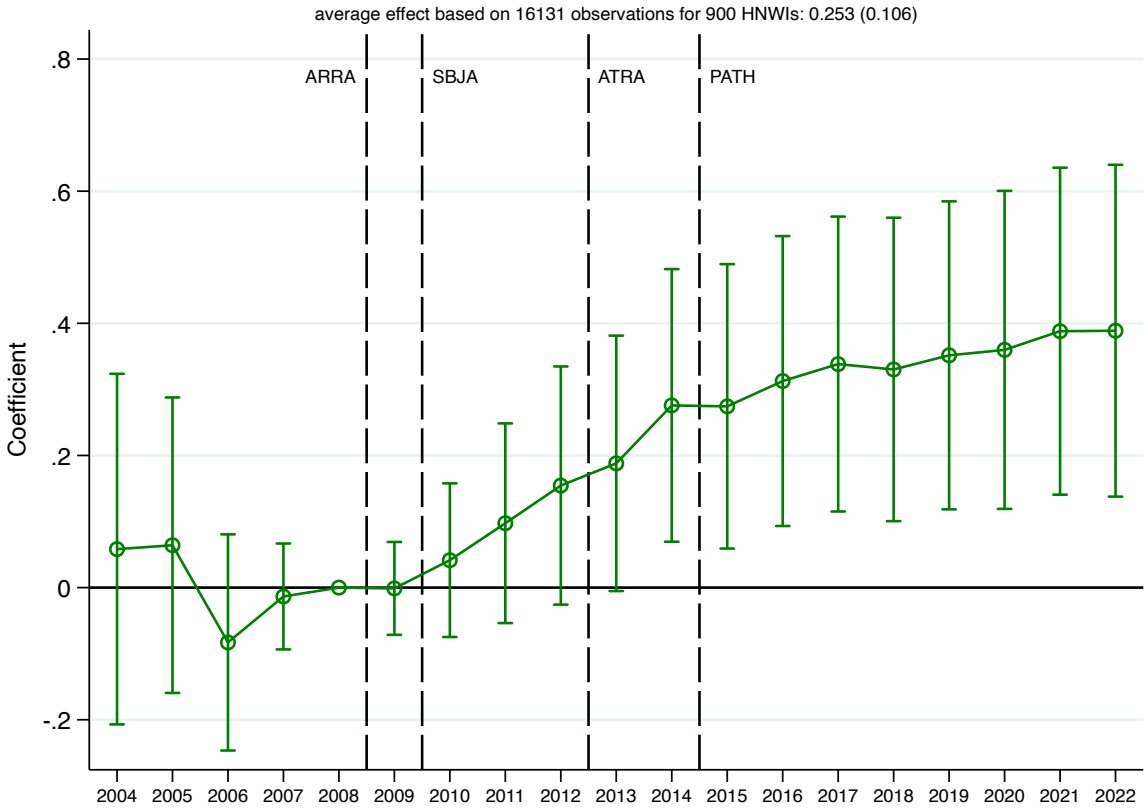
We start by using a similar firm-level design as in Section 4.2 and compare the valuation of QSBS and non-QSBS eligible companies before and after the QSBS reforms. Figure **??** shows that QSBS-eligible firms obtain on average a 32.6% higher valuation in the post-reform period relative to non-QSBS eligible firms. We then build upon this result and show that these existing QSBS investors—who benefited from higher returns that increased their wealth—also became more active investors in the early-stage market. For that, we implement an investor-level design and restrict the analysis to investors who invest in early-stage companies at least once during the pre-reform period (2004-2008). We run the following regression specification:

$$\ln Y_{i,t} = \beta_t(\ln X_{i,pre} \times \mathbb{1}_{t>2008}) + \alpha_i + \gamma_t + \epsilon_{i,t}, \tag{4}$$

where $Y_{i,t}$ is the cumulated amount invested in early-stage deals by investor i in year

t, $X_{i,pre}$ stands for the share of investments in QSBS-eligible companies out of total early-stage investments in or before 2008, and $\mathbb{1}_{t>2008}$ is a dummy variable equal to 1 for the post-reform period 2009-2022 and to 0 for the pre-reform period 2004-2008. We also include investor α_i and year fixed effects γ_t. Figure 10 shows that investors with a 100% share of their total early-stage investments in QSBS-eligible companies in or before 2008 invest on average 25.3% more in early-stage deals in the post-reform period relative to investors who did not invest at all in QSBS-eligible companies in or before 2008.

Figure 10: Difference in Differences Estimates from Equation (4)



Source: Pitchbook.
Notes: This figure depicts the coefficient estimates β_t in Equation (4), where the dependent variable measures the log cumulated amount invested in early-stage deals by each investor. The bars represent 95% confidence intervals. Standard errors are clustered at the investor level.

7 Conclusion

This paper studies the interplay between the growth in private capital markets, the shrinking in public markets, and the rise in income and wealth concentration over the last two decades in the U.S. For that, we rely on novel data sources, and exploit an exemption from capital gains tax for investments in early-stage companies introduced during the

financial crisis as a quasi-experimental shock increasing the participation of HNWIs in private capital markets.

We obtain three main findings. First, we document that the share of financing raised by early-stage companies from U.S.-based high-net-worth individuals (HNWIs) tripled from 2004 to 2022. Second, exploiting both company- and state-level variation in exposure to the expanded federal capital gains tax exclusion on qualified small business stock (QSBS), we find that QSBS-eligible companies' probability of staying private increased by 3.5 percentage points, and that the average income gap between HNWIs and other income earners increased by 7.2%. Finally, using counterfactual simulations, we find that HNWIs' excess returns on these investments accounted for 2% and 14% of the growth in the top 1% share of post-tax income and wealth, respectively, from 2010 to 2022. The rise in economic inequalities further rises private capital markets activity, suggesting the existence of a feedback loop among the two.

Taken together, our paper reveals that private capital market dynamics may have non-negligible distributional implications due to the differences in portfolio composition and in returns across asset classes across the income and wealth distribution. Our analyses are based on reduced-form approaches and partial equilibrium counterfactual simulations. Further research is needed to quantify the distributional implications of changing private capital markets taking a general equilibrium approach. Our paper has important insights in light of the current policy debate of democratizing private markets to retail investors and it's implications for economic inequalities in the U.S. If we widen access to private capital markets, it can increase or decrease inequality depending on the deals individuals have access to and their outperformance of private relative to public markets.

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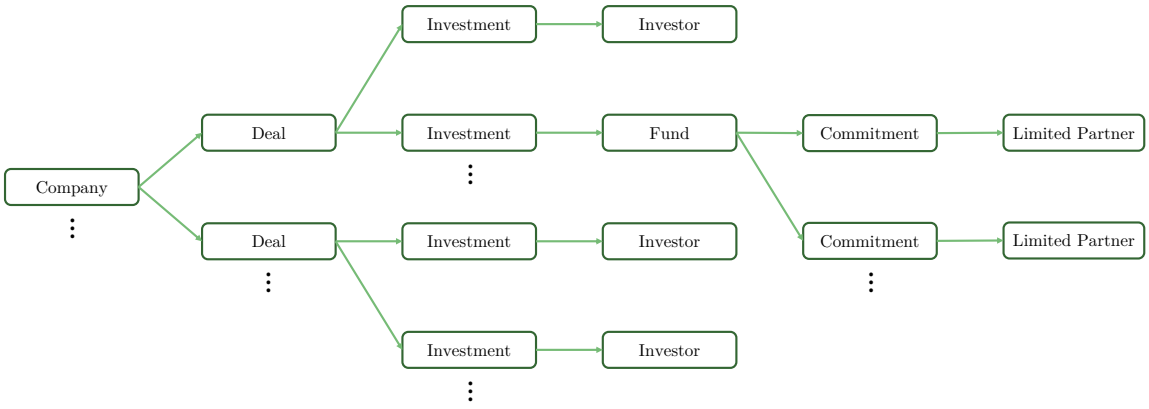
Appendix A: Private Capital Market Activity

In this appendix, we describe our procedure to clean the data on private capital market activity from Pitchbook. Section A.1 first outlines the structure of the data. Section A.2 then details our classification of private capital market deals, while Section A.3 details our classification of investors. Section A.4 next explains how we calculate the amount invested by each investor in each deal. Finally, Section A.5 compares Pitchbook's coverage to that of Preqin, an alternative data provider, while Section A.6 reports additional descriptive evidence on high-net-worth individuals' participation in private capital markets.

A.1 Structure of Data

The data includes datasets on companies; on the deals as part of which each company raised financing; on the investments made as part of each deal; and on the investors making the investments, including whether or not each investor is a fund intermediating on behalf of its limited partners. Figure A1 illustrates the structure of the data and how we link together the different datasets. First, by linking companies to deals, we identify when each company raised financing. Then, by linking the deals to the investments made as part of them and the investments to the investors making them, we identify the investors investing in each deal. Finally, for the investments intermediated by funds, we link the funds to the commitments made to them and the commitments to the limited partners making them, thereby identifying the limited partners ultimately investing in each deal.

Figure A1: Structure of the Data on Private Capital Market Activity from Pitchbook



Sources: Pitchbook.
Notes: This figure illustrates the structure of the data on private capital market activity from Pitchbook. The vertical ellipses indicate where there could be additional nodes of the data tree.

A.1.1 Versions

To ensure maximal coverage of companies, we merge two versions of the data. One version (updated as of August 19, 2025) contains information only on companies that have ever raised early-stage or private equity financing or that are less than two years old. The other version (updated as of November 23, 2023) also contains information on companies that have only ever raised debt financing, gone bankrupt, become publicly listed, or been acquired by another company. When a company appears in both of our versions of the data, we consider its information only from the more recent one.[41]

A.1.2 Companies

We observe 1,027,479 companies, 681,217 (66.3%) of which appear in the more recent version of the data. Of the 402,645 (39.2%) companies headquartered in the United States, we observe the state in which they are headquartered for all but 5,045 (1.3%).

A.1.3 Deals

We observe at least one deal for all but 15,151 (1.5%) companies. Excluding these, the median number of deals per company is 2, and the mean is 2.3. Of the 2,373,978 deals, we drop 48,883 (2.1%) that were never or are not yet completed. We also drop a further 224,751 (9.5%) with unknown completion dates, since our analysis will depend on their timing. This leaves us with 2,100,344 deals.

A.1.4 Investments

Across the remaining deals, we observe 2,822,674 (82.3%) equity investments and 601,986 (17.6%) debt investments. From the investors named in the synopses of capitalization deals (i.e., the only deal type for which Pitchbook does not separately record the investment made by each investor), we infer an additional 4,823 (0.1%) equity investments.

A.1.5 Intermediated Investments

Of the 3,429,483 total investments, 3,042,648 (88.7%) were investments directly into companies, while 386,835 (11.3%) were made by funds intermediating on behalf of their

[41] Both versions miss Pitchbook's data on companies two years old or older that never raised financing.

limited partners. The latter correspond to 4,792,572 investments ultimately attributable to the funds' limited partners, which we identify based on the investors that have committed to each fund. This leaves us with 7,835,220 total (i.e., direct and intermediated) investments.

A.2 Classification of Private Capital Market Deals

We distinguish between four categories of private capital market deals: early-stage, private equity, private debt, and real asset deals. Early-stage deals refer to equity investments in startups. Private equity deals refer to equity investments in more mature companies. Private debt deals refer to debt investments by non-bank lenders, other than in the form of bonds. Finally, real asset deals refer to acquisitions of real estate, infrastructure, or natural resources. Consistent with the categorization used by private investment professionals (see https://www.preqin.com/academy/lesson-2-private-capital/what-is-private-capital), our classification is based on Pitchbook's own classifications of companies, deals, and lenders.

A.2.1 Company and Deal Types

Based on the descriptions and the primary industry codes of acquired companies, we first identify deals that involve other companies' acquisitions of real estate, infrastructure, or natural resources. Based on the deal types provided by Pitchbook, we then classify the remaining deals as early-stage, private equity, debt, or other deals. Early-stage deals refer to capitalization, crowdfunding, grant, angel, accelerator/incubator, seed, early-stage venture capital, and later-stage venture capital deals. Private equity deals refer to buyouts, private investments in public equity, growth/expansion investments, platform creations, and acquisitions of stakes in general partners. Debt deals refer to loans, bonds, convertible debt, and mezzanine financing. Finally, other deals refer to bankruptcies, public offerings, corporate mergers/acquisitions, and all other deal types that do not fit into our classification. In this way, we classify 893,911 (42.6%) deals as early-stage deals, 311,222 (14.8%) as private equity deals, 166,239 (7.9%) as debt deals, 44,947 (2.1%) as real asset deals, and 683,998 (32.6%) as other deals.

A.2.2 Lender Types

Finally, among the 601,986 debt investments (not deals), we distinguish between traditional and private debt investments based on lender types, as well as on whether or not each debt facility was structured as a bond. Specifically, we classify bank loans and bonds as traditional debt investments, while classifying all other debt investments by non-

bank lenders as private debt investments. In this way, we classify 193,189 (32.1%) debt investments as private debt, which is the asset class relevant to private capital markets.

A.3 Classification of Investors

We distinguish between two categories of investors: high-net-worth individuals and institutional investors. HNWIs refer to individuals, angel groups, and family offices, while institutional investors refer to pension funds, endowment plans, insurance companies, investment firms, accelerators/incubators, banks/lenders, corporations, and governments. Among HNWIs, we further distinguish between founders and non-founders, as well as between those ranked and those not ranked in the Forbes 400 rich list.

A.3.1 High-Net-Worth Individuals

Of the 4,792,572 intermediated investments, 47,322 (1.0%) were by HNWIs residing in the U.S. Likewise, of the 3,042,648 direct investments, 120,219 (4.0)% were by U.S. HNWIs.

A.3.2 Founders

The data from Pitchbook also contains information on companies' board members, including their founders. Using this data, we identify when U.S. HNWIs invested in companies that they themselves founded. Of their 167,541 investments, 2,796 (1.7%) were by founders.

A.3.3 Forbes 400

We match HNWIs in the data from Pitchbook to those ranked in the Forbes 400 rich list. Of the 168,664 investments made by U.S. HNWIs, 8,310 (4.9%) were by ranked ones.[42]

A.4 Calculation of Investment Amounts

To calculate the amount invested by each investor as part of each of their investments, we combine deal-level data on the total amount of financing raised as part of each deal with investment-level data on the amount of financing raised from each investor. First, we use

[42] Since multiple different HNWIs ranked in the Forbes 400 can invest through (and so be matched to) the same family office, our matching procedure increases the number of investments by U.S. HNWIs by 1,123.

information from both datasets to impute missing amounts at the deal level. Then, we calculate each investor's share of each deal and follow a similar procedure to calculate each limited partner's share of each fund. Finally, we calculate the amount invested by each investor in each deal as the product of these shares with the size of the deal.

A.4.1 Deal Size

Of the 2,100,344 deals in the deal-level data, we observe the total amount of financing raised for 1,027,295 (48.9%), the amount of equity financing for 575,530 (27.4%), and the amount of debt financing for 173,446 (8.3%). Using the identity that the total amount of financing raised equals the sum of the amounts of equity and debt financing, we impute the third unknown amount whenever we observe the other two.[43] If we instead observe only one or none of these amounts, then our imputation also considers investment-level data on the amounts of financing raised from investors.[44] In these ways, we impute the amount of equity financing raised for 323,631 (15.4%) deals, the amount of debt financing for 17,004 (0.8%) deals, and the total amount of financing for 42,319 (2.0%) deals.

A.4.2 Investor Share

When calculating each investor's share of each deal, we consider the deal's equity and debt components separately. For each component, we add up the amounts of financing raised from investors, subtract their sum from the total amount of financing, and distribute the remainder equally across all the other investors. We similarly calculate each limited partner's share of each fund by comparing the sum of the amounts committed by the fund's limited partners to the total size of the fund.

A.4.3 Investment Amount

For direct investments, we calculate the amount invested as part of each investment as the product of the size of each deal with each investor's share of the deal. For intermediated investments, we further multiply this product by each limited partner's share of each fund. As a result, the total amount of financing raised as part of each deal always equals the sum of the amounts of financing raised from the deal's investors.

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[43] We observe all three amounts for only 22,012 (1.0%) deals, of which 21,049 (95.6%) satisfy the identity.

[44] For example, if we observe only the total amount of financing raised at the deal level, then we impute the amount of debt financing as the sum of the amounts of debt financing raised from investors, and the amount of equity financing as the difference between the total amount and this sum.

A.5 Preqin

We follow a similar procedure to clean the data on early-stage and private equity investments from Preqin, an alternative data provider. We cannot compare the two data providers' coverage of private debt and real asset investments, since our version of Preqin's data (updated as of July 24, 2025) does not allow us to identify such investments.

Table A1 confirms Pitchook's claim (see https://www.pitchbook.com/compare/pitchbook-vs-preqin) that it has better coverage of private capital market activity than Preqin. In terms of the amounts of financing raised by U.S. companies from all investors, the amounts recorded by Pitchbook are moderately larger. However, in terms of the numbers of investments by U.S. high-net-worth individuals in these companies, the numbers recorded by Pitchbook are a magnitude larger. Thus, our focus on HNWIs' participation in private capital markets requires us to use Pitchbook's data rather than Preqin's.

Table A1: Pitchbook's vs. Preqin's Data on Private Capital Market Activity

	Investments in U.S. Private Capital Market Deals (2004-2022)			
	By All Investors (Billions of $)		By U.S. HNWIs (Count)	
	Pitchbook	Preqin	Pitchbook	Preqin
Total	6,535.34	5,481.27	118,343	1,181
incl. early stage	1,876.66	1,474.41	105,542	1,078
incl. private equity	4,658.68	4,006.86	12,801	103

Sources: Pitchbook, Preqin.
Notes: This table compares Pitchbook's and Preqin's data on early-stage and private equity investments in U.S. companies from 2004 to 2022. The amounts in U.S. dollars are expressed in nominal terms.

A.6 Additional Results

Table A2: U.S. HNWIs' Early-Stage Investments in U.S. Companies

	Investments			
	Count		Amount Invested	
	Number	Share	Billions of $	Share
Total	105,542	1.000	112.88	1.000
incl. individuals	78,767	0.746	70.14	0.621
incl. angel groups	14,689	0.139	13.03	0.115
incl. family offices	12,086	0.115	29.70	0.263
incl. pre-seed deals	10,278	0.097	4.42	0.039
incl. seed deals	41,404	0.392	12.15	0.108
incl. venture capital deals	53,579	0.508	95.58	0.847
incl. grant deals	281	0.003	0.73	0.007
incl. Forbes 400	5,278	0.050	14.36	0.127
incl. non-Forbes 400	100,264	0.950	98.52	0.873
incl. founders	2,464	0.023	3.64	0.032
incl. non-founders	103,078	0.977	109.24	0.968
incl. direct	86,249	0.817	107.17	0.949
incl. intermediated	19,293	0.183	5.71	0.051
incl. information technology	54,401	0.515	49.34	0.437
incl. business-to-consumer	21,177	0.201	20.48	0.181
incl. business-to-business	12,180	0.115	11.50	0.102
incl. healthcare	12,390	0.117	21.06	0.187
incl. financial services	3,545	0.034	5.67	0.050
incl. materials/resources	1,849	0.018	4.82	0.043

Sources: Pitchbook.
Notes: This table summarizes U.S. high-net-worth individuals' early-stage investments in U.S. companies from 2004 to 2022. Pre-seed deals include angel capitalization, crowdfunding, angel, and accelerator/incubator deals. The amounts in U.S. dollars are expressed in nominal terms.

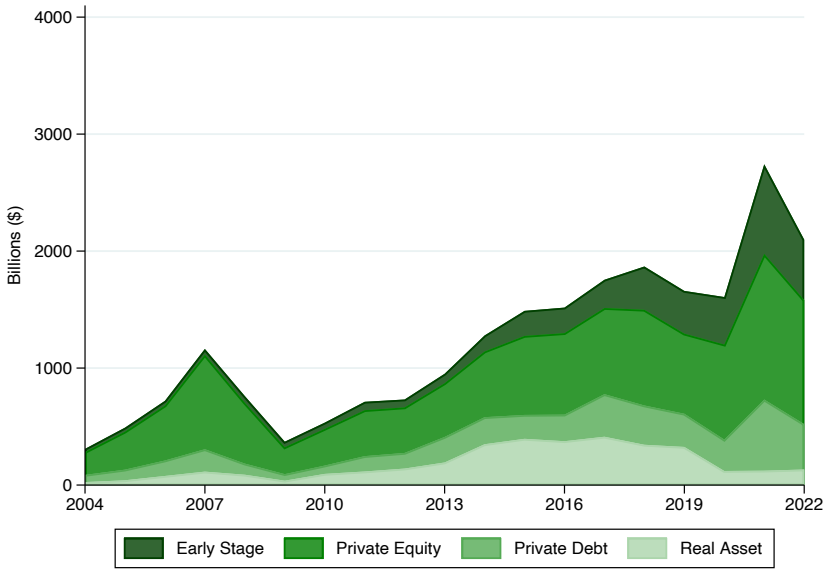
Table A3: U.S. Institutional Investors' Early-Stage Investments in U.S. Companies

	Investments			
	Count		Amount Invested	
	Number	Share	Billions of $	Share
Total	1,095,641	1.000	1228.49	1.000
incl. pension funds	337,616	0.308	123.27	0.100
incl. endowment plans	199,189	0.182	126.44	0.103
incl. insurance companies	59,382	0.054	16.61	0.014
incl. investment firms	331,340	0.302	646.96	0.527
incl. accelerators/incubators	55,750	0.051	15.13	0.012
incl. banks/lenders	7,134	0.007	19.02	0.015
incl. corporations	41,747	0.038	137.05	0.112
incl. governments	62,576	0.057	142.66	0.116
incl. pre-seed deals	54,366	0.050	3.01	0.002
incl. seed deals	147,920	0.135	49.49	0.040
incl. venture capital deals	847,006	0.773	1034.96	0.842
incl. grant deals	46,349	0.042	141.03	0.115
incl. direct	334,333	0.305	905.20	0.737
incl. intermediated	761,308	0.695	323.29	0.263
incl. information technology	544,268	0.497	468.19	0.381
incl. business-to-consumer	135,414	0.124	183.84	0.150
incl. business-to-business	119,398	0.109	156.72	0.128
incl. healthcare	235,052	0.215	316.62	0.258
incl. financial services	28,444	0.026	52.02	0.042
incl. materials/resources	33,065	0.029	46.40	0.038

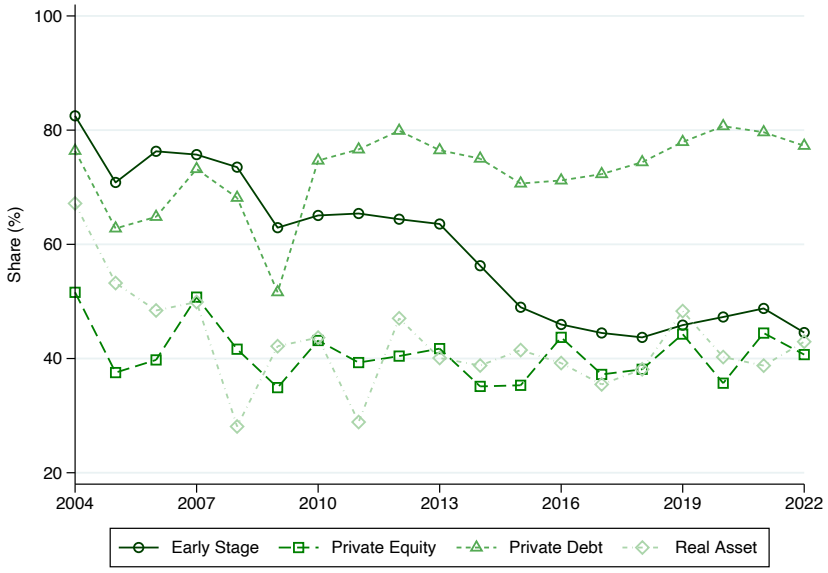
Sources: Pitchbook.
Notes: This table summarizes U.S. institutional investors' early-stage investments in U.S. companies from 2004 to 2022. Pre-seed deals include angel capitalization, crowdfunding, angel, and accelerator/incubator deals. The amounts in U.S. dollars are expressed in nominal terms.

Figure A2: Investments in Private Capital Market Deals Globally

(a) Investments in All Companies

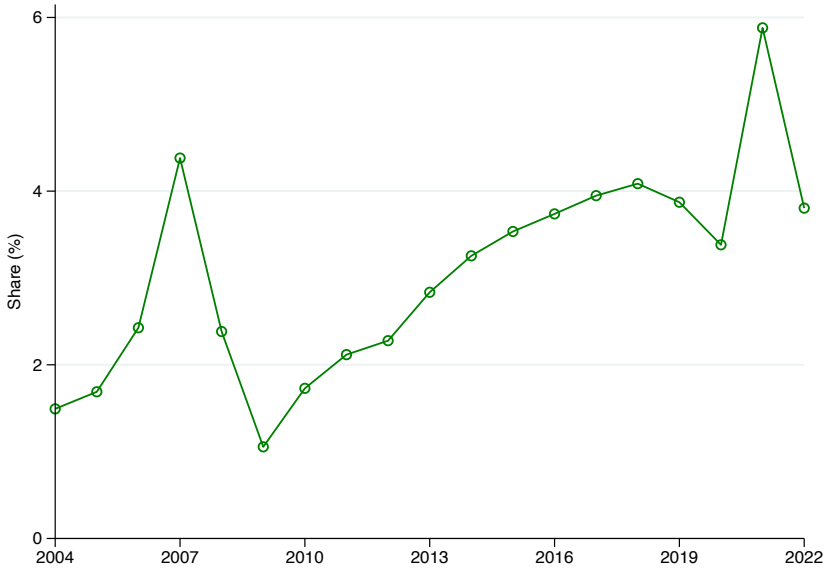


(b) U.S. Companies' Share of Investments



Sources: Pitchbook.
Notes: This figure describes the evolution of investments in private capital market deals for U.S. companies as a share of U.S. gross domestic product. Private capital market deals refer to early-stage, private equity, private debt, and real asset deals.

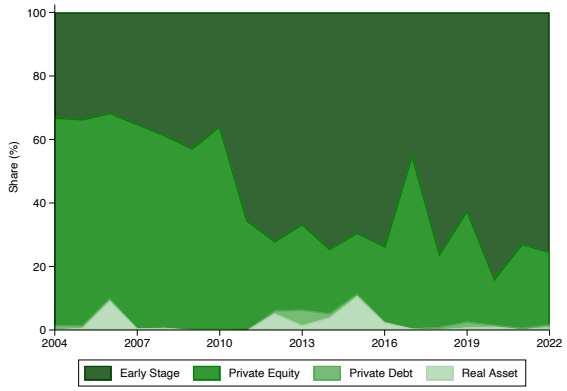
Figure A3: Investments in U.S. Private Capital Market Deals as a Share of U.S. GDP



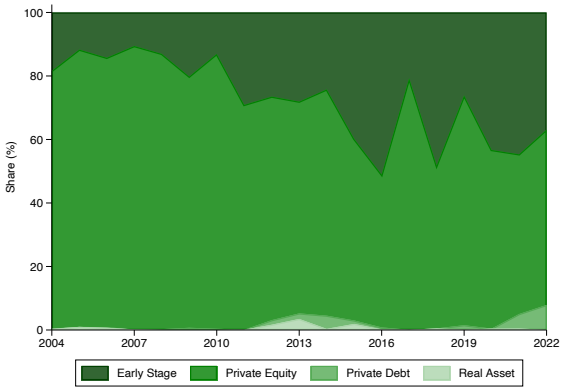
Sources: Pitchbook, U.S. Bureau of Economic Analysis.
Notes: This figure depicts the evolution of investments in private capital market deals for U.S. companies as a share of U.S. gross domestic product. Private captial market deals refer to early-stage, private equity, private debt, and real asset deals.

Figure A4: Asset Composition of U.S. HNWIs' Investments in Private Capital Markets

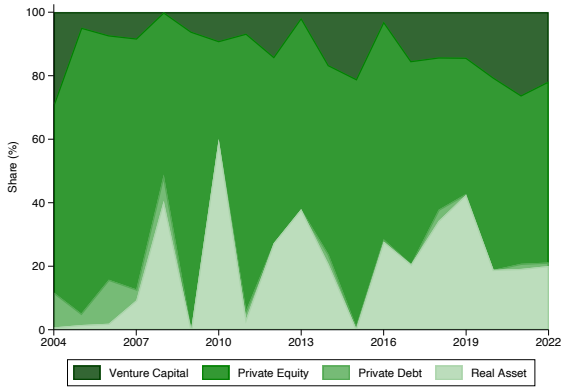
(a) All Investments



(b) Intermediated Investments



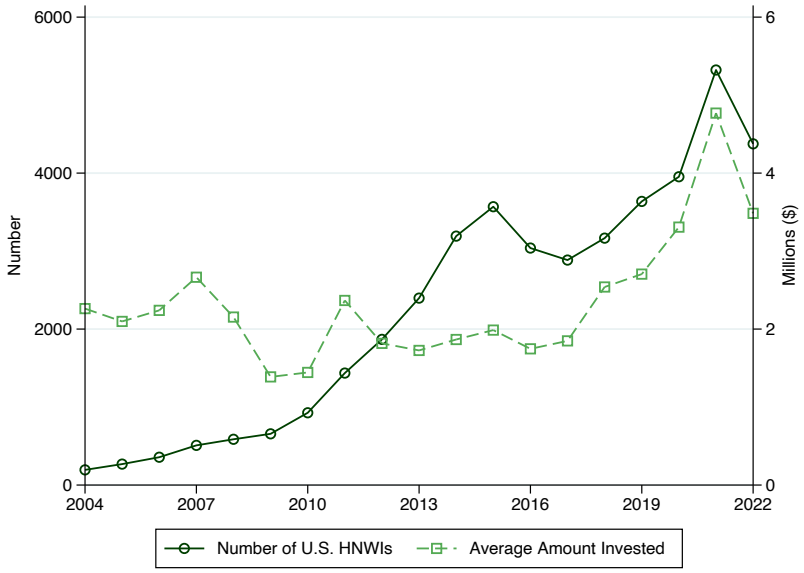
(c) Fund Commitments



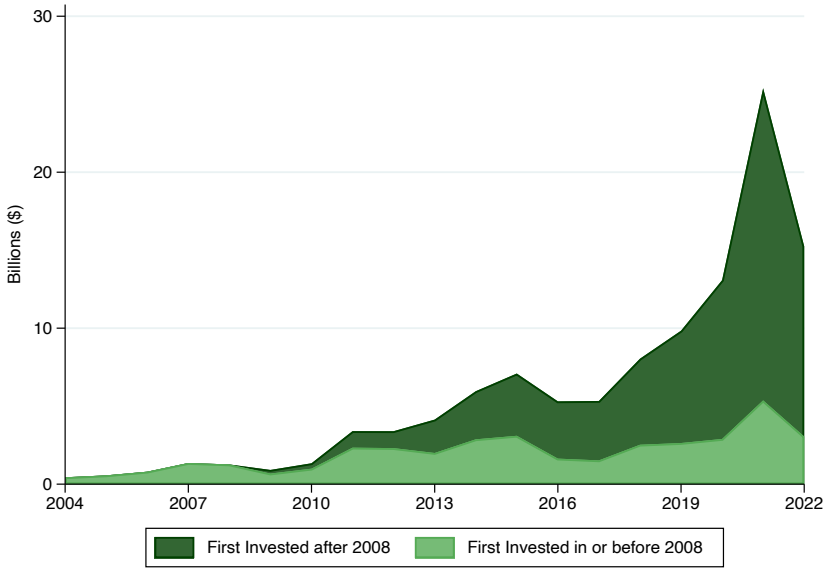
Sources: Pitchbook.
Notes: This figure describes the evolution of the asset composition of U.S. high-net-worth individuals' investments in private capital markets. Panel (a) considers both their direct and intermediated investments in U.S. companies, Panel (b) considers only their intermediated investments in U.S. companies, and Panel (c) considers their commitments to different categories of funds. The deal categories in Panels (a)-(b) are those described in Appendix Section A.2, while the fund categories in Panel (c) are based on Pitchbook's own classification of funds.

Figure A5: U.S. HNWIs' Participation in U.S. Early-Stage Capital Markets

(a) U.S. HNWIs Investing in Early-Stage U.S. Companies

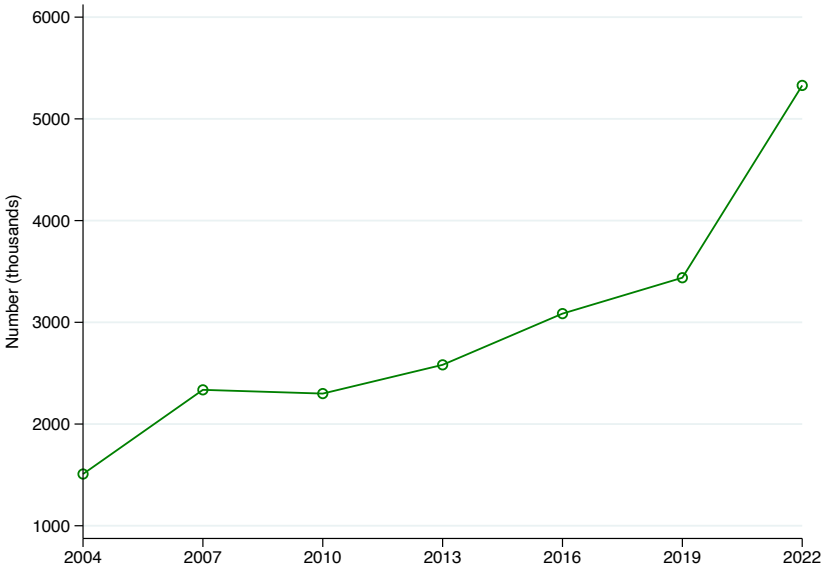


(b) U.S. HNWIs' Early-Stage Investments in U.S. Companies



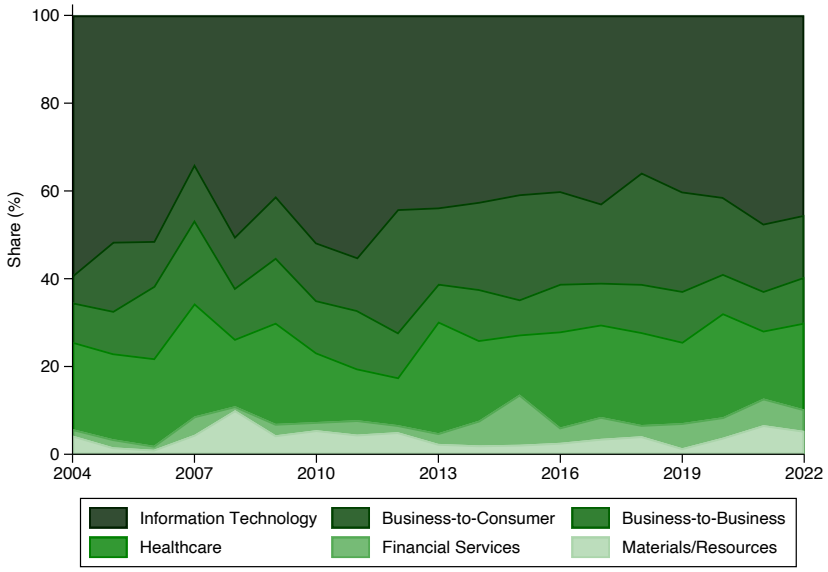
Sources: Pitchbook.
Notes: This figure describes the evolution of U.S. high-net-worth individuals' participation in U.S. early-stage capital markets. Panel (a) plots the number of distinct U.S. HNWIs investing in early-stage U.S. companies, as well as their average amount invested. Panel (b) distinguishes the early-stage investments in U.S. companies by U.S. HNWIs who made their first investment only after 2008 from those by U.S. HNWIs who made their first investment in or before 2008. The values plotted on the right axis in Panel (a) and those plotted in Panel (b) are expressed in nominal terms.

Figure A6: Number of U.S. Accredited Investors with Private Business Wealth



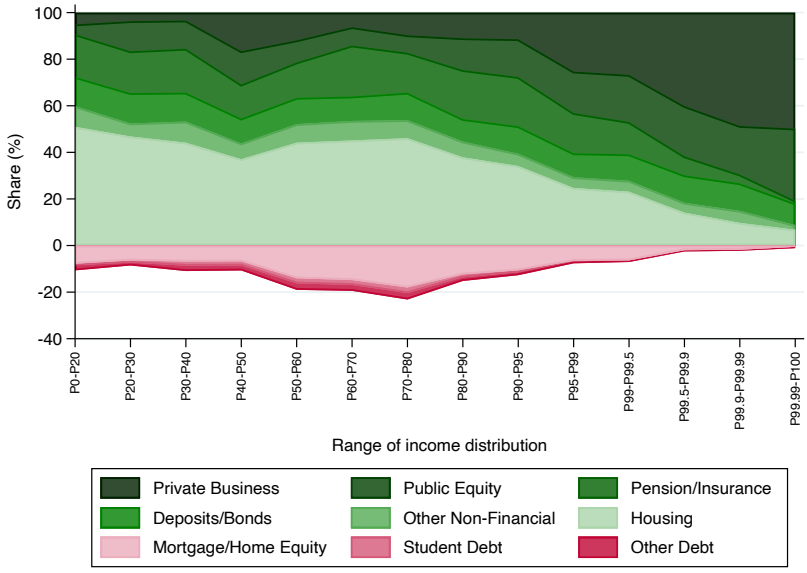
Sources: Survey of Consumer Finances.
Notes: This figure describes the evolution of the number of U.S. accredited investors who report having private business wealth. We define private business wealth as wealth in the form of partnerships or other private corporations (e.g., C corporations).

Figure A7: Sector Composition of U.S. HNWIs' Early-Stage Investments in U.S. Companies



Sources: Pitchbook.
Notes: This figure describes the evolution of the sector composition of U.S. high-net-worth individuals' early-stage investments in U.S. companies.

Figure A8: Asset Composition of Wealth along the U.S. Income Distribution, 2022



Sources: Survey of Consumer Finances.
Notes: This figure describes the asset composition of wealth along the U.S. income distribution in 2022, based on household-level information from the 2022 wave of the SCF.

Appendix B: Return Methodology

In this appendix, we describe our procedure to clean the data on companies' valuations from Pitchbook, as well as our methodology to calculate returns. Section B.1 first describes how we clean the valuation data, impute valuations when they are missing, and construct alternative samples of valuations to evaluate the robustness of our analyses. Section B.2 then describes how we calculate returns. Finally, Section B.3 reports additional descriptive evidence on high-net-worth individuals' returns on their early-stage investments.

B.1 Valuations

To calculate the return yielded by each investment in each company in each year, we first need to calculate the history of the rate of return yielded by the company's equity based on the valuations that we either observe or impute for it.

B.1.1 Observed Valuations

We obtain the valuation history of each company from Pitchbook's deal-level data. Whenever a company raises new financing, its investors and management must agree on its new valuation. By comparing the company's new pre-money valuation (before accounting for the equity financing that it raised as part of its new deal) to its previous post-money valuation (after accounting for the equity financing that it raised as part of its previous deal), we can differentiate between valuation growth due to new equity financing (which dilutes existing investors' shares between deals) and that due to organic growth.

Since our analysis of returns starts in 2004, we consider the 828,845 U.S. deals that were completed between 2004 and 2025. Of these, we obtain the pre-money valuation of the company raising financing from 102,666 (12.4%) deals and its post-money valuation from 135,784 (16.4%) deals. Among the latter, we ignore the information on these valuations from 33,678 (24.8%) deals for which Pitchbook only estimates the company's post-money valuation rather than directly sourcing it, since Pitchbook is not transparent about its estimation procedure.

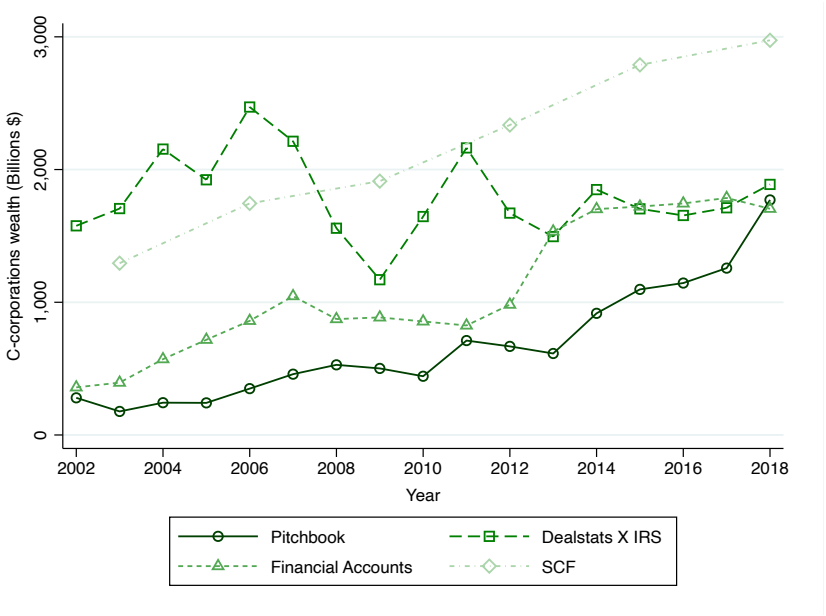
Among the 102,442 remaining deals with information on the company's actual pre-money valuation and/or post-money valuation, 10,537 (10.3%) have information on only one of these two valuations. In these cases, we calculate the other valuation using the identity that the amount of equity financing raised as part of a deal equals the difference between the company's post- and pre-money valuations. For example, if we observe only the

company's pre-money valuation, then we calculate its post-money valuation as the sum of its pre-money valuation with the amount of equity financing that it raised.

Finally, 44,535 (5.4%) deals identify when a company went bankrupt but do not specify its post-bankruptcy valuation. Following Korteweg and Sorensen (2010), we assume that the company's post-bankruptcy valuation equals 10% of its previous post-money valuation. Our baseline sample thus consists of 146,977 observed valuations and bankruptcies.

Appendix Figure B1 compares our valuation estimate using Pitchbook's deal-level data, with that of alternative data sources.

Figure B1: Comparison of Private Business Wealth across Alternative Sources



Source: Campbell and Robbins, 2025, Pitchbook.
Notes: This figure compares the value of private business wealth in the U.S. over the period 2002-2018 across alternative data sources. We restrict the analysis to C-corporations wealth, as this is mainly the type of private business wealth we will be focusing on this paper and there is information available about this type of private wealth that we can use to carry out the comparisons. All other series except for the Pitchbook series (i.e., Dealstats X IRS, Financial Accounts, Survey of Consumer Finances) are taken from Campbell and Robbins, 2025. According to Campbell and Robbins, 2025, the difference in trend between their estimate based on Dealstats X IRS and the FA and SCF estimates is likely due to their methodology of excluding firms with sales above $50 million, an adjustment that grows in magnitude in the later years of their sample. The Pitchbook series is derived by considering observed valuations and bankruptcy valuations for active and private U.S. corporations with at least one observed valuation. We do not carry any interpolation, we just extrapolate observed valuations forward until the next valuation, or until company becomes bankrupt, public, or acquired.

B.1.2 Imputed Valuations

To calculate the returns on (equity) investments in a company, we need to observe the company's valuation on at least two different dates. The large number of missing

valuations in the data therefore poses a challenge. To overcome this challenge, we impute the valuation of each company i in every year y from its founding to 2025 (or until its last deal, if that deal was a bankruptcy, public offering, or acquisition, indicating the end of its private life).[45] To that end, we first regress each company i's log post-money valuation at date t on company fixed effects and triple-interacted sector s × stage a × year y fixed effects, using only the deals for companies whose valuation we observe at least twice:

$$\ln(\text{PostValuation}_{i,t}) = \alpha_i + \beta_{s(i),a(i,t),y(t)} + \epsilon_{i,t} \tag{B1}$$

We classify companies into three aggregated sectors, with each company's sector s fixed over time: information technology; business-to-business and business-to-consumer products and services; and all other sectors (including financial services, healthcare, and materials/resources). We further classify companies into eight stages of maturity based on the number of times that they raised financing by date t, with each company's stage a changing over time: one, two, three, four, five, six, seven, and eight or more times.

After estimating Equation (B1), we predict the log post-money valuation of each company i in every year y as $\widehat{\beta}_{s(i),a(i,t),y(t)}$. We then adjust this prediction to account for relevant information on the company's most recently observed valuation, adding $\widehat{\epsilon}_{i,t'(i,t)}$ to it, where t' is the most recent date before t at which we can estimate the residual $\widehat{\epsilon}_{i,t}$ for company i. Next, we exponentiate the adjusted prediction $\widehat{\beta}_{s(i),a(i,t),y(t)} + \widehat{\epsilon}_{i,t'(i,t)}$ to impute the company's post-money valuation in every year in which it is missing. Finally, to impute the company's pre-money valuation, we subtract the amount of equity financing that the company raised during that year from its imputed post-money valuation.

This imputation procedure allows us to consider at least two valuations for 339,474 companies, a magnitude more than the 57,656 companies for which we only observe at least two valuations.

B.1.3 Alternative Samples

Our baseline sample of valuations consists of both observed and imputed valuations. We also consider two alternative samples, each addressing a different way in which our baseline sample may make companies appear overvalued.

Imputed Bankruptcies. To account for the potential incompleteness of Pitchbook's bankruptcy records, we impute bankruptcies for companies that appear inactive. Specifically, if a company last raised financing in or before 2022, then we assume that it went

[45] If we observe multiple deals for a company during a given year, we impute the company's valuation as of every deal for which its valuation is missing during that year.

bankrupt three years after its last deal (as long as that deal was not a bankruptcy, public offering, or acquisition). For example, we assume that a company that last raised financing in 2013 went bankrupt in 2016, that one that last raised financing in 2022 went bankrupt in 2025, and that one that last raised financing in 2023 has not gone bankrupt.

Haircut Valuations. We also account for the potential overvaluation of companies that have achieved "unicorn" status (i.e., have a valuation exceeding $1 billion), as documented by Gornall and Strebulaev (2020) and Gahng (2023). We apply a haircut to all of the valuations of a company after it first achieved this status, based on the main estimate from Gahng (2023) that the average unicorn's post-money valuation is overstated by 56%. To transform this estimate into a haircut that we apply to each pre-money valuation of each unicorn, we first use the identity that a unicorn's post-money valuation equals the sum of its pre-money valuation and the amount of new equity financing that it raised:

$$\frac{\text{PostValuationTrue}_{i,t}}{\text{PostValuation}_{i,t}} = \frac{\text{PreValuationTrue}_{i,t} + \text{EquityFinancing}_{i,t}}{\text{PostValuation}_{i,t}} = \frac{1}{1.56}$$

$$\implies \frac{\text{PreValuationTrue}_{i,t}}{\text{PreValuation}_{i,t}} = \left(\frac{1}{1.56} - \frac{\text{EquityFinancing}_{i,t}}{\text{PostValuation}_{i,t}}\right) \times \frac{\text{PostValuation}_{i,t}}{\text{PreValuation}_{i,t}} \tag{B2}$$

Equation (B2) reflects the haircut that we would apply to the unicorn's pre-money valuation, as long as it were positive. However, since it is possible for this equation to be negative (even though a negative valuation is theoretically impossible), we instead take the maximum of the equation and a number close to zero:

$$1 - \text{Haircut}_{i,t} \coloneqq \max\left\{\frac{1}{1.56} - \frac{\text{EquityFinancing}_{i,t}}{\text{PostValuation}_{i,t}} \times \frac{\text{PostValuation}_{i,t}}{\text{PreValuation}_{i,t}}, 10^{-7}\right\} \tag{B3}$$

Finally, we calculate the unicorn's haircut pre-money valuation by multiplying its actual pre-money valuation by Equation (B3), while calculating its haircut post-money valuation by adding the amount of new equity financing to its haircut pre-money valuation:

$$\text{PreValuationHaircut}_{i,t} \coloneqq (1 - \text{Haircut}_{i,t}) \times \text{PreValuation}_{i,t} \tag{B4}$$

$$\implies \text{PostValuationHaircut}_{i,t} \coloneqq \text{PreValuationHaircut}_{i,t} + \text{EquityFinancing}_{i,t} \tag{B5}$$

B.2 Returns

We now use the valuation history of each company, as constructed in Appendix Section B.1, to calculate returns. We first calculate rates of return at the company level, before then translating them into returns at the investment level.

B.2.1 Company-Level Returns

For each company i, we first calculate the number of days D_{i,t_1,t_2} between consecutive deals completed on dates t_1 and t_2, respectively, where $t_1 < t_2$. Let r_{i,t_1,t_2} be the percent change from i's previous post-money valuation at t_1 to its new pre-money valuation at t_2 (see Korteweg and Sorensen, 2010):

$$r_{i,t_1,t_2} := \frac{\text{PreValuation}_{i,t_2} - \text{PostValuation}_{i,t_1}}{\text{PostValuation}_{i,t_1}} \tag{B6}$$

Assuming that the company's valuation compounds at a constant daily rate between the two dates, we can convert r_{i,t_1,t_2} from Equation (B6) into the daily compounded rate $r_{i,t}$ for company i at date t:

$$\begin{aligned}
(1 + r_{i,t})^{D_{i,t_1,t_2}} &:= 1 + r_{i,t_1,t_2} \\
\implies \ln(1 + r_{i,t}) &= \frac{\ln(1 + r_{i,t_1,t_2})}{D_{i,t_1,t_2}} \\
\implies r_{i,t} &= -1 + \exp\left[\frac{\ln(1 + r_{i,t_1,t_2})}{D_{i,t_1,t_2}}\right]
\end{aligned} \tag{B7}$$

We use $r_{i,t}$ to construct the history of the daily rate of return yielded by the equity of each company i on every date t from 1 January 2004 to 31 December 2023. We further assume that this rate equals zero on each date that either precedes the completion date of the company's first deal or follows the completion date of its last deal during this period.

B.2.2 Investment-Level Returns

We calculate the rate of return that investment j in company i yielded during each calendar year y as:

$$\begin{aligned}
1 + r_{j,y}^{Annual} &:= \prod_{t=1/1/y}^{12/31/y} 1\{Entry_j \leq t < Exit_j\} \times (1 + r_{i(j),t}) \\
\implies r_{j,y}^{Annual} &= -1 + \prod_{t=1/1/y}^{12/31/y} 1\{Entry_j \leq t < Exit_j\} \times (1 + r_{i(j),t}),
\end{aligned} \tag{B8}$$

where $r_{i,t}$ is defined as in Equation (B7), $Entry_j$ is the date on which investment j was entered, and $Exit_j$ is the date on which investment j was exited. If either $Entry_j$ or $Exit_j$ took place during year y, then the annual rate of return $r_{j,y}^{Annual}$ in Equation (B8) would

not be an annualized rate of return. However, we can begin translating this annual rate of return into an annualized rate by first defining $D_{j,y} = \sum_{t=1/1/y}^{12/31/y} 1\{Entry_j \leq t < Exit_j\}$ as the number of days during year y for which investment j was held, and then calculating j's daily rate of return during y as follows:

$$(1 + r_{j,y})^{D_{j,y}} \coloneqq 1 + r_{j,y}^{Annual}$$

$$\implies r_{j,y} = -1 + \exp\left[\frac{\ln(1 + r_{j,y}^{Annual})}{D_{j,y}}\right] \tag{B9}$$

Given also the total number of days $D_y^{max} \in \{365, 366\}$ in that year, we can finally annualize $r_{j,y}$ from Equation (B9) to calculate j's annualized rate of return during y as:

$$1 + r_{j,y}^{Annualized} \coloneqq (1 + r_{j,y})^{D_y^{max}}$$

$$\implies r_{j,y}^{Annualized} = -1 + \left(\exp\left[\frac{\ln(1 + r_{j,y}^{Annual})}{D_{j,y}}\right]\right)^{D_y^{max}} \tag{B10}$$

B.2.3 Counterfactual Returns

If we replace $r_{i(j),t}$ in Equation (B8) with the rate of return $r_{k,t}$ yielded by some index k on date t, then we can calculate the rate of return that the counterfactual investment j in index k (rather than in company i) would have yielded during each calendar year y as:

$$r_{j,y}^{Annual,k} \coloneqq -1 + \prod_{t=1/1/y}^{12/31/y} 1\{Entry_j \leq t < Exit_j\} \times (1 + r_{k,t}) \tag{B11}$$

B.2.4 Aggregated Returns

As the last step, we aggregate the investment-level returns to characterize the overall performance of HNWIs' early-stage portfolios. Given the initial amount of U.S. dollars invested $Value_{j,0}$ as part of investment j, the year y_j in which the investment was entered, and its annual rate of return $r_{j,y}^{Annual}$ from Equation (B8), we can calculate the cumulative value of j by the end of year y as:

$$Value_{j,y} \coloneqq \begin{cases} Value_{j,0} & \text{if } y = y_j - 1 \\ Value_{j,0} \times \prod_{t=y_j}^{y}(1 + r_{j,y}^{Annual}) & \text{if } y \geq y_j \end{cases} \tag{B12}$$

The return (in U.S. dollars) that investment j yielded in year y is then:

$$Return_{j,y} \coloneqq Value_{j,y} - Value_{j,y-1} \tag{B13}$$

Given Equations (B12) and (B13), to aggregate returns across investments and calculate an average annual rate of return that assigns an appropriate weight to each investment, we can calculate the total value to paid-in capital ratio (TVPI) over a 1-year horizon as:

$$1 + r_y^{TVPI} \coloneqq \frac{\sum_j Value_{j,y}}{\sum_j Value_{j,y-1}} = \frac{\sum_j Value_{j,y-1} + Return_{j,y}}{\sum_j Value_{j,y-1}}$$

$$\implies r_y^{TVPI} = \frac{\sum_j Return_{j,y}}{\sum_j Value_{j,y-1}} \tag{B14}$$

r_y^{TVPI} in Equation (B14) is not annualized rate. To calculate an average annualized rate of return that assigns an appropriate weight to each investment based not only on its net asset value (NAV) at the start of the year but also on the number of days for which the investment was held during the year (Phalippou, 2025), we can calculate the 1-year NAV-to-NAV internal rate of return (IRR) as the solution to the following equation:

$$\sum_{t=0}^{D_y^{max}-1} \sum_{j:D_{j,y}=D_y^{max}-t} \frac{Value_{j,y-1}}{(1+r_y)^t} = \sum_{t=1}^{D_y^{max}} \sum_{j:D_{j,y}=D_y^{max}-t+1} \frac{Value_{j,y}}{(1+r_y)^t}$$

$$\implies 1 + r_y^{IRR} \coloneqq (1+r_y)^{D_y^{max}}$$

$$\implies r_y^{IRR} = -1 + (1+r_y)^{D_y^{max}} \tag{B15}$$

Finally, by replacing the actual annual rate of return $r_{j,y}^{Annual}$ in Equation (B12) with the counterfactual annual rate of return $r_{j,y}^{Annual,k}$ from Equation (B11) and then recalculating Equations (B13)-(B15), we can calculate the counterfactual average rates of return.

B.3 Risk-Adjusted Returns

While much of the literature has focused on risk-adjusted returns at the fund level such as the public market equivalent (Kaplan and Schoar (2005)), generalized public market equivalent (Korteweg and Nagel (2025)), we are unable to only rely on these measures as majority of the deals by HNWI to early-stage companies are direct investments. Hence, we follow Korteweg and Sorensen (2010) to estimate risk-adjusted returns at the deal level. We incorporate both, intermediated deals via funds and direct investments by HNWIs in companies. This allows us to incorporate differences in valuations of individual

companies over short times, which are is generally difficult to capture by cash flows distributed to investors investing in the fund. Further, valuations of portfolio companies which are required to calcuate returns at the deal level are most likely observed for the top performing companies, and likely when companies receive funding or exit via IPO, making the data subject to selection concerns, survivorship bias, and stale prices. Korteweg and Sorensen (2010)'s risk-adjustment at the deal level accounts for these selection concerns in their Dynamic Selection Model (DSM) and suggests to provide accurate finite-sample inference.

Specifically, we estimate a CAPM model (and Fama French three-factor model, incorporating the size factor–SMB, the value factor–HML along with the market factor–M) by adding a selection model which estimates the unobserved valuations and returns between observed valuations, and also specifies the probability of observing a valuation. In practice, the estimation procedure combines the Type-2 Tobit model with dynamic filtering and smoothing, and the Markov Chain Monte Carlo estimator uses Gibbs sampling. This approach is referred to as DSM. To show selection is present in the data, we compare the resulting DSM estimates which account for selection, to equivalent OLS and GLS estimates which do not account for selection. The OLS estimator regresses the log returns on the market factor, and the GLS estimator scales each observation with the inverse of the square root of the time since the last financing round.

The DSM consists of a valuation process and a selection process. To formalize ideas, we briefly describe the methodology from Korteweg and Sorensen (2010) below.

Valuation Process. Log-valuation $v(t)$ at time t follows,

$$v(t) = v(t-1) + X'(t)\theta + \varepsilon(t), \tag{B16}$$

where θ is the parameter of interest and $X(t)$ are the observed variables over time. The valuation process is eventually converted to a continous-time specification using geometric Brownian motion.

Selection Process. Valuation is observed when the latent selection variable, $w(t) \geq 0$, where $w(t)$ is given by,

$$w(t) = Z'(t)\gamma_0 + v(t)\gamma_v + \eta(t) \tag{B17}$$

$Z(t)$ consists of characteristics that affects the probability of the valuation being observed, for instance, when there is an exit or a refinancing event.

Assuming the errors in the valuation process and selection process are uncorrelated, i.e.,

$\varepsilon(t) \perp \eta(t)$, and $E[\varepsilon(t)] = 0$, the selection concern exists when the probability that we observe a valuation depends on the valuation itself, i.e., $\gamma_v \neq 0$. This is likely the case in our setting as companies with higher valuations are more likely to be occur in the data. The DSM accounts for this by allowing the conditional mean of the observed valuation, i.e., $E[v(t)|data]$, to be dependent on the last observed valuation $v(t-1)$, observed variables in between observed valuations $\left[\sum_{\tau=t-1}^{t} X'(\tau)\right]\theta$ and observables where valuations are unobserved $E\left[\sum_{\tau=t-1}^{t} \varepsilon(\tau)|data\right]$. The estimation procedure integrates over various possible paths of the unobserved outcome and the selection procedure.

Overview of the Estimation Procedure. We use the Bayesian Gibbs sampling procedure which has three separate steps. First, estimating unobserved valuations in the data, second is the selection of variables, and third is the parameters of interest. The second part, we sample from the truncated normal distribution defined in Eqn B17 such that $w(t) \geq 0$. The third part uses the selection equation in Eqn B17 and one-period transition of the valuation equation in Eqn B16 using the market-factor (or the Fama-French three-factors). The first part traces the path of unobserved latent valuations using parameters, selection variables, and returns using the Forward Filtering Backwards Sampling (FFBS). The Gibbs sampler simulates a posterior distribution by sample variables in each part taking the previous relations from other parts. Our DSM estimates use 300 iterations of the Gibbs sampling procedure, with the first 100 iterations discarded for burn-in. We verify that our estimates converged and attempted the procedure with more iterations as well. For more details on the DSM model and estimation procedure refer to Korteweg and Sorensen (2010).

Weighted Estimates. To calculate the weighted DSM estimates, we scale the early-stage returns and the independent variables in the valuation equation by $\frac{W}{T_{end}-T_{start}}$, where W is the amount of financing raised by each company from the relevant group of investors, T_{end} is December 2022 and T_{start} is the first month in which we observe a valuation for the company. Next, repeat the process for the selection equation. When estimating the parameters of the posterior distribution of σ, we replaced the residual sum of squares with the weighted residual sum of squares. This makes sure that the company which raises more capital gets more weight in the estimate, hence accounting for the skewness in the data.

Table B1: Replication of the Risk-Adjusted Returns of Korteweg and Sorensen (2010)

		KS (2010)			Our Replication		
Parameter	Period	OLS	GLS	DSM	OLS	GLS	DSM
β	1987-2005	2.077***	2.291***	2.751***	2.183***	2.379***	2.245***
		(0.100)	(0.117)	(0.113)	(0.080)	(0.110)	(0.112)
	1987-1993			0.381			4.620**
				(0.671)			(2.208)
	1994-2000			2.501***			2.751***
				(0.205)			(0.186)
	2001-2005			1.086***			1.540***
				(0.184)			(0.121)
σ	1987-2005	1.370	0.416	0.411***	1.136	0.369	0.418***
				(0.005)			(0.004)
	1987-1993			0.330***			0.342***
				(0.012)			(0.030)
	1994-2000			0.419***			0.451***
				(0.005)			(0.008)
	2001-2005			0.362***			0.369***
				(0.009)			(0.005)
α	1987-2005		0.068	0.033***		0.066	0.043***
				(0.002)			(0.002)
	1987-1993			0.016***			0.017
				(0.006)			(0.018)
	1994-2000			0.058***			0.088***
				(0.003)			(0.004)
	2001-2005			-0.027***			0.015***
				(0.003)			(0.002)

Notes: The table generates OLS, GLS, and dynamic selection model estimates following (Korteweg and Sorensen, 2010) for the one-factor "RMRF" model. β is the slope on the market log return in excess of the risk-free rate. σ is the estimated standard deviation of the error term. Korteweg and Sorensen (2010)'s sample consists of 5,501 deals from 1987-2005, and our sample consists of 6,708 deals with observed valuations over the same time period. ***, **, and * denotes whether zero is contained in the 1%, 5%, and 10% confidence intervals, respectively.

Table B2: Risk-Adjusted Returns using Dynamic Section Model (DSM) for 2009-2022

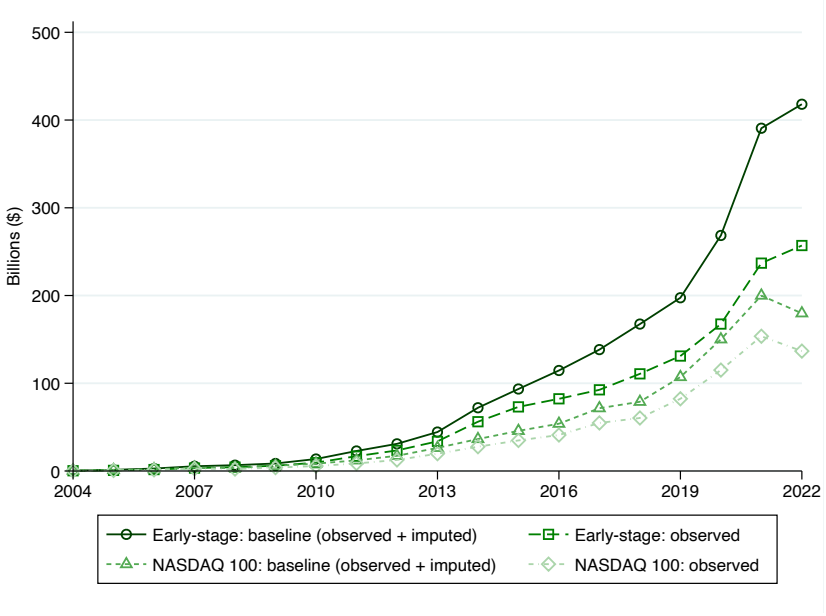
	All Deals		HNWIs Deals		All Minus HNWIs Deals	
	One-Factor (1)	Three-Factor (2)	One-Factor (3)	Three-Factor (4)	One-Factor (5)	Three-Factor (6)
β_M	1.781***	1.863***	1.878***	2.084***	1.817***	1.836***
	(0.069)	(0.110)	(0.103)	(0.156)	(0.126)	(0.097)
β_{SMB}		-0.241		-0.381*		-0.150
		(0.148)		(0.213)		(0.973)
β_{HML}		-0.099		0.128		-0.119
		(0.081)		(0.119)		(0.085)
α	0.070***	0.069***	0.080***	0.077***	0.067***	0.067***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
σ	0.340***	0.340***	0.384***	0.381***	0.337***	0.335***
	(0.001)	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)

Notes: The table generates DSM β and α estimates following (Korteweg and Sorensen, 2010) for the one-factor "RMRF" model and Fama-French three-factor model. σ is the estimated standard deviation of the error term. Columns (1) and (2) correspond to all early-stage U.S. company deals in our sample from 2004-2022. Columns (3) and (4) focus on the subset of deals with at least one HNWI investor. Columns (5) and (6) focus on the deals which are the difference between Columns (1)-(2) and Columns (3)-(4). We use valuations on the top 2,500 companies for each of the estimations for computational efficiency, which account for 65% of total financing raised by all investors (Column (1)-(2)), 77% of total financing raised by U.S. HNWIs (Column (3)-(4)), and 65% of total financing raised by other investors (Column (5)-(6)). ***, **, and * denotes whether zero is contained in the 1%, 5%, and 10% confidence intervals, respectively.

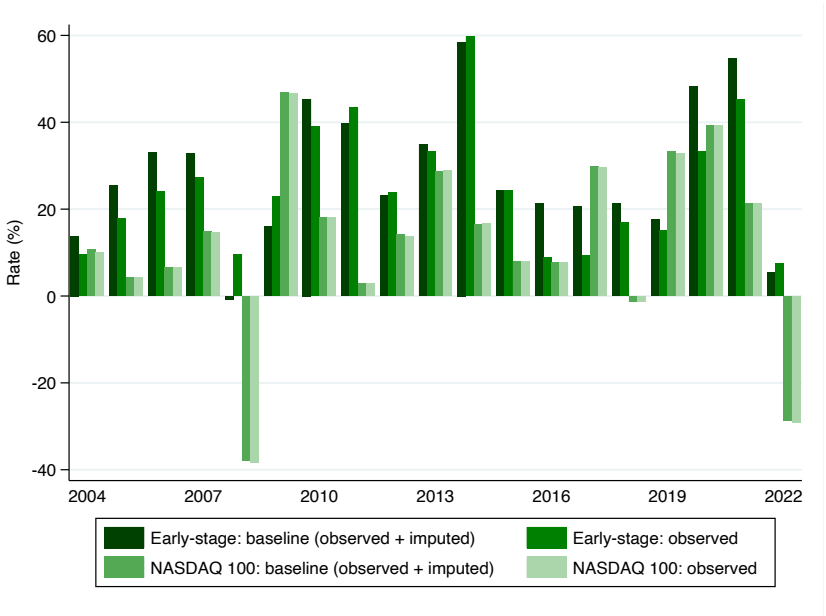
B.4 Additional Results

Figure B2: HNWIs' Excess Returns on Early-Stage Investments: Observed Valuations

(a) Accumulated value by end of year



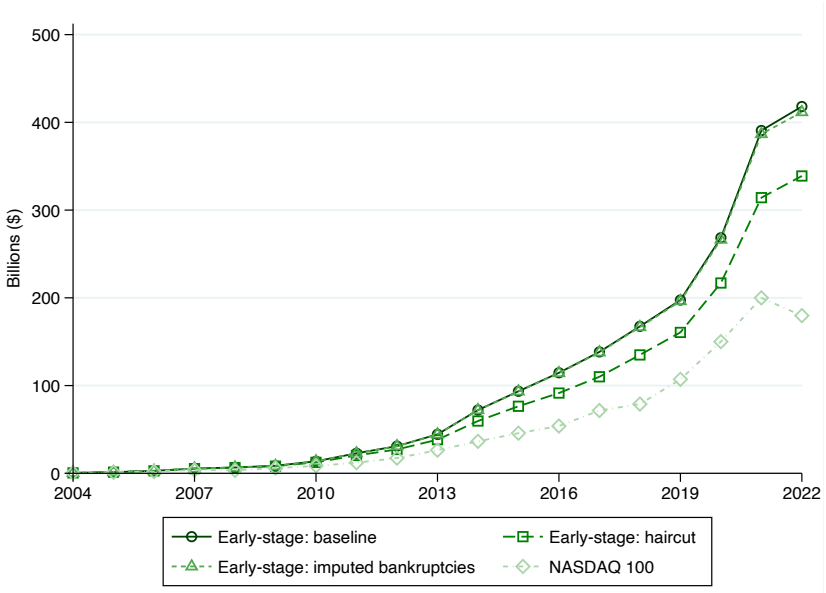
(b) Average annual rate of return



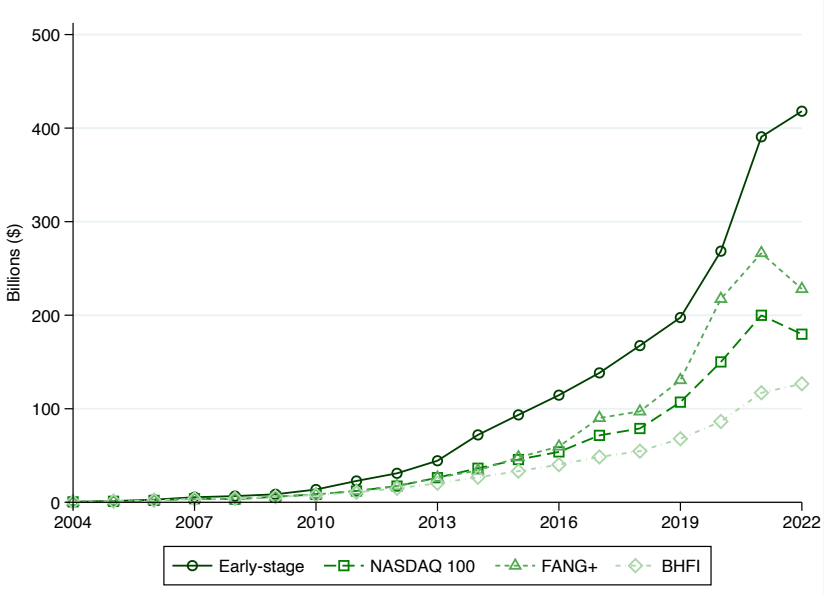
Source: Pitchbook, S&P Global.
Notes: This figure describes the excess returns that U.S. HNWIs earned on their early-stage investments in U.S. companies from 2004 to 2022 relative to the NASDAQ 100, comparing our baseline sample of observed and imputed valuations to the subsample of only observed valuations. Panel (a) plots the accumulated end-of-year value of HNWIs' investments, while Panel (b) plots the average annual rate of return that HNWIs earned across their investments, calculated as their total value to paid-in capital ratio (TVPI) minus one over a 1-year horizon. The values in Panel (a) are expressed in nominal terms.

Figure B3: HNWIs' Excess Returns on Early-Stage Investments: Robustness

(a) Alternative samples of valuations



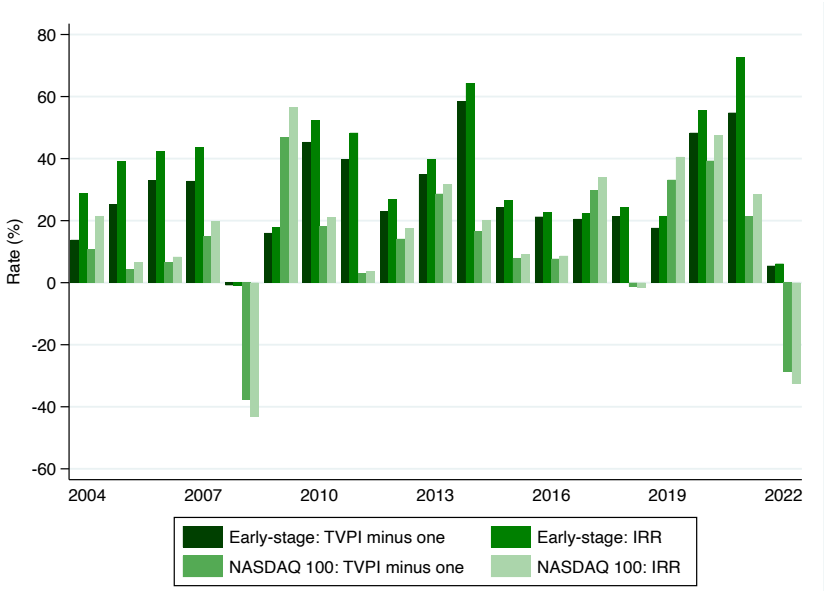
(b) Alternative counterfactual investments



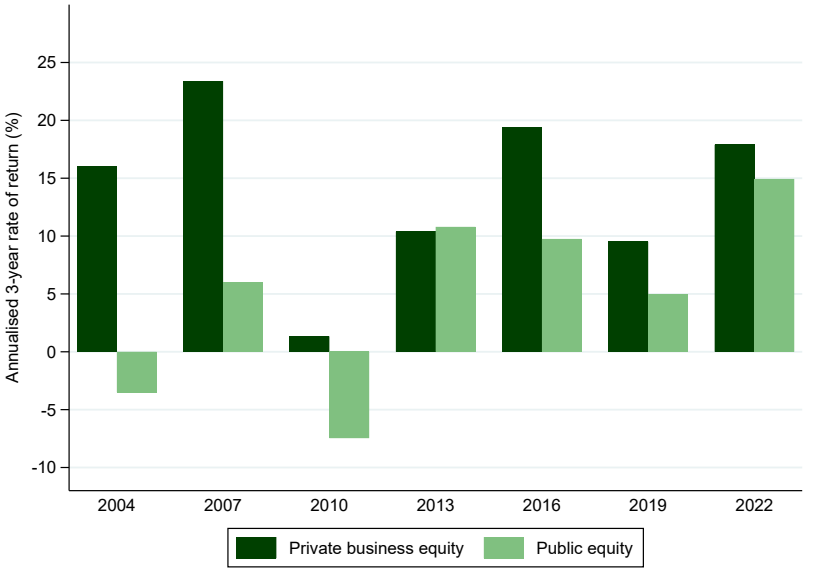
Source: Pitchbook, S&P Global, Bloomberg.
Notes: This figure evaluates the robustness of our finding that U.S. HNWIs earned excess returns on their early-stage investments in U.S. companies from 2004 to 2022. Panel (a) compares the accumulated end-of-year value of HNWIs' early-stage investments with counterfactual scenarios where they had instead invested in the NASDAQ 100, based on alternative samples of valuations: either applying haircuts to "unicorn" valuations that exceed $1 billion but have been shown to be overvalued (Gornall and Strebulaev, 2020; Gahng, 2023) or imputing bankruptcies for companies that appear to be inactive. Panel (b) reproduces this plot but for alternative counterfactual investments in the FANG+ public stock market index or the Barclay Hedge Fund Index (BHFI). Values are expressed in nominal terms.

Figure B4: HNWIs' Average Rate of Return on Early-Stage Investments: Robustness

(a) TVPI minus one vs. IRR



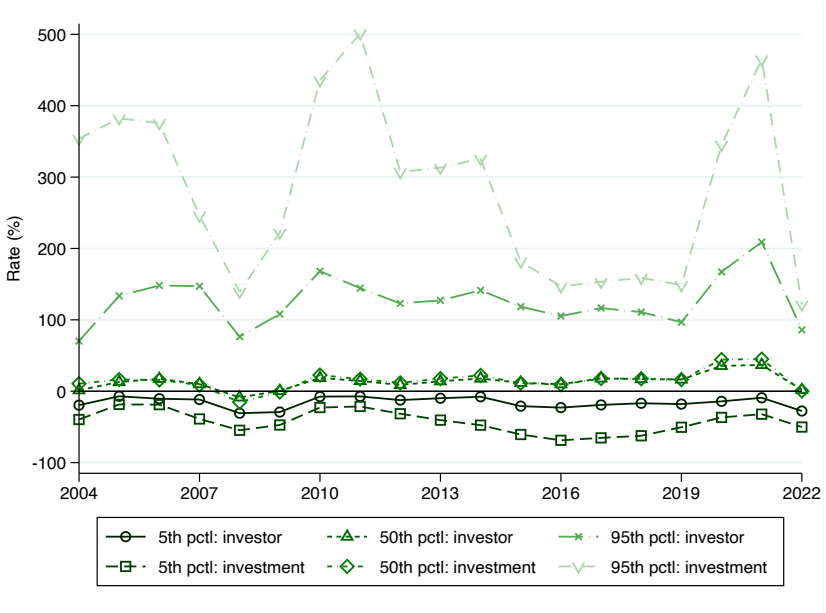
(b) Private business equity vs. public equity



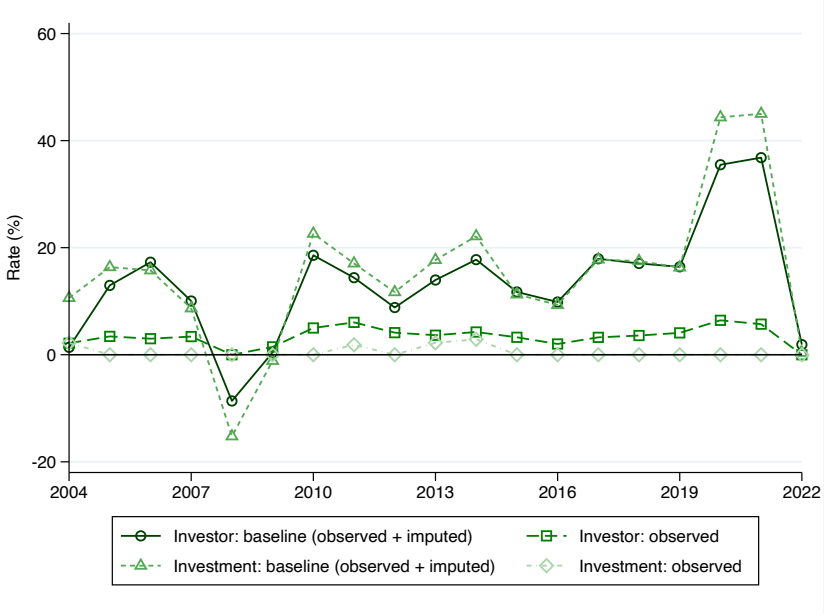
Source: Pitchbook, S&P Global, SCF.
Notes: This figure evaluates the robustness of our finding that the average annual rate of return on U.S. HNWIs' early-stage investments in U.S. companies from 2004 to 2022 generally exceeded that of the NASDAQ 100. Panel (a) compares different measures of the average annual rate of return: the total value to paid-in capital ratio (TVPI) minus one over a 1-year horizon, and the internal rate of return (IRR) over a 1-year horizon from Phalippou (2025). Panel (b) uses the SCF and the methodology developed by Moskowitz and Vissing-Jørgensen (2002) and Kartashova (2014) to compare the average annualized 3-year rate of return on private business equity to that on public equity.

Figure B5: Distribution of Returns on HNWIs' Early-Stage Investments: Robustness

(a) Investors vs. investments



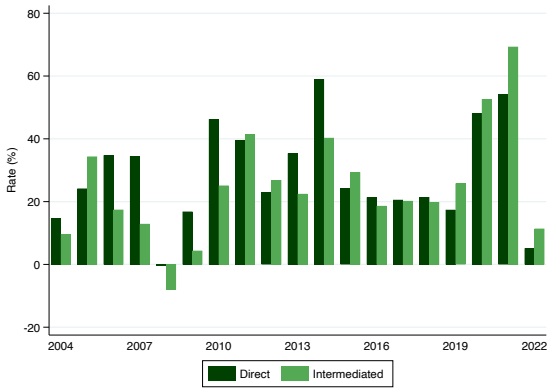
(b) Baseline sample vs. subsample of observed valuations



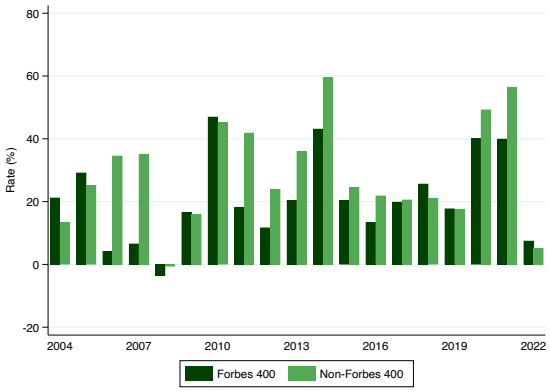
Source: Pitchbook.
Notes: This figure compares the distribution across investors to the distribution across investments of the annual rates of return of U.S. HNWIs' early-stage investment in U.S. companies from 2004 to 2022. Panel (a) plots the 5th, 50th, and 95th percentiles of these distributions, while Panel (b) compares the 50th percentiles of the two distributions based on our baseline sample (of both observed and imputed valuations) to the distributions based on the subsample of only observed valuations.

Figure B6: HNWIs' Average Rate of Return on Early-Stage Investments: Heterogeneity

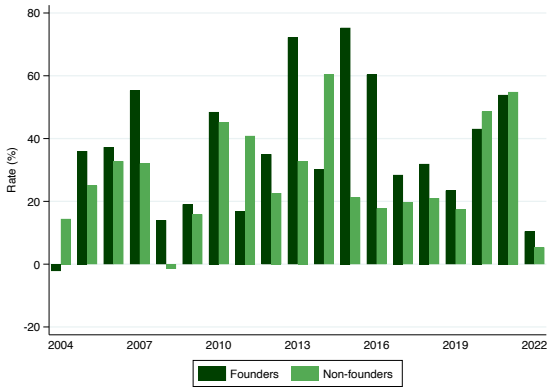
(a) Direct vs. intermediated



(b) Forbes 400 vs. non-Forbes 400



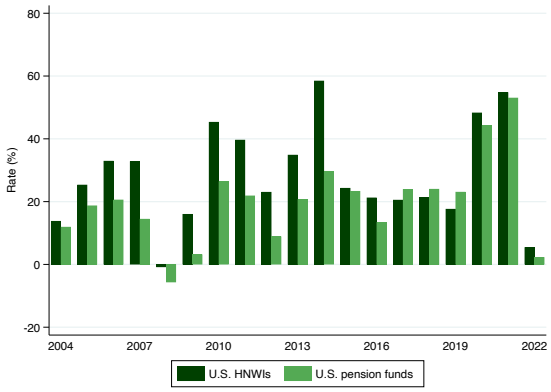
(c) Founders vs. non-founders



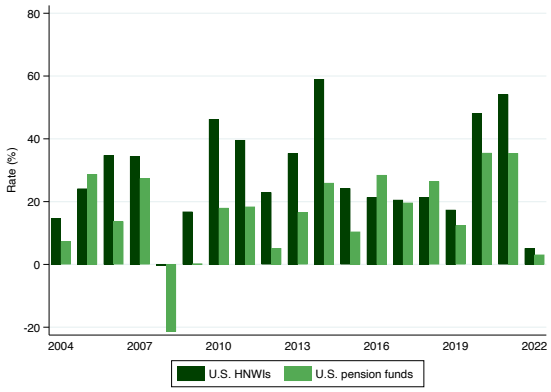
Source: Pitchbook.
Notes: This figure analyzes heterogenities in the average annual rate of return on U.S. HNWIs' early-stage investments in U.S. companies from 2004 to 2022. Panel (a) compares HNWIs' direct investments to their intermediated investments, Panel (b) compares investments by HNWIs ranked in the Forbes 400 to those by unranked ones, and Panel (C) compares investments by founders to those by non-founders.

Figure B7: HNWIs' vs. Pension Funds' Average Rate of Return on Early-Stage Investments

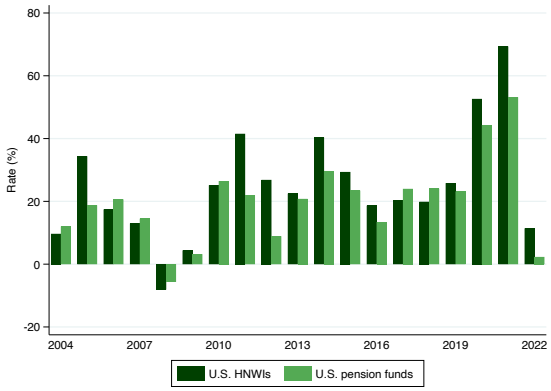
(a) All investments



(b) Direct investments



(c) Intermediated investments



Source: Pitchbook.
Notes: This figure compares the average annual rates of return on U.S. HNWIs' and U.S. pension funds' early-stage investments in U.S. companies from 2004 to 2022. Panel (a) plots the rates of return on all their investments, Panel (b) plots the rates of return on only their direct investments, and Panel (c) plots the rates of return on only their intermediated investments.

Appendix C: QSBS Capital Gains Tax Exclusion

In this appendix, we describe our procedure to identify eligible issuers of qualified small business stock (QSBS) based on the data from Pitchbook, as well as elaborate upon our company-level and state-level regression analyses. Section C.1 first describes how we identify the eligibility-relevant characteristics of each U.S. company: when it was active and private, whether it is classified as a C corporation, whether it is active primarily in a qualified trade or business, and when its gross assets first exceeded $50 million. Section C.2 then examines how a single regression pooling different founding-year cohorts of companies could generate spurious pre-trends due to the unbalancedness of the sample in the pre-period. Finally, Section C.3 reports additional results on the history of the QSBS tax exclusion and the effects of its expansion at both the company and state level.

C.1 QSBS-Eligible Companies

C.1.1 Active Private Companies

We first identify the years during which each company was active and private. To do so, we assume that each company was active and private from the year in which it was founded to the year in which it first went bankrupt, became publicly listed, or was acquired.

For 340,250 (84.5%) of the 402,645 U.S. companies in the data from Pitchbook, we observe the year in which they were founded. For an additional 58,777 (14.6%), we impute their missing year founded as the first year in which they raised financing.

We also identify the first bankruptcy date for 45,399 (11.3%) U.S. companies, the first public offering date for 16,554 (4.1%), and the first acquisition date for 200,277 (49.7%).

C.1.2 C Corporations

We next identify C corporations by parsing each company's legal name. We observe this for 316,779 (78.7%) U.S. companies, using their trade name if their legal name is missing.

We can identify limited partnerships ("LP"), limited liability partnerships ("LLP"), and limited liability limited partnerships ("LLLP"), none of which can be taxed as C corporations. Though we can also identify limited companies ("LC" or "Ltd"), limited liability companies ("LLC"), professional limited liability companies ("PLLC"), and professional corporations ("PC"), these could have been—but were not necessarily—taxed as C corporations.

We therefore classify only the remaining corporations ("Corp" or "Inc") as C corporations. The reason why we can classify all of them as C corporations is that companies seeking financing in private capital markets were unlikely to be taxed as S corporations, since these can have at most 100 shareholders (Polsky and Yale, 2023). By our classification, 198,984 (49.4%) of the U.S. companies that we observe were C corporations.

C.1.3 Qualified Trades and Businesses

We now identify U.S. companies that were active primarily in a qualified trade or business. We consider each company's primary industry code, which is missing for only 146 (<0.1%) of them.[46] We classify a company as active primarily in a disqualified trade or business if its primary industry code was either missing or related to healthcare services, legal services, engineering services, accounting services, consulting services, financial services, performing arts, athletics, hospitality, agriculture, or natural resources. By this classification, 278,168 (69.1%) U.S. companies were active primarily in qualified trades or businesses.

C.1.4 Gross Assets Exceeding $50 Million

We finally calculate a proxy for each company's gross assets: the total financing that it raised up to the completion of each of its deal. For this calculation, we consider only the deals in which we would expect the financing raised by the company to have increased the gross assets on its balance sheet. For example, this excludes buyout and refinancing deals.

We can calculate the total financing raised as of the end of 2008 for 28,782 U.S. companies, 23,125 (80.3%) of which had not raised financing exceeding $50 million. Similarly, of the 166,635 U.S. companies that at least once raised financing likely to have increased their gross assets, 143,585 (86.2%) never raised more than $50 million.

We validate this proxy using data on financial statements available for 86,054 (21.6%) U.S. companies. For 3,491 of these companies, in addition to their total financing raised as of the end of 2008, we can calculate their true gross assets as of the end of 2008 as the sum of their cash and cash equivalents and their net property, plants, and equipment.[47] The proxy and the true gross assets lie on the same side of the $50 million threshold for

[46] Section 1202 of the Internal Revenue Code requires that at least 80% of a QSBS-eligible company's assets be used in the active conduct of one or more qualified trades or businesses (Polsky and Yale, 2023). However, since we cannot observe how much of its assets each company actually uses in each trade or business in which it is active, we consider only its primary industry code.

[47] In addition to cash, gross assets include "the fair market value of property contributed to the corporation measured at the time of the contribution" and "the adjusted basis of property other than contributed property" (Polsky and Yale, 2023). Net property, plant, and equipment proxies for their sum.

2,311 (66.2%) of these companies. The proxy's accuracy is even greater (82.0%) for the 172 of these companies that were founded in or after 2004.

C.2 Issue with Pooled Regressions on Unbalanced Samples

We examine how a single regression (see Equation (1) in Section 4.2.1) pooling different founding-year cohorts of companies could generate spurious pre-trends due to the unbalancedness of the sample in the pre-period. Consider the following stylized example with three periods and four companies: a treated company 1 founded in the first period with the outcome history $Y_1 \coloneqq \{Y_{1,t}\}_{t=1}^{3} = \{0, 0, 1\}$; a treated company 2 founded in the second period with the history $Y_2 = \{\cdot, 0, 1\}$, where $\cdot$ indicates an observation missing because the company was not yet founded; a control company 3 founded in the first period with the history $Y_3 = \{0, 0, 0\}$; and a control company 4 founded in the second period with the history $Y_4 = \{\cdot, 0, 0\}$. After we partial out the company fixed effects $\{\bar{Y}_i\}_{i=1}^{4} = \{\frac{1}{3}\sum_t Y_{i,t}\}_{i=1}^{4} = \{\frac{1}{3}, \frac{1}{2}, 0, 0\}$, the histories become $Y_1 - \bar{Y}_1 = \{-\frac{1}{3}, -\frac{1}{3}, \frac{2}{3}\}$, $Y_2 - \bar{Y}_2 = \{\cdot, -\frac{1}{2}, \frac{1}{2}\}$, $Y_3 - \bar{Y}_3 = \{0, 0, 0\}$, and $Y_4 - \bar{Y}_4 = \{\cdot, 0, 0\}$.

First, consider the case of a pooled regression. After we further partial out the year fixed effects $\{\bar{Y}_t\}_{t=1}^{3} = \{\frac{1}{2}\sum_i (Y_{i,1} - \bar{Y}_i), \frac{1}{4}\sum_i (Y_{i,2} - \bar{Y}_i), \frac{1}{4}\sum_i (Y_{i,3} - \bar{Y}_i)\} = \{-\frac{4}{24}, -\frac{5}{24}, \frac{7}{24}\}$, the histories become $Y_1 - \bar{Y}_1 - \bar{Y}_t = \{-\frac{4}{24}, -\frac{3}{24}, \frac{9}{24}\}$, $Y_2 - \bar{Y}_2 - \bar{Y}_t = \{\cdot, -\frac{7}{24}, \frac{5}{24}\}$, $Y_3 - \bar{Y}_3 - \bar{Y}_t = \{\frac{4}{24}, \frac{5}{24}, -\frac{7}{24}\}$, and $Y_4 - \bar{Y}_4 - \bar{Y}_t = \{\cdot, \frac{5}{24}, -\frac{7}{24}\}$. The differences between the period-specific means of the treated companies 1 and 2 and the untreated companies 3 and 4 are then $\{-\frac{8}{24}, -\frac{10}{24}, \frac{14}{24}\}$, or $\{\frac{1}{12}, 0, 1\}$ if we set the second period as the base period. Thus, even though $Y_{i,t} = 0$ for all companies in the first two periods, the unbalancedness of the company-level panel generates a spurious pre-trend in the first period.

Now, consider the case of a separate regression for each founding-year cohort. Since we have already partialed out company fixed effects, the difference between the partialed outcomes for companies 1 and 3 (which were founded in the first period) is $(Y_1 - \bar{Y}_1) - (Y_3 - \bar{Y}_3) = \{-\frac{1}{3}, -\frac{1}{3}, \frac{2}{3}\}$, or $\{0, 0, 1\}$ if we set the second period as the base period. Similarly, the difference between the partialed outcomes for companies 2 and 4 (which were founded in the second period) is $(Y_2 - \bar{Y}_2) - (Y_4 - \bar{Y}_4) = \{\cdot, -\frac{1}{2}, \frac{1}{2}\}$, or $\{\cdot, 0, 1\}$ if we set the second period as the base period. Thus, the weighted average of these cohort-specific differences—with the weight applied to each cohort's difference equal to its share of the companies appearing in each year's sample—is $\{0, 0, 1\}$. In other words, this weighted average does not suffer from the same spurious pre-trend as the pooled regression.

C.3 Additional Results

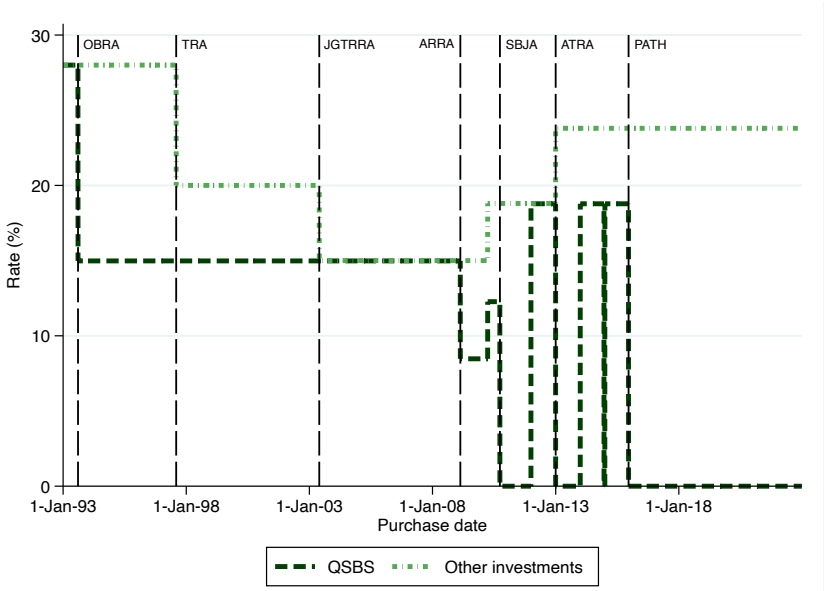
Table C1: Summary Statistics for Company-Level Analysis

	QSBS-eligible	QSBS-ineligible
	Share	Share
Total	1.000	1.000
incl. founded between 2004 and 2008	0.444	0.386
incl. founded before 2004	0.556	0.614
incl. C corporations	1.000	0.337
incl. other companies	0.000	0.663
incl. technology	0.785	0.406
incl. professional services	0.000	0.200
incl. financial services	0.000	0.073
incl. extraction/farming	0.000	0.078
incl. hospitality	0.000	0.045
incl. other non-technology	0.215	0.594
incl. financing raised $\in [0, 0.5)$ million \$	0.212	0.229
incl. financing raised $\in [0.5, 1)$ million \$	0.076	0.050
incl. financing raised $\in [1, 5)$ million \$	0.266	0.170
incl. financing raised $\in [5, 10)$ million \$	0.139	0.094
incl. financing raised $\in [10, 25)$ million \$	0.187	0.120
incl. financing raised $\in [25, 50)$ million \$	0.120	0.084
incl. financing raised $\in [50, 100)$ million \$	0.000	0.127
incl. financing raised $\in [100, 500)$ million \$	0.000	0.102
incl. financing raised $\in [500, 1000)$ million \$	0.000	0.014
incl. financing raised $>$ 1,000 million \$	0.000	0.010
incl. early-stage financing from U.S. HNWI	0.060	0.048
Number of companies	7,971	6,523

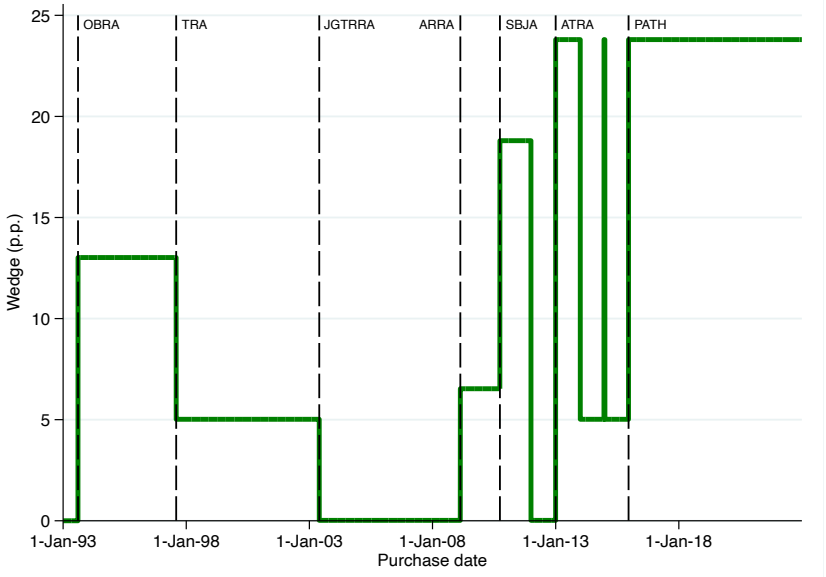
Sources: Pitchbook.
Notes:

Figure C1: History of the Federal Tax Exclusion on QSBS Capital Gains

(a) Expected tax rates on QSBS vs. other investments

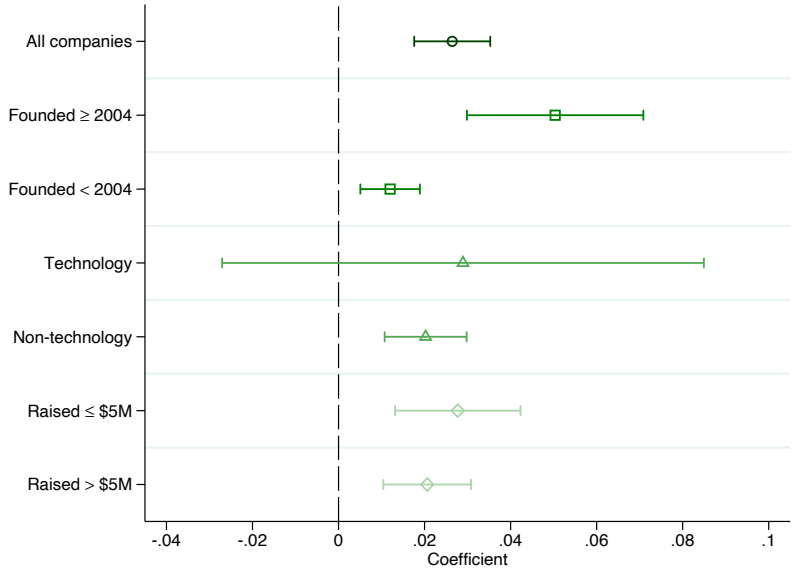


(b) Expected tax wedge on QSBS



Source: Polsky and Yale (2023).
Notes: This figure compares the evolution of the expected federal tax rate on QSBS capital gains between 1993 and 2022 with that of the expected federal long-term capital gains tax rate on other investments. Panel (a) depicts the evolution of the two expected rates separately, while panel (b) plots the difference between the two rates. The dashed vertical black bars indicate the different Acts that changed QSBS legislation: the Omnibus Budget Reconciliation Act (OBRA), the Taxpayer Relief Act (TRA), the Jobs and Growth Tax Relief Reconciliation Act (JBGTRRA), the American Recovery and Reinvestment Act (ARRA), the Small Business Jobs Act (SBJA), the American Tax Payer Relief Act (ATRA), and the Protecting Americans from Tax Hikes Act (PATH).

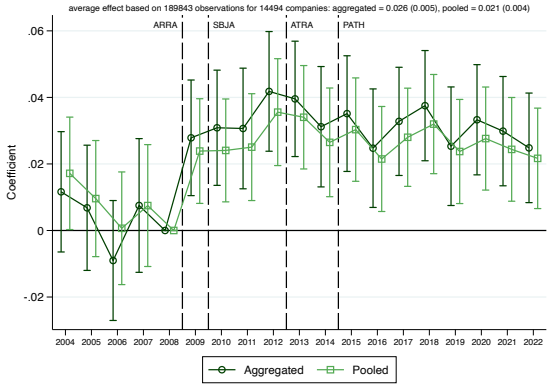
Figure C2: Effects of the QSBS Reforms on Companies: Heterogeneity



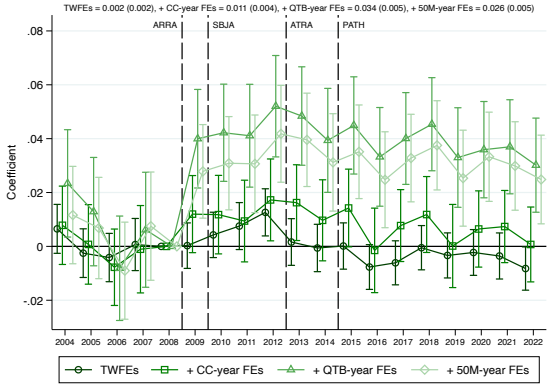
Source: Pitchbook.
Notes: This figure analyzes the heterogeneous effects of the QSBS reforms on QSBS-eligible companies' probability of raising early-stage financing from at least one U.S. HNWI. Specifically, it compares their effects on companies founded from 2004 to 2008 to their effects on companies founded before 2004.

Figure C3: Effects of the QSBS Reforms on Companies: Robustness

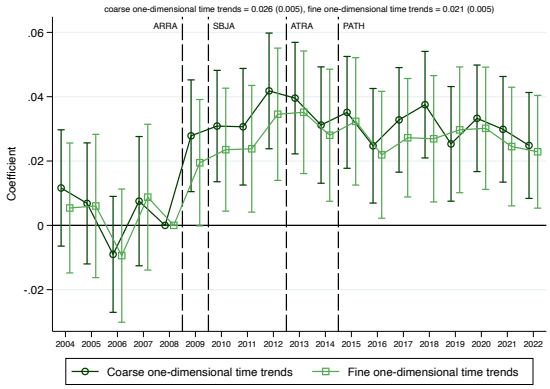
(a) Aggregated vs. pooled estimator



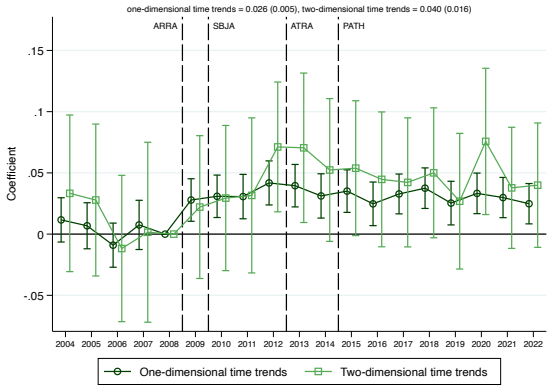
(b) Iteratively adding time trends



(c) Coarse vs. fine time trends



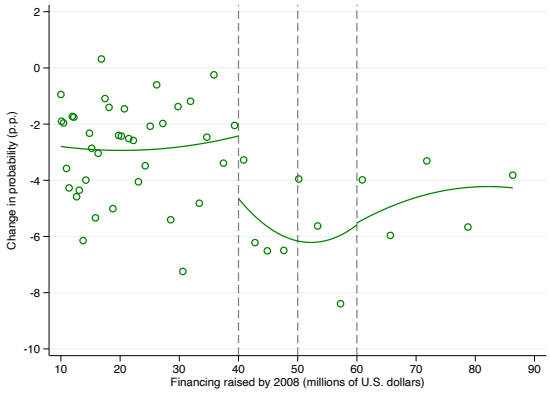
(d) One- vs. two-dimensional time trends



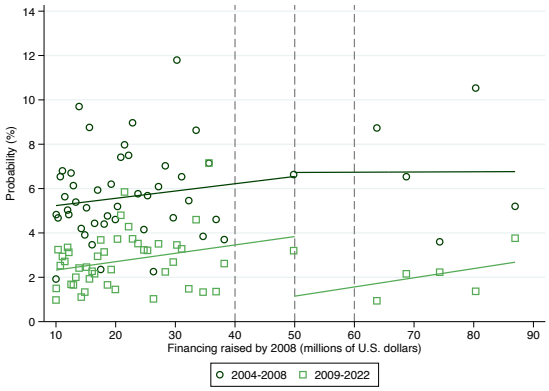
Source: Pitchbook.
Notes: This figure evaluates the robustness of our baseline estimates of the effects of the QSBS reforms on QSBS-eligible companies' probability of raising early-stage financing from at least one U.S. HNWI. Panel (a) compares our aggregated estimates, which are weighted averages of the estimates from separate regressions for each founding-year cohort, with pooled estimates from a single regression. Panel (b) documents how, starting from a simple regression with only two-way fixed effects, we can build up to our baseline estimates by iteratively adding separate time trends for each of the three dimensions of QSBS eligibility. Panel (c) shows that our baseline estimates are robust to switching to to finer time trends for different subsets of QSBS-eligible and QSBS-ineligible companies (e.g., rather than just for C corporations and other companies, add separate time trends for C corporations, limited liability companies, partnerships, and professional companies). Finally, Panel (d) replaces the separate time trends for each of the three dimensions of QSBS eligibility with ones for each two-dimensional combination of them.

Figure C4: Effects of the QSBS Reforms on Companies: RDD

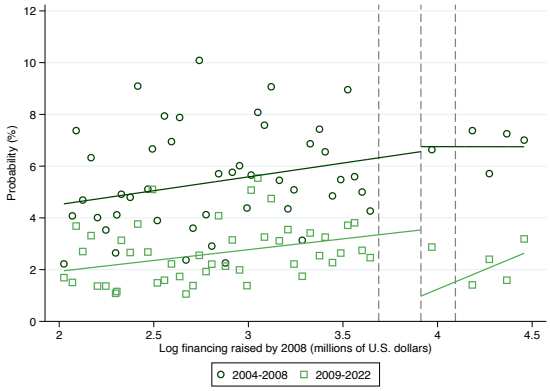
(a) Difference-in-discontinuities



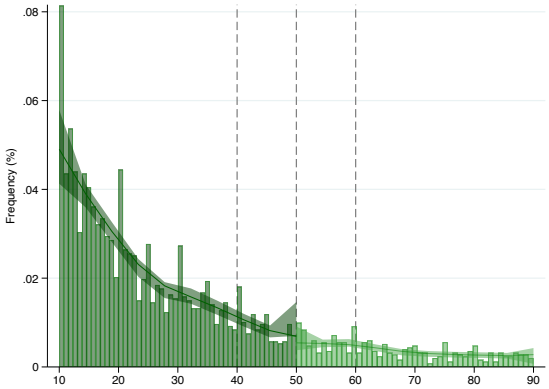
(b) Discontinuity in financing raised



(c) Discontinuity in log financing raised



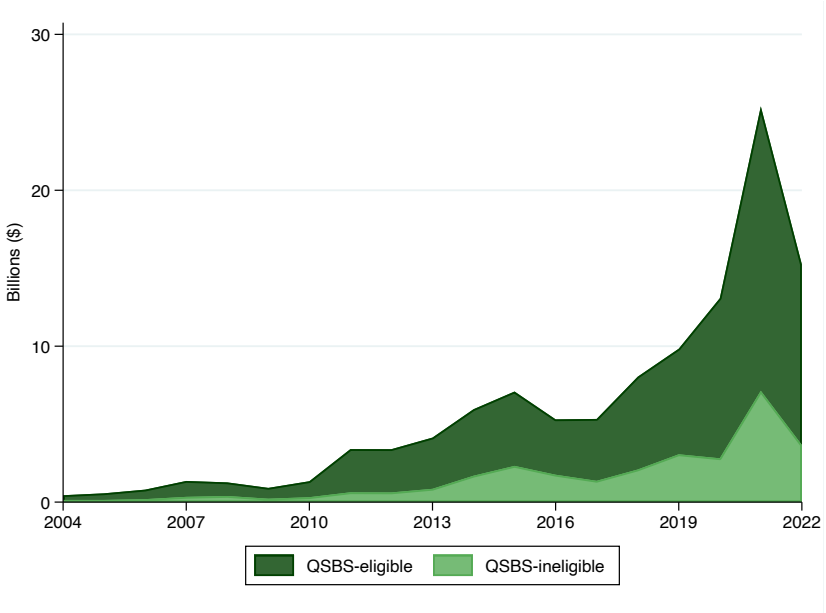
(d) Test for manipulation in running variable



Source: Pitchbook.

Notes: This figure explores an auxiliary regression discontinuity design (RDD) to evaluate the effects of the QSBS reforms on QSBS-eligible companies' probability of raising early-stage financing from at least one U.S. HNWI. We consider only C corporations that are active primarily in a qualified trade or business, investigating discontinuities around the $50 million gross assets threshold that corresponds to the third dimension of QSBS eligibility. We use total financing raised as a proxy for gross assets—which is the actual measure used to determine QSBS eligibility—as Pitchbook has information on gross assets only for a limited sample of companies. Panel (a) plots the change between the pre-reform and post-reform periods in each company's probability of raising financing from HNWIs (on the vertical axis) against the total financing that it had raised as of the end of 2008 (on the horizontal axis), fitting separate quadratic functions for the ranges $10-40 million, $40-60 million, and $60-90 million. Panel (b) plots both the pre-reform and post-reform probabilities, fitting separate linear functions for the ranges $10-40 million and $40-60 million. Panel (c) simply replaces the horizontal axis in Panel (b) with its logged equivalent. Panel (d) reports the test for manipulation in the running variable.

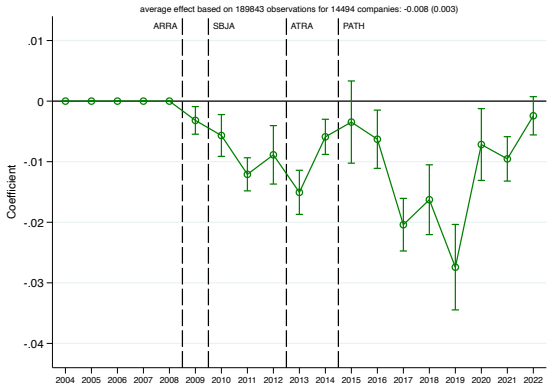
Figure C5: U.S. HNWIs' Early-Stage Investments in U.S. Companies: QSBS Eligibility



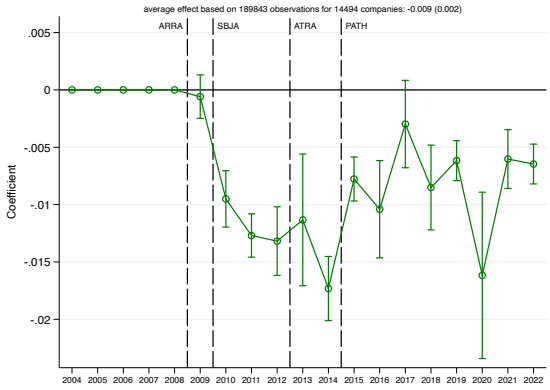
Source: Pitchbook.
Notes:

Figure C6: Effects of the QSBS Reforms on Companies: Staying Private

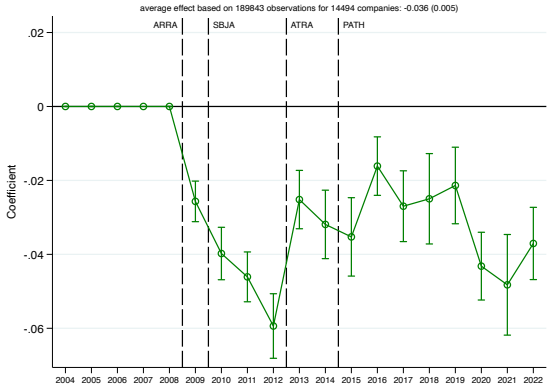
(a) Probability of going bankrupt



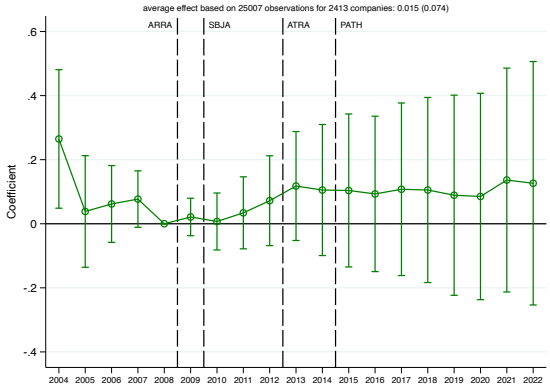
(b) Probability of publicly listing



(c) Probability of being acquired

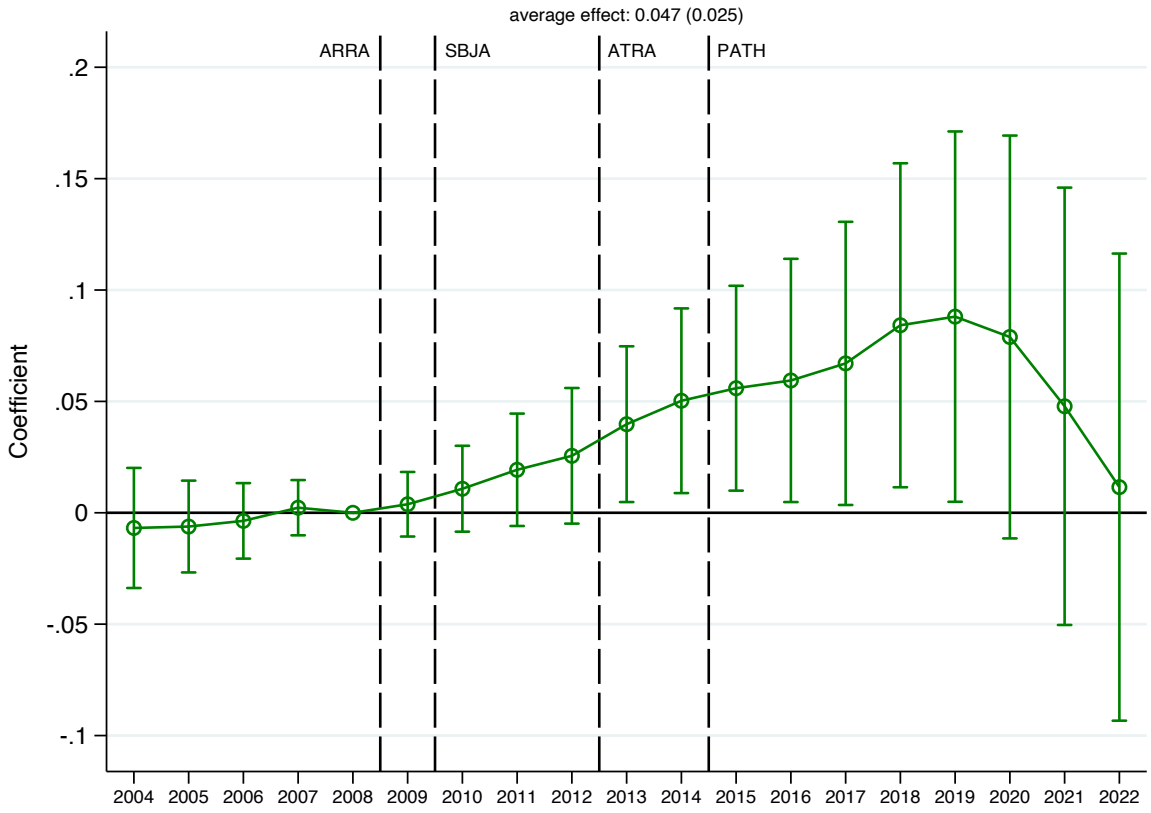


(d) Log number of employees



Source: Pitchbook.
Notes: This figure evaluates the effects of the QSBS reforms on QSBS-eligible companies' other outcomes, namely, their probability to go bankrupt, their probabilty to become publicly listed, their probability to be acquired, and their log number of employees. Panel (a) plots the QSBS reforms' effects on QSBS-eligible companies' probability to go bankrupt, Panel (b) plots their effects on the companies' probability to become publicly listed, and Panel (c) plots their effects on the companies' probability to be acquired. Panel (d) instead plots the effects on the log number of employees in each company.

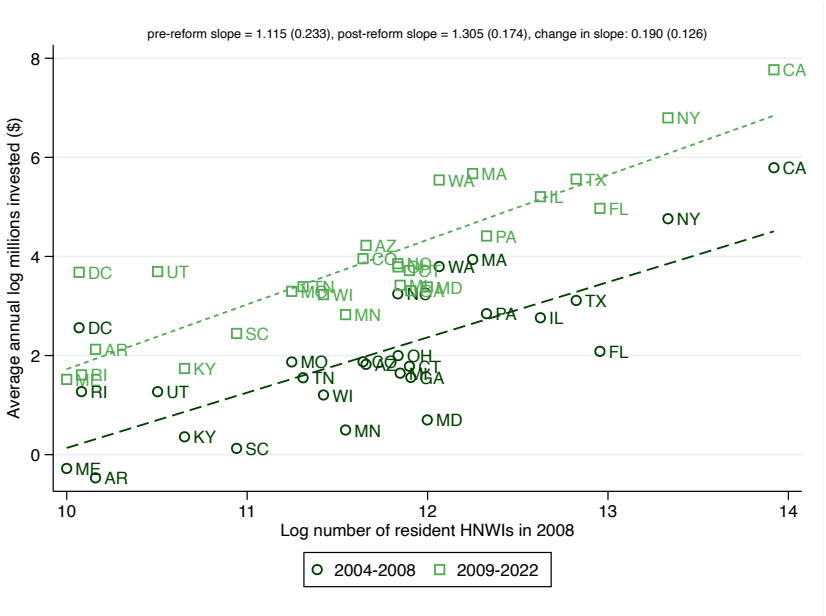
Figure C7: Effects of the QSBS Reforms on the Number of Active Private Companies



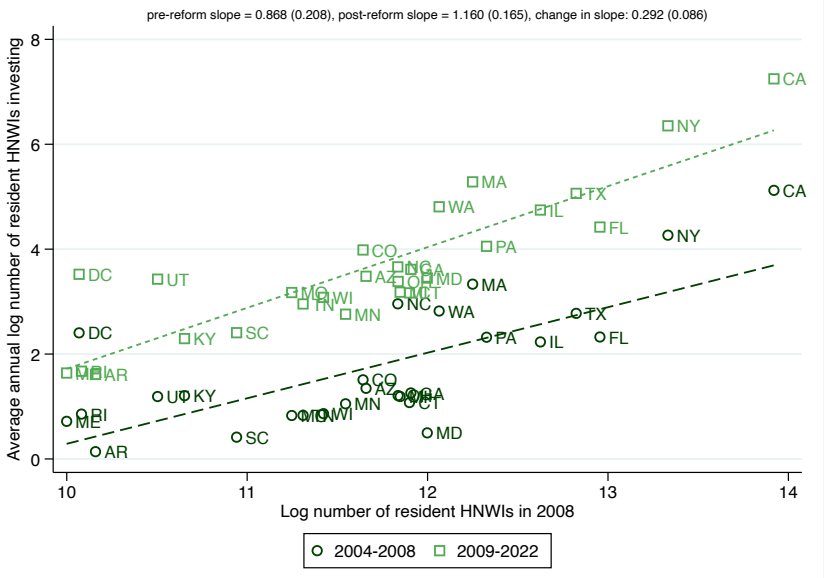
Source: Pitchbook.
Notes:

Figure C8: Number of Resident HNWIs in 2008 as State-Level Exposure to QSBS Reforms

(a) Resident HNWIs' early-stage investments

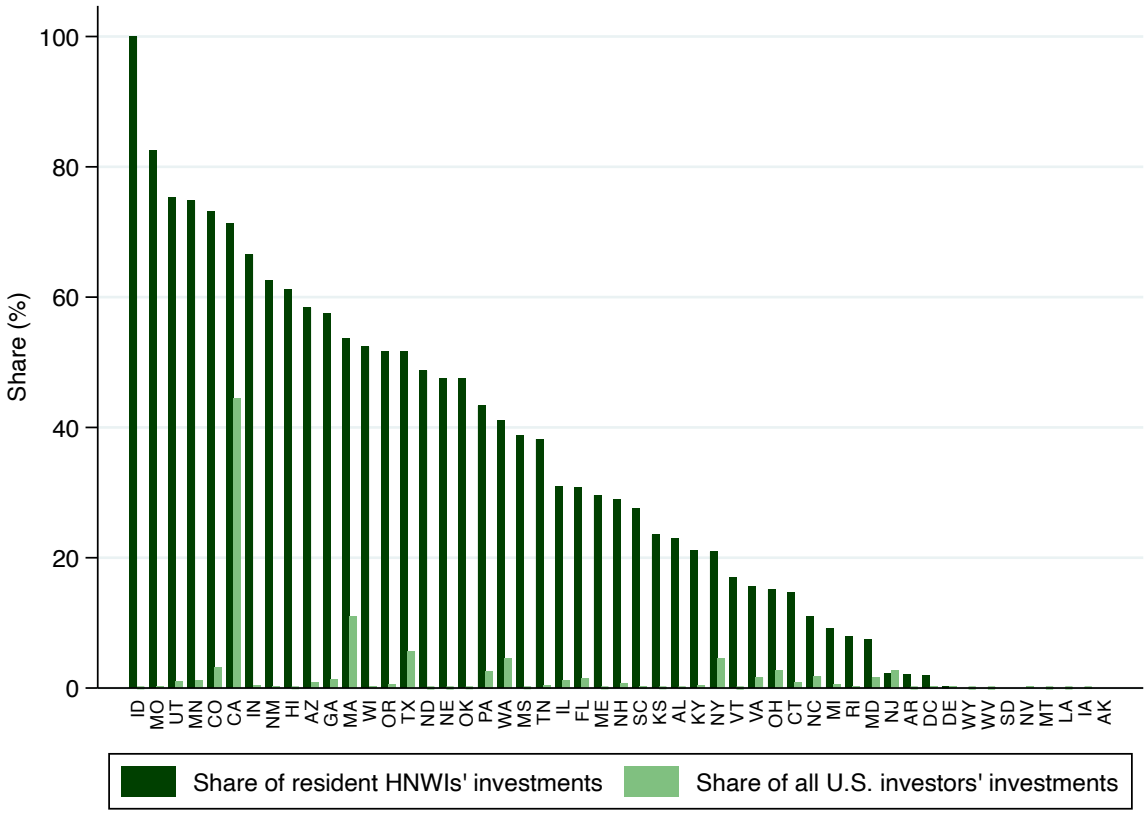


(b) Number of resident HNWIs with early-stage investments



Source: Pitchbook, GEOWEALTH-US.
Notes: This figure plots the relationships across states between various measures of resident HNWIs' early-stage investment activity and the number of resident HNWIs in 2008, comparing how the relationship changes from the 2004-2008 period before the QSBS reforms to the 2009-2022 period after the reforms. On the vertical axis, Panel (a) plots the average across years of the log millions of dollars invested by resident HNWIs as part of their early-stage investments in U.S. companies, while Panel (b) plots the average of the log number of distinct resident HNWIs making such investments. On the horizontal axis, both panels plot the log number of resident accredited investors in 2008, as measured in the GEOWEALTH-US dataset. To ensure a balanced panel, we consider only the 27 states with at least one early-stage investment by a resident HNWI in every pre-reform and post-reform year.

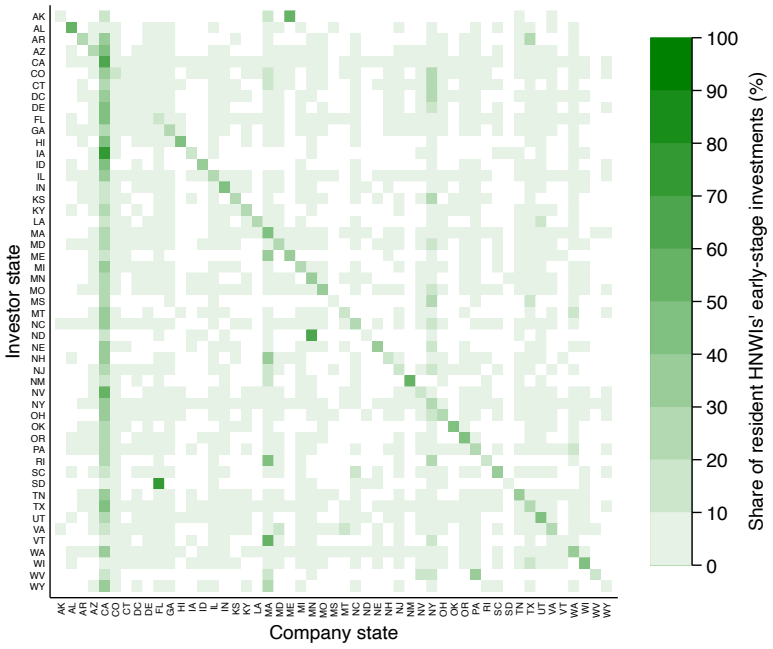
Figure C9: In-State Bias of Resident HNWIs' Early-Stage Investments, 2004-2008



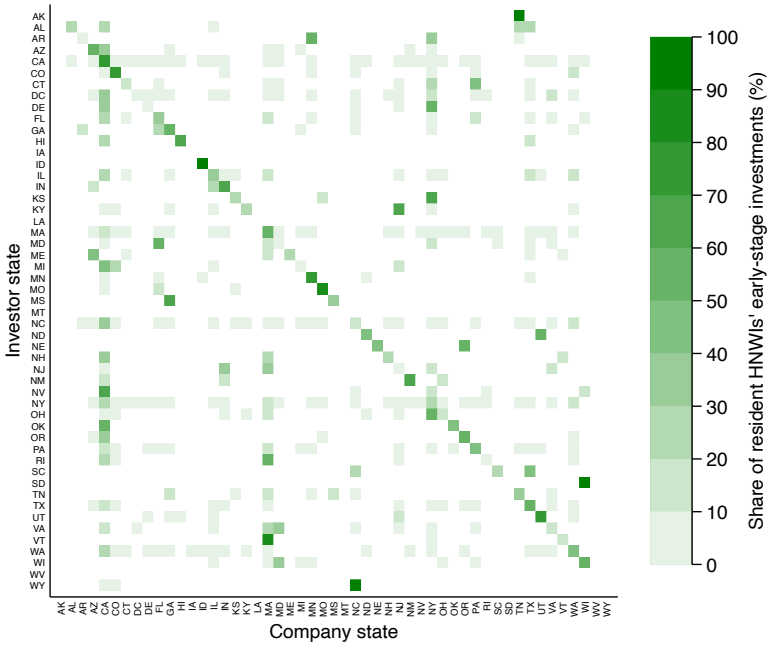
Source: Pitchbook.
Notes: This figure compares the share of each state's resident HNWIs' early-stage investments allocated to companies headquartered in it to the share allocated by all U.S. investors (i.e., both HNWIs and institutions) during the 2004-2008 period.

Figure C10: Distribution across States of Resident HNWIs' Early-Stage Investments

(a) 2004-2022

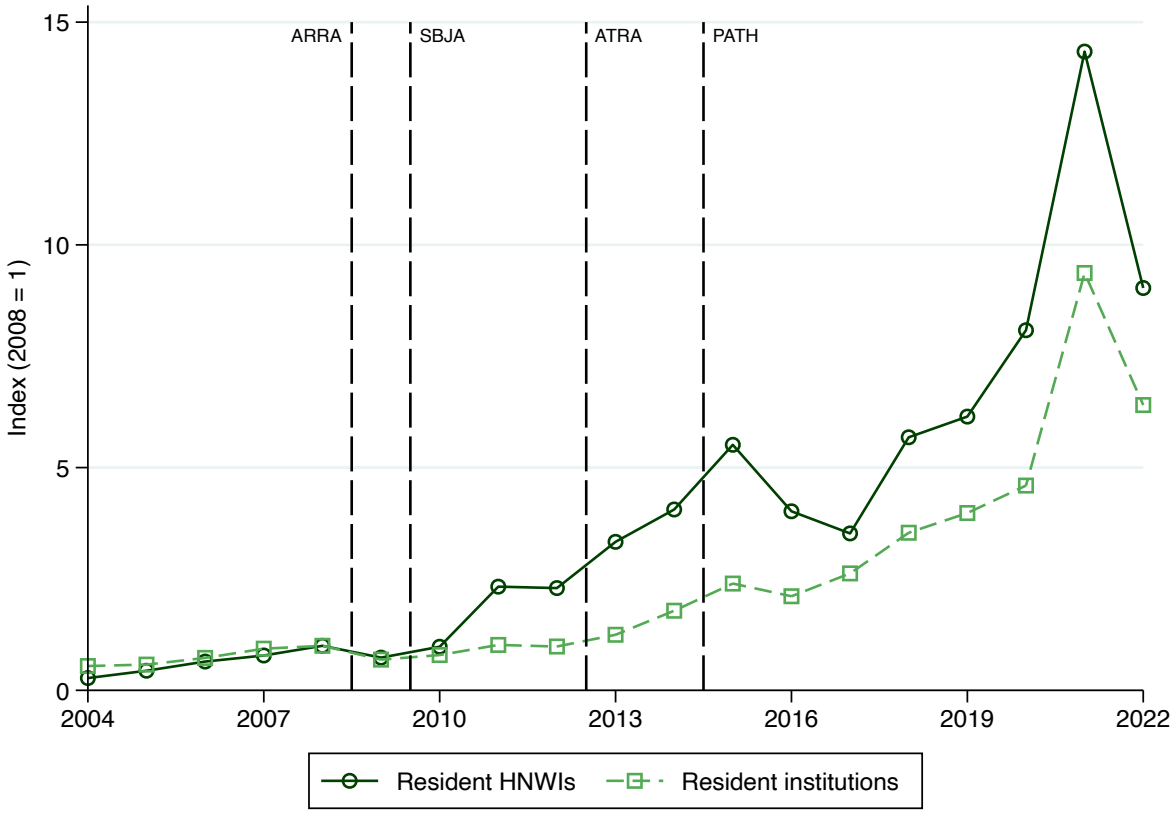


(b) 2004-2008



Source: Pitchbook.
Notes: This figure plots the share of each state's resident HNWIs' early-stage investments allocated to companies headquartered in each state. Panel (a) plots this distribution across states for the 2004-2022 period, while Panel (b) plots it for the 2004-2008 period.

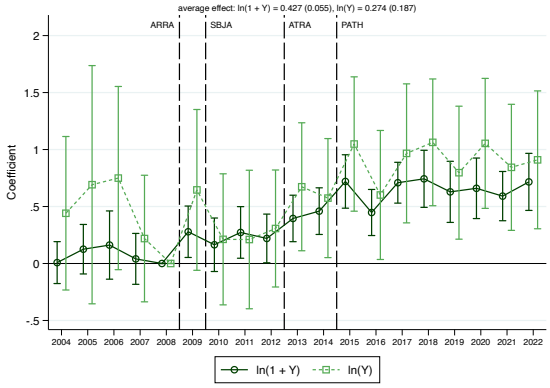
Figure C11: In-State Early-Stage Investments by Resident HNWIs vs. Resident Institutions



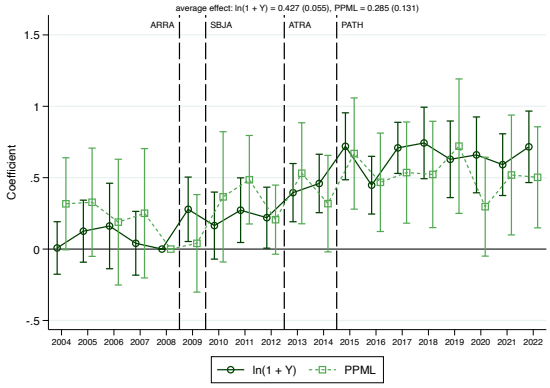
Source: Pitchbook.
Notes: This figure compares the evolution since 2008 of resident HNWIs' and resident institutions' early-stage investments in companies headquartered in their own state.

Figure C12: Effects of the QSBS Reforms on Early-Stage Investments: Estimators

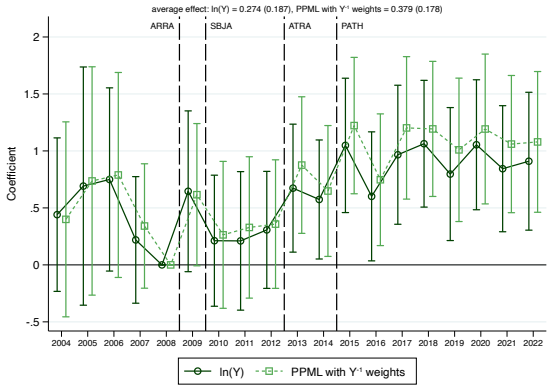
(a) $\ln(1 + Y_{s,g,t})$ vs. $\ln Y_{s,g,t}$



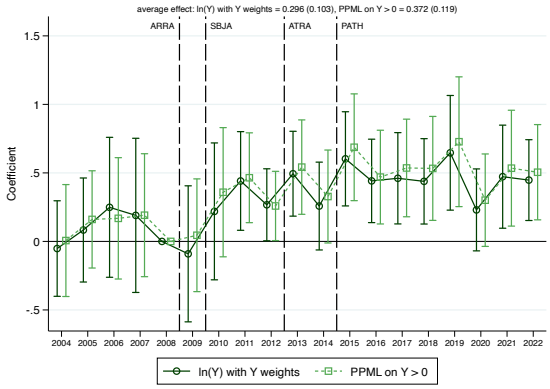
(b) $\ln(1 + Y_{s,g,t})$ vs. PPML



(c) $\ln Y_{s,g,t}$ vs. PPML with $Y_{s,g,t}^{-1}$ weights



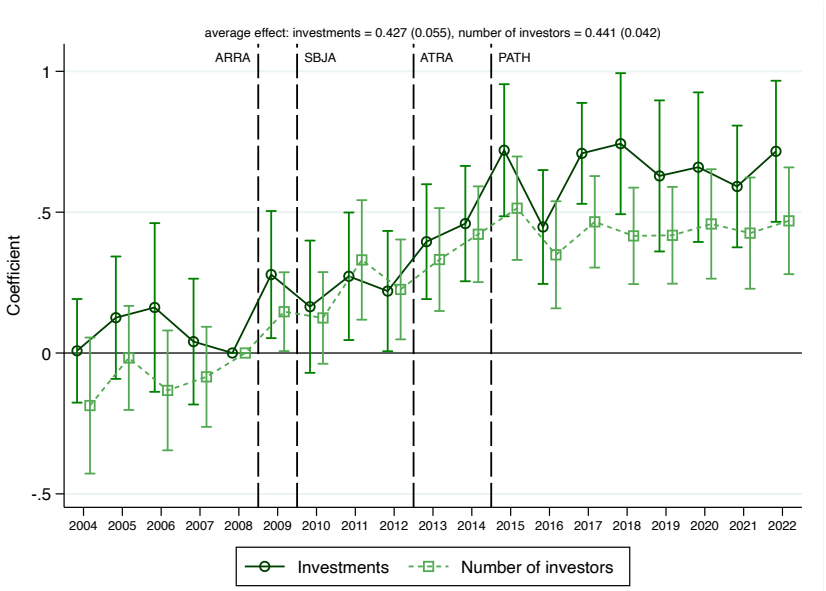
(d) $\ln Y_{s,g,t}$ with $Y_{s,g,t}$ weights v.s PPML



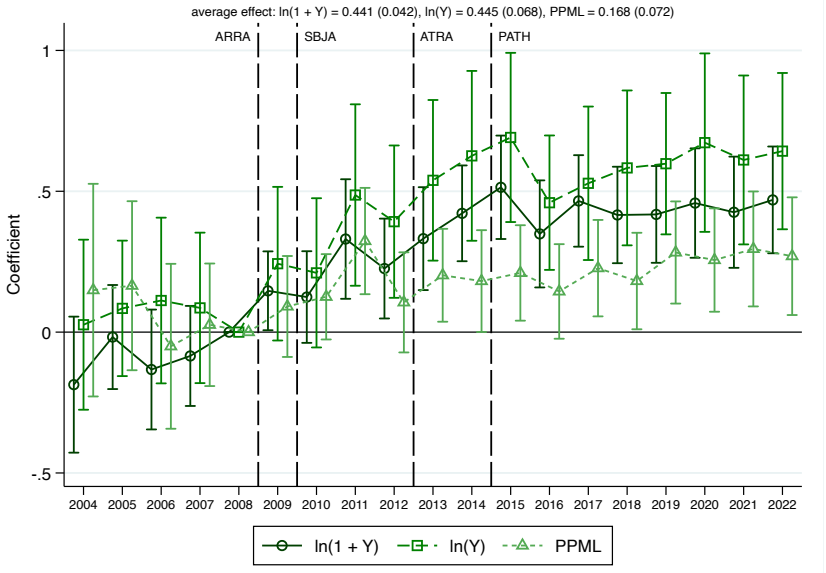
Source: Pitchbook, GEOWEALTH-US.
Notes: This figure plots the triple-difference estimates (with 95% confidence intervals) of Equation (2) based on alternative estimators. Panel (a) compares OLS estimates with $\ln(1 + Y_{s,g,t})$ as the outcome to OLS estimates with $\ln Y_{s,g,t}$ as the outcome. Panel (b) compares OLS estimates with $\ln(1 + Y_{s,g,t})$ as the outcome to PPML estimates with $Y_{s,g,t}$ as the outcome. Panel (c) compares unweighted OLS estimates with $\ln Y_{s,g,t}$ as the outcome to weighted PPML estimates with $Y_{s,g,t}$ as the outcome and $Y_{s,g,t}^{-1}$ as the weights, based on the subsample of observations for which $Y_{s,g,t} > 0$. Finally, Panel (d) considers the same subsample, comparing weighted OLS estimates with $\ln Y_{s,g,t}$ as the outcome and $Y_{s,g,t}$ as the weights to unweighted PPML estimates with $Y_{s,g,t}$ as the outcome.

Figure C13: Effects of the QSBS Reforms on the Number of Resident HNWIs Investing

(a) Investments vs. number of investors



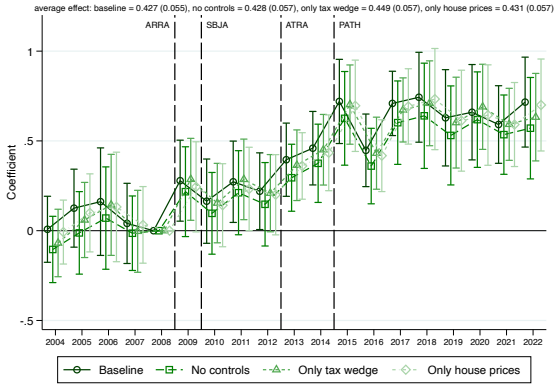
(b) Alternative estimators



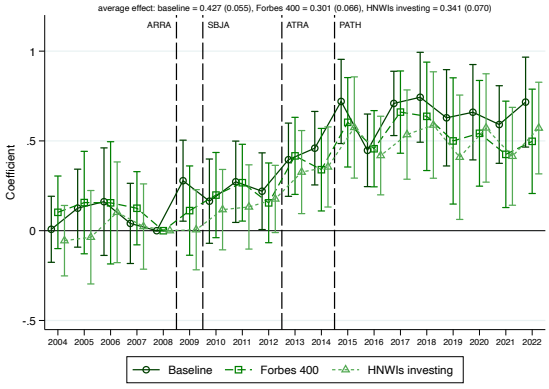
Source: Pitchbook, GEOWEALTH-US.
Notes: This figure plots the triple-difference estimates (with 95% confidence intervals) of Equation (2), replacing the early-stage investments by each investor group as the outcome with the number of distinct investors in each investor group with early-stage investments. Panel (a) compares the effects on investments to the effects on the number of investors, while Panel (b) evaluates the robustness of the effects on the number of investors to the use of alternative estimators.

Figure C14: Effects of the QSBS Reforms on Early-Stage Investments: Robustness

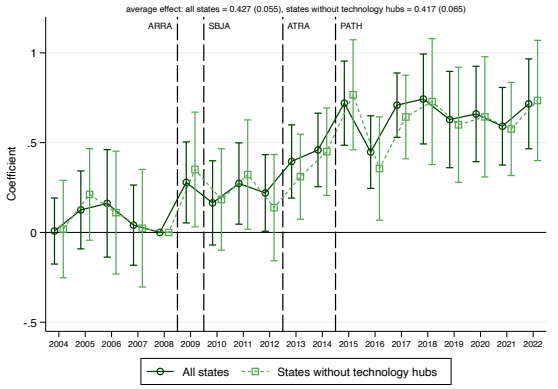
(a) Vector of control variables



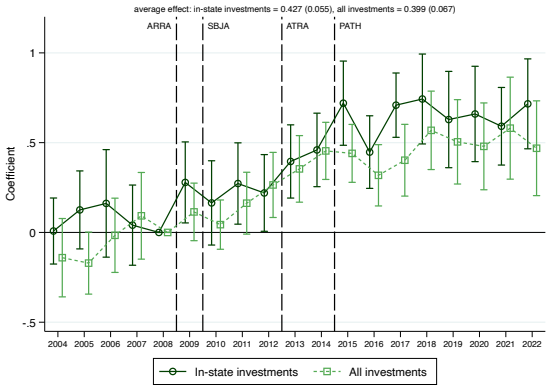
(b) Number of resident HNWIs in 2008



(c) States without technology hubs



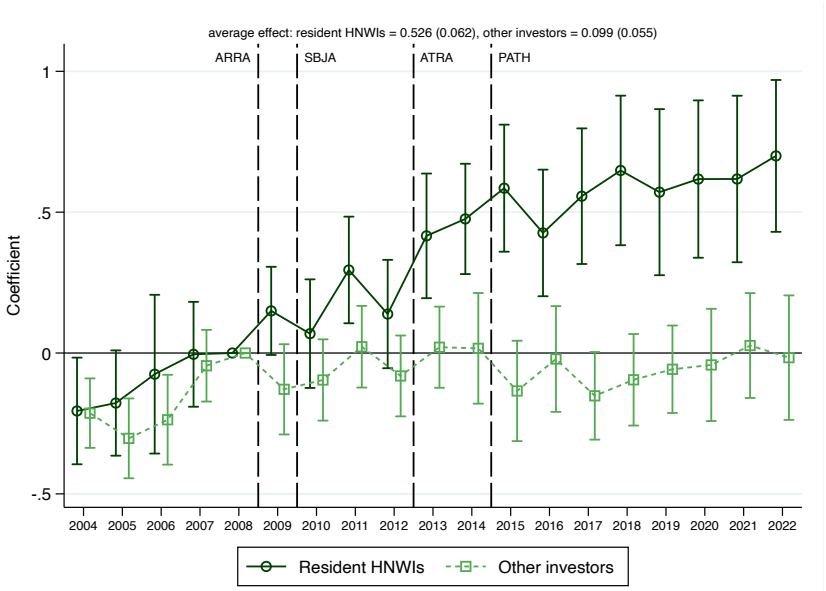
(d) All investments



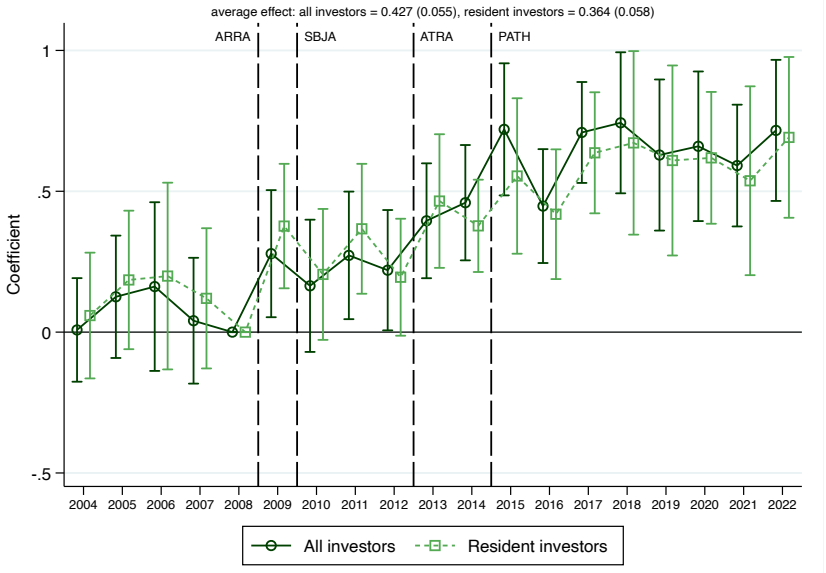
Source: Pitchbook, GEOWEALTH-US, Forbes.
Notes: This figure evaluates the robustness of the triple-difference estimates (with 95% confidence intervals) of Equation (2). Panel (a) compares our baseline estimates, which control for both the expected state-level long-term capital gains tax wedge on QSBS and the state-level house price index, to estimates that instead control for neither or only one of the two. Panel (b) compares our baseline estimates, which exploit state-level variation in the log number of resident accredited investors in 2008, to estimates that instead exploit variation in the log (1 plus) number of residents ranked in the Forbes 400 rich list in 2008 or of distinct resident HNWIs with early-stage investments in 2008. Panel (c) evaluates the robustness of our baseline estimates to dropping California, Colorado, the District of Columbia, Georgia, Illinois, Massachusetts, New York, Texas, and Washington, states which contain cities considered to be major technology hubs (see https://builtin.com/tech-hubs). Finally, Panel (d) extends Equation (2) to further consider each state's resident HNWIs' early-stage investments in companies headquartered in states other than their own. Since HNWIs did not allocate their early-stage investments equally across states (Appendix Figure C10), we weight each set of investor state-by-company state observations by the average across the 2004-2008 period of the share of each investor state's resident HNWIs' early-stage investments allocated to the companies headquartered in each company state.

Figure C15: Effects of the QSBS Reforms on Early-Stage Investments: Investor Groups

(a) Difference-in-differences estimates



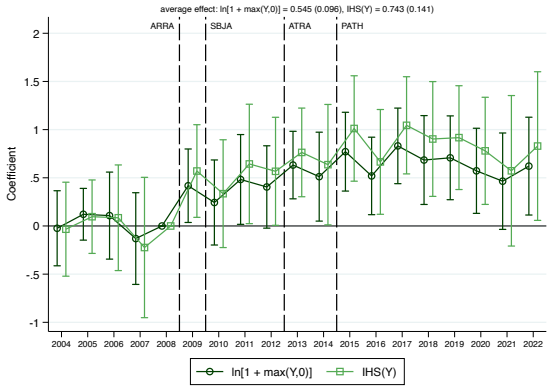
(b) Triple-difference estimates with only resident investors



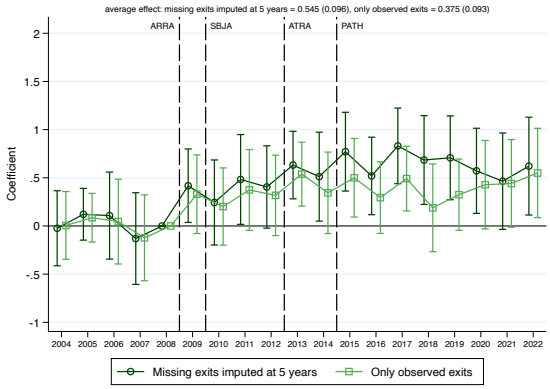
Source: Pitchbook, GEOWEALTH-US.
Notes: This figure analyzes the investor groups included in the triple-difference regression in Equation (2). Panel (a) plots the difference-in-difference estimates (with 95% confidence intervals) underlying our baseline triple-difference estimates, comparing the difference-in-difference estimates for resident HNWIs to the pooled ones for the three other "untreated" investor groups (i.e., resident institutions, non-resident institutions, and non-resident HNWIs). Panel (b) compares our baseline triple-difference estimates based on all four investor groups to ones based on only resident HNWIs and resident institutions.

Figure C16: Effects of the QSBS Reforms on Resident HNWIs' Early-Stage Capital Gains

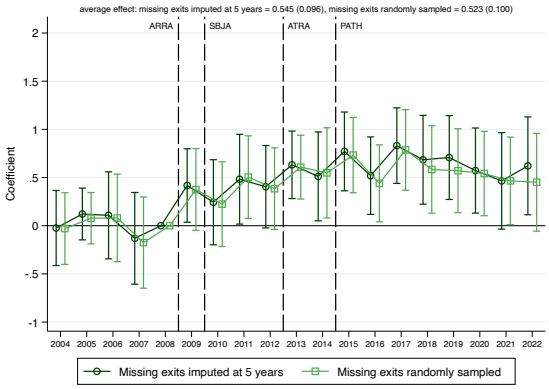
(a) Alternative transformations



(b) All exits vs. observed exits



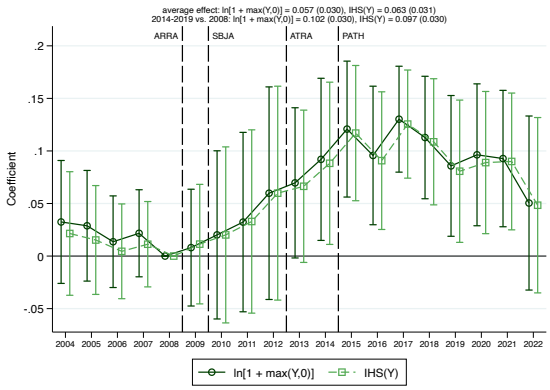
(c) Alternative impuations of missing exits



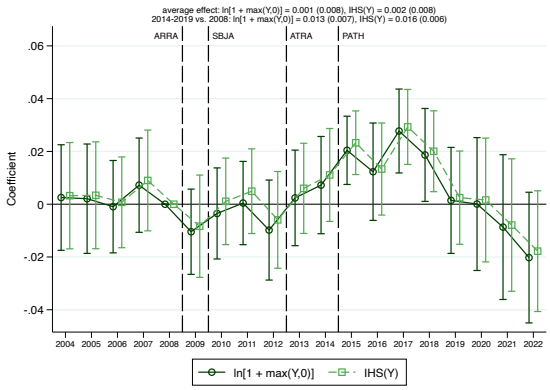
Source: Pitchbook, GEOWEALTH-US.

Figure C17: Implications for State-Level Income Inequality: Other Income

(a) Capital gains income vs. other income



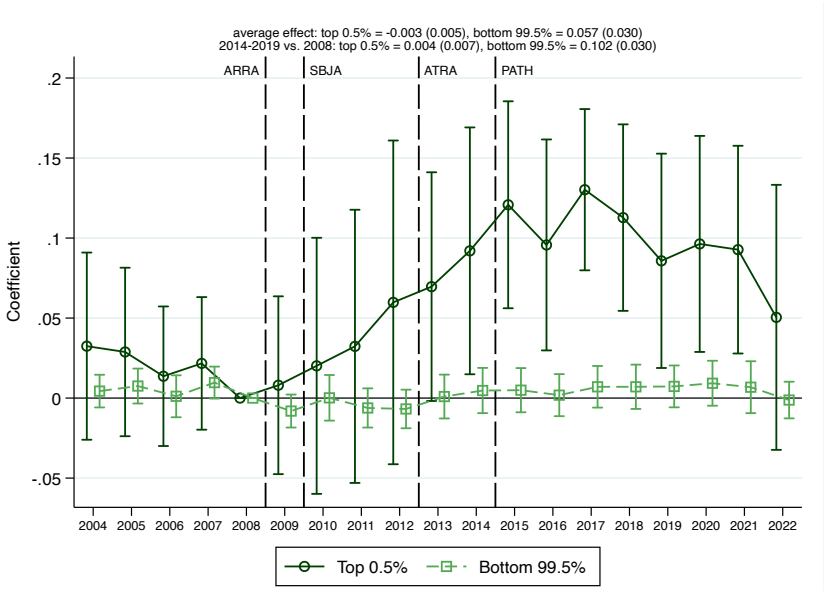
(b) Alternative transformations



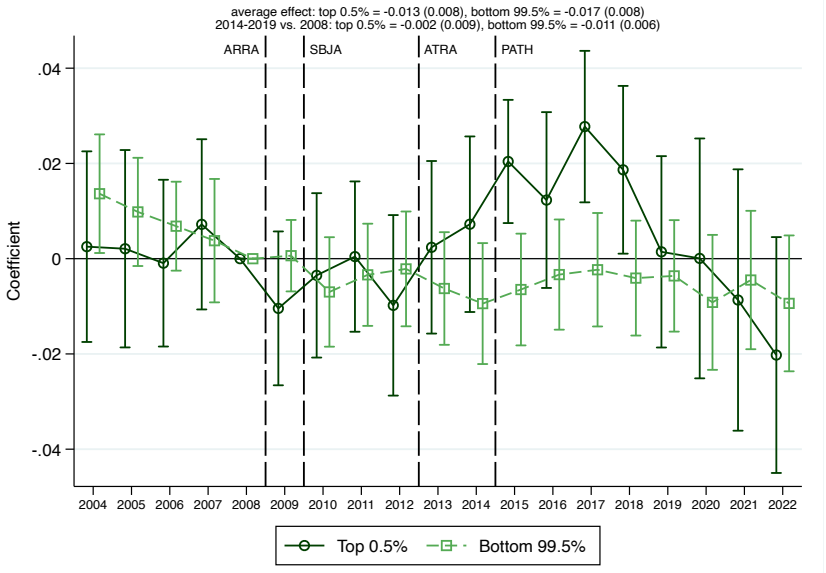
Source: IRS Statistics of Income, GEOWEALTH-US.

Figure C18: Implications for State-Level Income Inequality: Top 0.5% vs. Top 0.1%

(a) Capital gains income



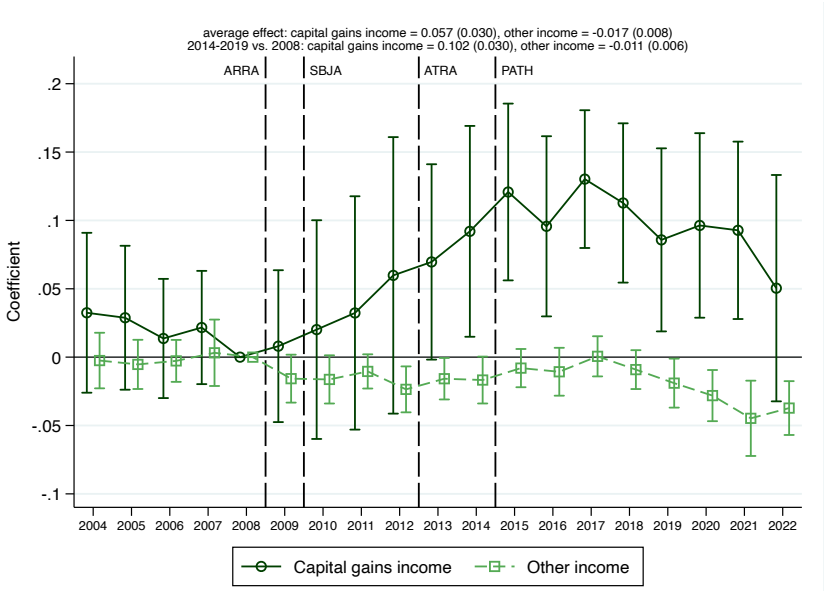
(b) Total income



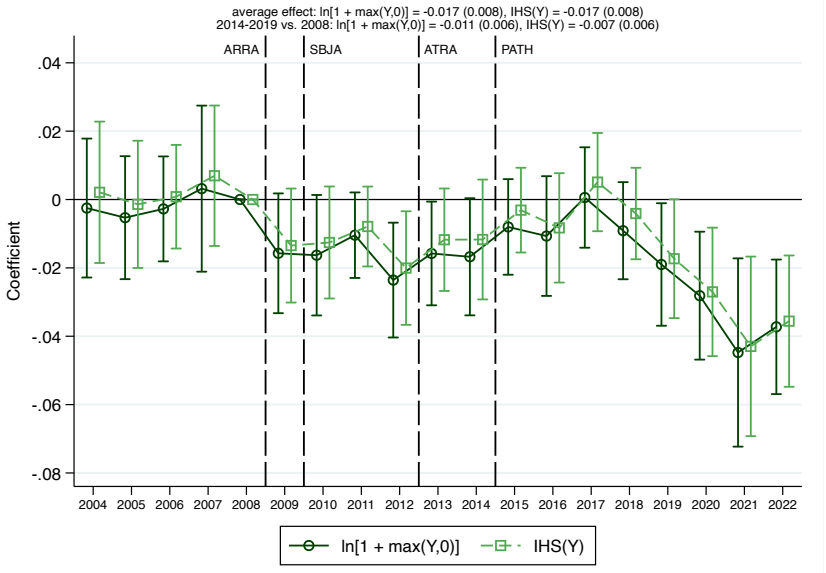
Source: IRS Statistics of Income, GEOWEALTH-US.

Figure C19: Implications for State-Level Income Inequality: Alternative Transformation

(a) Capital gains income



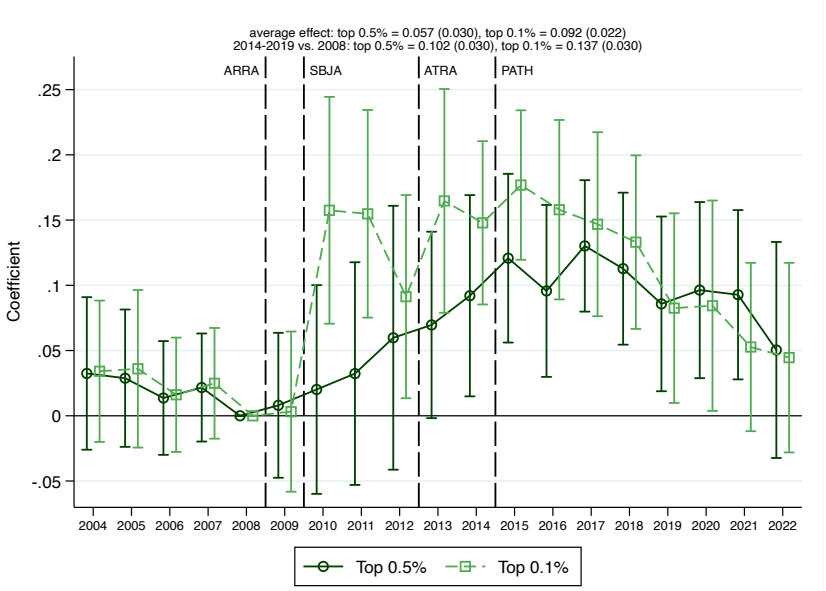
(b) Total income



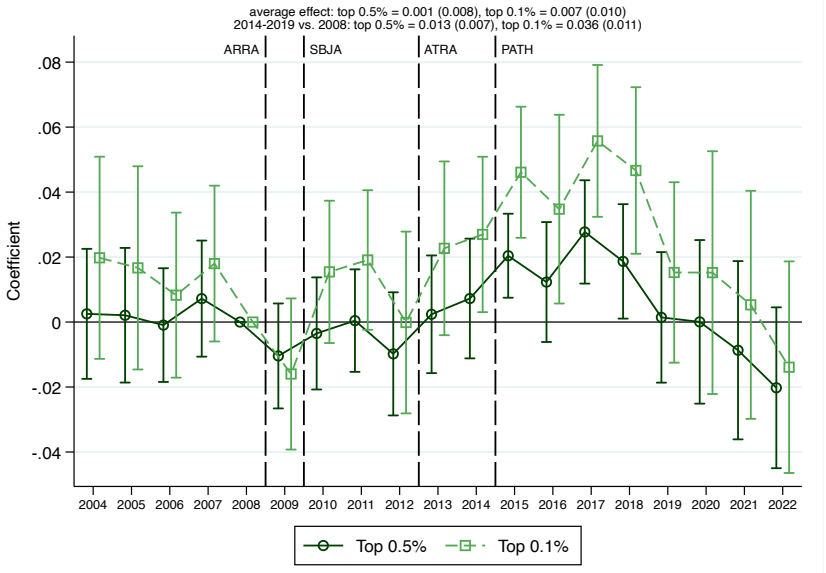
Source: IRS Statistics of Income, GEOWEALTH-US.

Figure C20: Implications for State-Level Income Inequality: Top 0.5% vs. Bottom 99.5%

(a) Capital gains income



(b) Total income



Source: IRS Statistics of Income, GEOWEALTH-US.

Appendix D: HNWIs, Income and Wealth Inequality

D.1 High-net-worth Individuals

This section validates the state-level measure of the number of resident HNWIs that we use in the regression analyses of Sections 4.3 in the main text. As described in Section 2.2, we rely on the measure from the GEOWEALTH-US dataset built by Suss et al. (2024), who define HNWIs so as to resemble the SEC's legal definition of accredited investors—that is, those whose net worth (excluding the value of their primary residence) exceeds $1 million, or whose household income exceeds $300,000. We validate this baseline measure by comparing it with alternative estimates of the number of HNWIs residing in the U.S. provided by the Phoenix/MarketCast Wealth and Affluent Monitor, the Forbes 400, the Credit Suisse/UBS Global Wealth Report, and the Survey of Consumer Finances.

Appendix Figure D1a depicts the correlation between the state-level average of the log number of resident HNWIs from the GEOWEALTH-US dataset and the analogous measure from the Phoenix/MarketCast Wealth and Affluent Monitor over the period 2006-2019. The latter measure is based on the estimated number of individuals with $1 million or more in investable assets residing in each U.S. state, which Phoenix/MarketCast constructs by combining information from the Survey of Consumer Finances with data from Nielsen-Claritas. The correlation between the two measures is quite high (0.9). The differences between the two measures are likely driven by the fact that Suss et al. (2024) estimate the number of resident accredited investors, while Phoenix/MarketCast estimate the number of resident millionaires (in terms of investable assets).

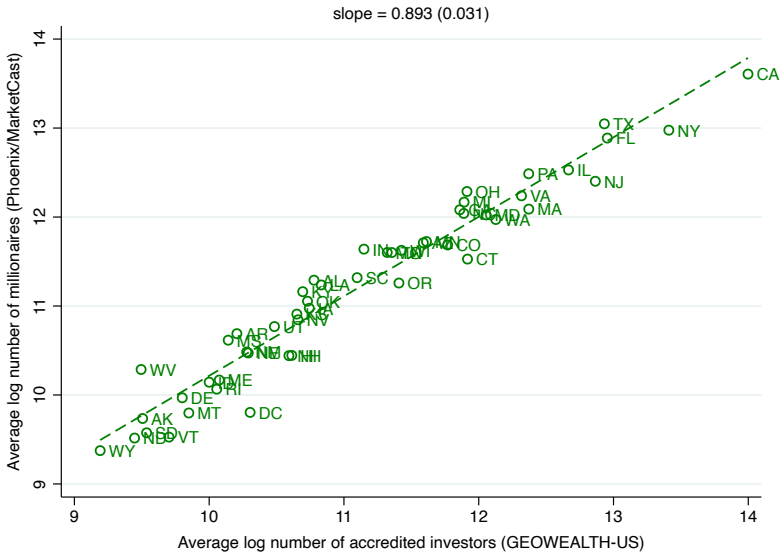
Appendix Figure D1b further depicts the correlation between the same state-level average from the GEOWEALTH-US dataset and an analogous measure based on the Forbes 400 list of the richest Americans, which has been published every year since 1982. We use the digitized and harmonized series from Saez and Zucman (2022). The correlation between the two measures is again quite high (0.8). The differences between the two measures are likely driven by the fact that the Forbes 400 list only includes billionaires.

Appendix Figure D2a compares the evolution of the total number of accredited investors residing in the U.S. from the GEOWEALTH-US dataset with the analogous measure from the Credit Suisse/UBS Global Wealth Report over the period 2006-2019. The time series correlation between the two measures is quite high (0.5). The differences between the two measures are likely driven by the fact that Credit Suisse/UBS estimates the number of millionaires, rather than the number of accredited investors. Furthermore, the former estimates are based on household units, while the latter are based on individual units.

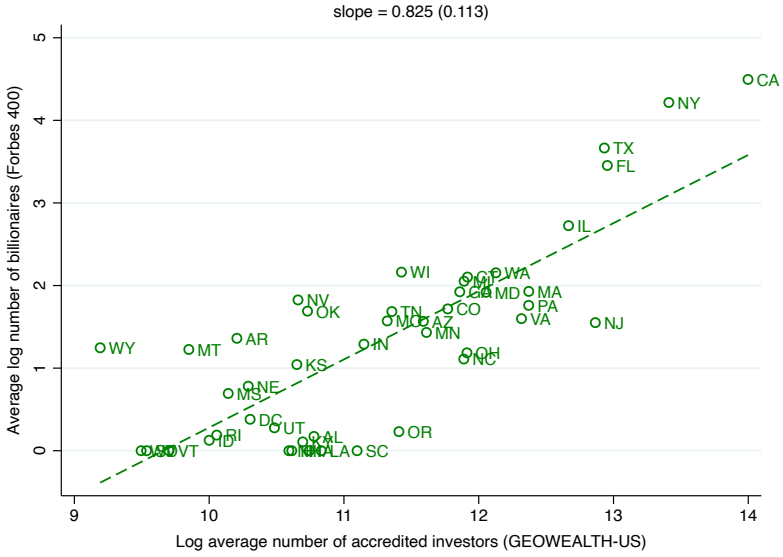
Finally, Appendix Figure D2b compares the same time series for the 2006-2022 period based on the GEOWEALTH-US with an analogous one based on the SCF.

Figure D1: HNWIs Residing in U.S. States, 2006-2019

(a) GEOWEALTH-US vs. Phoenix/MarketCast Wealth and Affluent Monitor



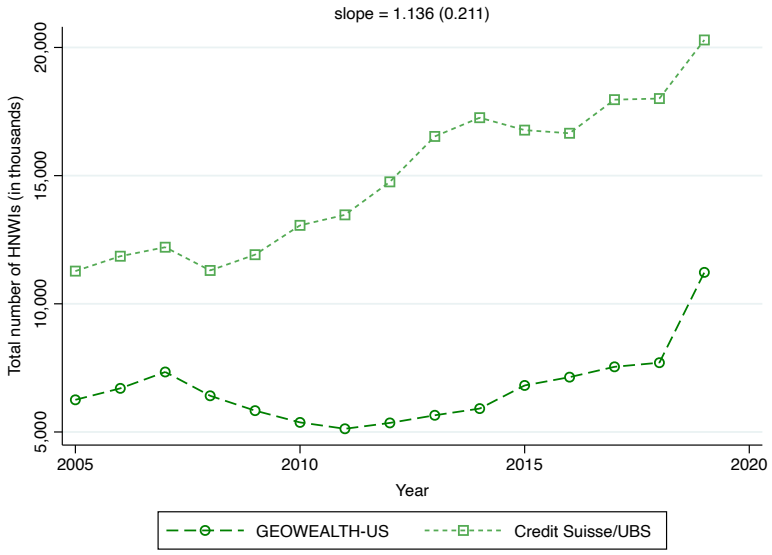
(b) GEOWEALTH-US vs. Forbes 400



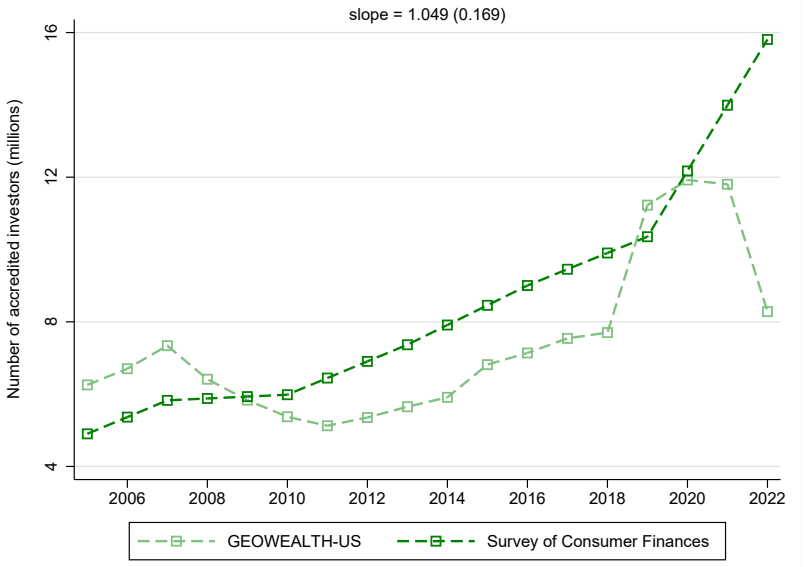
Source: Suss et al. (2024), Phoenix/MarketCast, Forbes.
Notes: This figure correlates the average log number of U.S. accredited investors across U.S. states between 2006 and 2019 in Suss et al. (2024), with the average log number of U.S. accredited millionaires across U.S. states derived by Phoenix/MarketCast Wealth and Affluent Monitor (panel (a)) on the one hand, and with the number of billionaires across U.S. states from the Forbes 400 rich lists (panel (b)), on the other hand. Suss et al. (2024) refers to the data from the GEOWEALTH-US database. The data from Phoenix/MarketCast comes from its Wealth and Affluent Monitor, while the data from Forbes comes from Saez and Zucman (2022).

Figure D2: U.S. HNWIs Time Series Evolution, 2006-2019

(a) GEOWEALTH-US vs. Credit Suisse/UBS Global Wealth Report



(b) GEOWEALTH-US vs. SCF



Source: Suss et al. (2024), Credit Suisse/UBS, SCF, Forbes.
Notes: This figure compares the evolution of the number of U.S. accredited investors in Suss et al. (2024) with the Credit Suisse Global Wealth Report (panel (a)) on the one hand, and with the number directly derived from the SCF (panel (b)), on the other hand. The comparison is made over the period 2005 and 2022. Suss et al. (2024) refers to the data from the GEOWEALTH-US database. The data from Credit Suisse/UBS comes from its Global Wealth Report. The SCF series is based on the adjustment to build Distributional Financial Accounts using the aggregate Financial Accounts and the Forbes 400 rich lists (see more details about the methodology in Section D.3.1).

D.2 Wealth inequality

D.2.1 Construction of the wealth distribution series

We build wealth distribution series for the United States following the Distributional Financial Accounts (DFA) methodology developed by Batty et al. (2020). The DFA builds on two existing Federal Reserve Board statistical products—quarterly aggregate measures of household wealth from the Financial Accounts (FA) of the United States, and triennial wealth distribution measures from the Survey of Consumer Finances (SCF)—to incorporate distributional information into a national accounting framework. The wealth distributional series are thus consistent with macro aggregates.

The definitions of assets and liabilities differ between the SCF and FA. The first step in the harmonization process is thus to reconcile the definitions of the major asset and liability categories, i.e. combining the subcategories of assets and liabilities in the survey such that the total is analogous to the definition of that asset or liability in the FA. Some components such as real estate, checkable deposits and home mortgages require minimal combinations, while others such as consumer durables require considerable adjustments. Some components such as life insurance are measured only indirectly in the SCF and must be imputed from the FA. Furthermore, the SCF does not record amounts of defined benefits (DB) pensions and is therefore augmented with data obtained on request from the Federal Reserve. These are proportionally assigned to households qualifying for them based on the age composition, as well as employment status of the members of the household.

We also follow the DFA methodology developed by Batty et al. (2020) to improve the SCF's ability to capture the top of the wealth distribution by relying on the Forbes 400 rich lists. We use data on the Forbes 400 rich lists from Saez and Zucman (2020). We complement these series with Moretti and Wilson (2017), who also gather the industry and the source of wealth for each member of the Forbes 400. We also use their series for 2004, as Saez and Zucman (2020) do not have this year in their database.

The Forbes rich lists only provide an estimate of net worth, so that we need to impute the Forbes' household amounts of the different subcomponents of assets and liabilities. We do so by assuming that the Forbes households have a similar portfolio of assets and liabilities to the top 0.1% of the SCF distribution. Finally, we rescale each survey component proportionally so as to match its value with the one in the FA. The final output are thus wealth distribution series which are consistent with national accounts and appropriately capture the top of the wealth distribution for every wave of the survey between 2004 and 2022. Appendix Table D1 presents summary statistics for U.S. top wealth groups in

2010 and 2022. In particular, it reports the wealth share, as well and the mean, median, minimum and maximum absolute wealth for each wealth group within the top 10%.

Table D1: Summary Statistics for U.S. Top Wealth Groups in 2010 and 2022

			Absolute Wealth			
Year	Wealth Group	Wealth Share	Mean	Median	Min	Max
2010	Top 10%	69.92%	5.04	2.8	1.63	98,767
	Top 0.5%	21.74%	31.33	19.72	13.23	98,767
	Top 0.1%	11.31%	81.31	40.66	29.77	98,767
	Forbes 400	2.95%	6,259	3658	1,829	98,767
2022	Top 10%	66.03%	6.89	3.67	2.15	262,393
	Top 0.5%	23.22%	48.47	26.78	16.19	262,393
	Top 0.1%	13.42%	139.72	70.35	45.41	262,393
	Forbes 400	3.05%	10,467	5,331	2,823	262,393

Sources: SCF, FA, Forbes 400.
Notes: This table presents summary statistics for U.S. top wealth groups for 2010 and 2022. Both wealth shares and absolute wealth are estimated based on the Distributional Financial Accounts (DFA) methodology developed by Batty et al. (2020), which combines the Financial Accounts (FA) with the Survey of Consumer Finances (SCF) and the Forbes 400 rich lists to build U.S. wealth distribution series consistent with macroeconomic aggregates. Wealth shares are reported in % and absolute wealth in 2022 constant million USD.

D.2.2 Wealth counterfactual simulations

Methodology. To understand the implications of the increasing importance of U.S. HNWIs in U.S. private capital markets for the overall dynamics of wealth inequality, we carry out counterfactual simulations and compare them with the baseline U.S. actual wealth distribution derived as explained in D.2.1. In particular, we estimate the wealth distribution had individuals instead invested the money they put in private capital markets in public capital markets, in particular, in the NASDAQ 100. This is the baseline public stock market index we use, since it is the one that puts more weight on tech companies and thus it is closer to the portfolio of early-stage companies (Appendix Figure A7). We rerun our counterfactual simulations using other public stock market indices, a hedge fund index, as well as the FANG+ index as a robustness check. Appendix Table D4 compares the baseline growth rate in the wealth share for different subgroups within the top 10% to the growth rate under the different counterfactual scenarios for each of the other indices.

To run the counterfactuals, we subtract from the baseline wealth distribution the private business capital gains from Pitchbook, that is, the accumulated value of early-stage investments in each year depicted in Figure 2a and estimated following the methodology explained in Appendix B. We then add the counterfactual accumulated value of public

gains depicted in Figure 2a, had they invested in the NASDAQ 100 or any of the other indices.

The main challenge we face when carrying out the counterfactuals is how to distribute the actual and counterfactuals accumulated gains along the wealth distribution in the SCF. For the Forbes 400 individuals this is not an issue, as we can match the individuals on the list to their Pitchbook profiles, as explained in Appendix A.3. We can thus directly attribute on an individual basis to each Forbes 400 individual the accumulated capital gains they earn on these investments. In 2022, we are able to match up to 16.5% of the Forbes 400 individuals to their Pitchbook profiles.

For assigning the remaining Pitchbook private business capital gains, we need to rely on an imputation procedure as—with the exception of the Forbes 400 individuals—we cannot match the identities of the Pitchbook investors to the identities of the anonymous households in the SCF. For that, we first need to identify the population of investors in the SCF to whom we attribute the Pitchbook capital gains. We thus consider as private capital investors accredited investors (i.e., individuals whose combined current net worth with their spouse (excluding the value of their primary residence) exceeds $1 million, or whose combined current (individual) income exceeds $300,000 ($200,000)), who report owning private business wealth of a partnership or a non-S corporation type, as these tend to be the type of companies that receive early-stage financing.

To distribute the accumulated capital gains to these investors, we account for the possibility that investors with different levels of private business assets obtain different rates of return and thus different capital gains in proportion to their business assets. For that, we assume that the end-of-year value of accumulated private investments is proportional to their end-of-year business wealth and rank the population of investors in Pitchbook excluding the Forbes 400 individuals by their end-of-year value of accumulated investments every year into 100 percentiles. Similarly, we also rank the population of accredited investors in the SCF excluding the Forbes 400 individuals by their level of private business wealth (i.e., partnership or non-S corporation business wealth) into the same 100 percentiles. To ensure the Pitchbook distribution of end-of-year value of accumulated private investments and the SCF distribution of end-of-year private business wealth are comparable, we further adjust the SCF distribution so that the median of private business wealth for our population of investors in the SCF matches the median of accumulated private private investments in Pitchbook.

The end-of-year accumulated private investments from Pitchbook are distributed proportionally to each household's business wealth in the SCF on an annual basis. This methodology ensures heterogeneity in returns coming from the direct assignment of capital

gains to the Forbes 400 individuals, as well as from differences in returns across the private business wealth distribution in the imputation procedure.

Finally, although Pitchbook provides a very rich snapshot of HWNIs private investments and its returns, it does not capture the universe of HWNIs private investments in early-stage companies. Thus, we miss part of the private capital gains HNWIs earn in this market (see Appendix Figure D3). We thus rescale the private and counterfactual public capital gains we derive from Pitchbook to get a more accurate aggregate number for capital gains. For that, we use as a multiplier the gap we observe between the total excluded QSBS realized gains (152 billion USD) officially reported by the IRS (Abdulrauf et al., 2025), and the estimate of excluded QSBS gains (28 billion USD) we derive from Pitchbook over the period 2012-2022. To estimate the excluded QSBS gains from Pitchbook, we rely on our baseline accumulated value of early-stage investments estimated following the methodology explained in Appendix B and proceed with the following three steps: first, we identify the early-stage investments held for at least 5 years in QSBS-eligible issuers, defined as U.S. C corporations active primarily in qualified trades or businesses that had raised no more than $50 million in financing by the date of the investment; second, we calculate the maximum excludable amount as the greater of $10 million and 10 times the initial investment amount; and third, we calculate the excluded positive capital gains based on the the date of the investment, differentiating between the 50%, 75%, and 100% exclusions. This implies an approximate rescaling of 5.4 times the Pitchbook capital gains. We run counterfactuals with and without rescaling capital gains for different top wealth groups, in particular, the top 10%, top 0.5%, the top 0.1%, and the Forbes 400.

Results. All appendix results with regards to the wealth inequality simulations are included below.

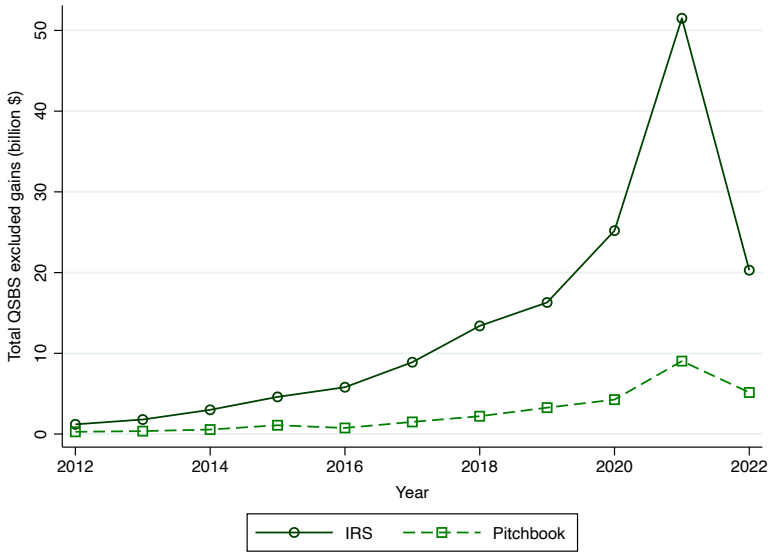
Table D2: Counterfactuals: Heterogeneous vs. Homogeneous Returns (2010–2022)

		W/o excess returns (NASDAQ)	
	Baseline	W/ Heterogeneous Returns	W/ Homogeneous Returns
Panel A: W/o rescaling			
Absolute growth rate	6.80%	6.46%	6.74%
Share of growth rate	/	5.00%	0.88%
Panel B: W/ rescaling			
Absolute growth rate	6.80%	5.04%	6.12%
Share of growth rate	/	25.88%	10.00%

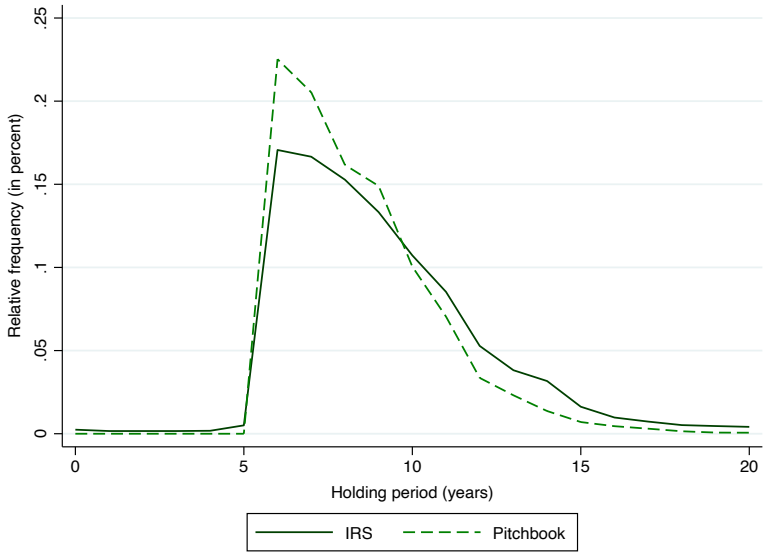
Sources: Pitchbook, SCF, FA, Forbes 400, Bloomberg.
Notes: This table compares the baseline growth rate in the top 0.5% wealth share between 2010 and 2022 with counterfactuals under two scenarios: one with no heterogeneous private capital gains but with counterfactual public capital gains, using NASDAQ 100 as the public stock market index; and another one with no homogeneous private capital gains but with counterfactual public capital gains, using NASDAQ 100 as the public stock market index. The table also reports the share of growth in the top 0.5% wealth share explained by either homogeneous or heterogeneous excess returns. This share is derived as one minus the ratio between the counterfactual growth rate in the wealth share without excess returns and the baseline growth rate in the wealth share. Panel A reports the counterfactual growth rates without rescaling capital gains, while Panel B reports the counterfactual growth rates after rescaling capital gains.

Figure D3: Comparison of QSBS excluded gains: IRS vs. Pitchbook

(a) Total QSBS Excluded Gains



(b) Distribution of Holding Periods for Assets Eligible for QSBS Exclusions



Source: Abdulrauf et al. (2025), Pitchbook.
Notes: This figure compares in panel (a) the total 1202 exclusion amounts claimed annually from 2012 to 2022 on e-filed tax returns by individual taxpayers, complex trusts, and other (i.e., estates, simple trusts, and other non-individual entities filing form 8949) as reported by the IRS (see Abdulrauf et al., 2025), to the same definition of excluded gains that we can quantify via Pitchbook. Panel (b) compares the relative frequency distribution of holding periods for assets eligible for QSBS exclusions among individual taxpayers, complex trusts and other as reported by the IRS (see Abdulrauf et al., 2025), to the one we can derive via Pitchbook. The IRS numbers are just an approximation based on the graphs in Abdulrauf et al. (2025), since the authors did not have legal permission to share the underlying data with us.

Table D3: Wealth Inequality Counterfactuals: Excess Returns using Different Indices (2010-2022)

		W/o excess returns				
	Baseline	W/ NASDAQ	W/ S&P 500	W/ RUT	W/ BHFI	W/ FANG
Panel A: W/o rescaling						
Absolute growth rate	6.80%	6.46%	6.43%	6.41%	6.39%	6.52%
Share of growth rate	/	5.00%	5.44%	5.74%	6.03%	4.12%
Panel B: W/ rescaling						
Absolute growth rate	6.80%	5.04%	4.79%	4.87%	4.59%	5.32%
Share of growth rate	/	25.88%	29.56%	28.38%	32.50%	21.76%

Source: Survey of Consumer Finances, Pitchbook, FA, Forbes 400, Bloomberg, Benchmark Database.
Notes: This table compares the growth rates between the baseline cumulative U.S. top 0.5% wealth share and the different counterfactuals (without private capital gains but with any of the different indices (i.e., NASDAQ 100, S&P 500, Russell 2000, Barclay Hedge Fund, and FANG+ index)) between 2010 and 2022. The table also reports below the absolute growth the share of growth explained by the different excess returns. This share is derived as one minus the ratio between the counterfactual growth rate in the wealth share without excess returns and the baseline growth rate in the wealth share. Panel A reports the counterfactual growth rates without rescaling capital gains, while Panel B reports the counterfactual growth rates after rescaling capital gains.

Table D4: Counterfactuals: Growth of Non-Cumulative Top Wealth Shares (2010-2022)

		W/o excess returns				
	Baseline	W/ NASDAQ	W/ S&P 500	W/ RUT	W/ BHFI	W/ FANG+
Panel A: W/o rescaling						
Top 10%-0.5%	-11.13%	-11.06%	-11.06%	-11.06%	-11.04%	-11.07%
/	/	0.60%	0.62%	0.64%	0.80%	0.54%
Top 0.5%-0.1%	-6.14%	-6.27%	-6.29%	-6.29%	-6.31%	-6.23%
/	/	2.18%	2.49%	2.53%	2.77%	1.62%
Top 0.1%-Forbes 400	24.14%	23.46%	23.43%	23.39%	23.37%	23.55%
/	/	2.80%	2.93%	3.11%	3.19%	2.43%
Forbes 400	3.45%	3.36%	3.32%	3.29%	3.26%	3.45%
/	/	2.79%	3.91%	4.84%	5.62%	0.05%
Panel B: W/ rescaling						
Top 10%-0.5%	-11.13%	-10.74%	-10.66%	-10.76%	-10.67%	-10.74%
/	/	3.48%	4.18%	3.28%	4.11%	3.46%
Top 0.5%-0.1%	-6.14%	-6.78%	-7.12%	-6.78%	-7.25%	-6.60%
/	/	10.45%	16.09%	10.53%	18.17%	7.63%
Top 0.1%-Forbes 400	24.14%	20.59%	20.45%	20.30%	20.21%	20.92%
/	/	14.69%	15.28%	15.92%	16.27%	13.32%
Forbes 400	3.45%	2.93%	2.71%	2.54%	2.39%	3.44%
/	/	15.25%	21.40%	26.50%	30.79%	0.30%

Source: Survey of Consumer Finances, Pitchbook, FA, Forbes 400, Bloomberg, Benchmark Database.
Notes: This table compares the growth rates between the baseline non-cumulative U.S. top wealth shares and the different counterfactuals (without private capital gains but with any of the different indices (i.e., NASDAQ 100, S&P 500, Russell 2000, Barclay Hedge Fund, and FANG+ index)) between 2010 and 2022. The table also reports below the absolute growth the share of growth for every wealth group explained by the different excess returns. This share is derived as one minus the ratio between the counterfactual growth rate in the wealth share without excess returns and the baseline growth rate in the wealth share. Note that whenever the growth rates are negative, we report the absolute value of one minus the ratio to ensure consistency. Panel A reports the counterfactual growth rates without rescaling capital gains, while Panel B reports the counterfactual growth rates after rescaling capital gains.

Table D5: Counterfactuals: Growth of Cumulative Top Wealth Shares (2010-2022)

		W/o excess returns				
	Baseline	W/ NASDAQ	W/ S&P 500	W/ RUT	W/ BHFI	W/ FANG+
Panel A: W/o rescaling						
Top 10%	-5.55%	-5.61%	-5.62%	-5.63%	-5.62%	-5.60%
	/	1.08%	1.26%	1.44%	1.26%	0.90%
Top 0.5%	6.80%	6.46%	6.43%	6.41%	6.39%	6.52%
	/	5.00%	5.44%	5.74%	6.03%	4.12%
Top 0.1%	18.74%	18.21%	18.18%	18.14%	18.12%	18.30%
	/	2.83%	2.99%	3.20%	3.31%	2.35%
Forbes 400	3.45%	3.36%	3.32%	3.29%	3.26%	3.45%
	/	2.61%	3.77%	4.64%	5.51%	0.00%
Panel B: W/ rescaling						
Top 10%	-5.55%	-5.84%	-5.87%	-5.91%	-5.93%	-5.76%
	/	5.23%	5.77%	6.49%	6.85%	3.78%
Top 0.5%	6.80%	5.04%	4.79%	4.87%	4.59%	5.32%
	/	25.88%	29.56%	28.38%	32.50%	21.76%
Top 0.1%	18.74%	15.97%	15.81%	15.65%	15.55%	16.35%
	/	14.78%	15.64%	16.49%	17.02%	12.75%
Forbes 400	3.45%	2.93%	2.71%	2.54%	2.39%	3.44%
	/	15.07%	21.45%	26.38%	30.72%	0.29%

Source: Survey of Consumer Finances, Pitchbook, FA, Forbes 400, Bloomberg, Benchmark Database.
Notes: This table compares the growth rates between the baseline cumulative U.S. top wealth shares and the different counterfactuals (without private capital gains but with any of the different indices (i.e., NASDAQ 100, S&P 500, Russell 2000, Barclay Hedge Fund, and FANG+ index)) between 2010 and 2022. The table also reports below the absolute growth the share of growth for every wealth group explained by the different excess returns. This share is derived as one minus the ratio between the counterfactual growth rate in the wealth share without excess returns and the baseline growth rate in the wealth share. Note that whenever the growth rates are negative, we report the absolute value of one minus the ratio to ensure consistency. Panel A reports the counterfactual growth rates without rescaling capital gains, while Panel B reports the counterfactual growth rates after rescaling capital gains.

D.3 Income inequality

D.3.1 Construction of the income distribution series

We build pre- and post-tax income distribution series for the U.S. by relying on the Internal Revenue Service's historical Statistics of Income (SOI) Tax statistics available for the period 1996-2022. For each state in each year, these data files provide personal income tax tabulations disaggregated by bracket of adjusted gross taxable income (i.e., gross taxable income net of deductibles and other adjustments[48]) and by income component (i.e., wages, business profits, capital gains, interest, etc.). The personal income tax tabulations

[48] These adjustments include business expenses, alimony, student loan interest payments, and certain educator and military expenses.

also contain information about personal income tax liabilities by adjusted gross taxable income bracket, so that we can use these statistics to derive federal and state-level pre- and post-tax income distribution series.

We build the pre-tax income distribution series by applying the generalized pareto interpolation (GPI) method developed by Blanchet et al. (2022). GPI is a non-parametric approach that avoids the assumptions of a Pareto approximation, which are often violated by empirical data. The unit of observation in the IRS statistics is the tax unit. For every state in each year, we construct the individual and state-level pre-tax income distribution series, assuming that the reported income of couples filing jointly is shared equally between spouses. We are able to generate income shares for 127 g-percentiles, whereby the top percentile is split into smaller income groups up to the top 0.01%. Our series are consistent with those of Sommeiller and Price (2018), who also build state-level pre-tax income inequality series for the U.S. using IRS personal income tax tabulations available between 1917 and 2015 (Appendix Figure D4). The differences between Sommeiller and Price (2018) series and ours lies on the fact that they use tax units as the unit of observation and we use individual units. As there are more joint filers at the top of the income distribution, using tax units instead of equal-splitting these units—as we do in our methodology—increases the concentration of income at the top.

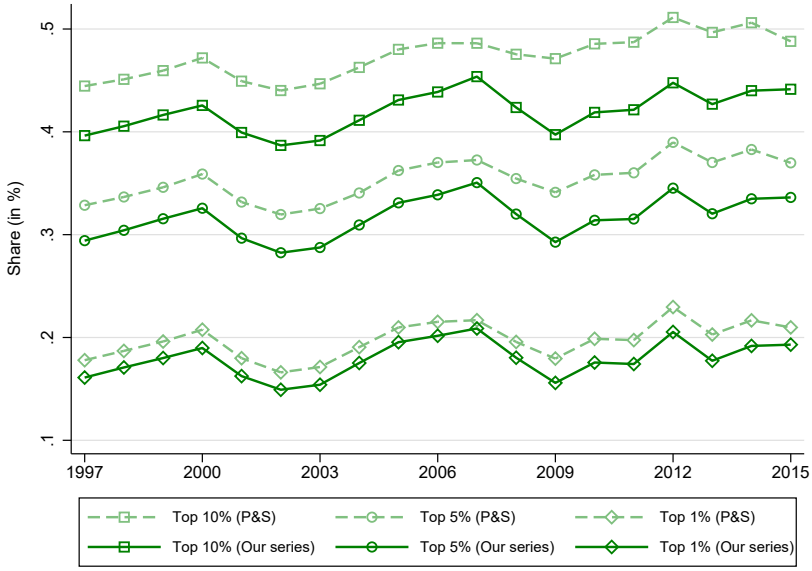
Using the information available on the composition of income for each tax bracket, we further decompose the income distribution series into its different subcomponents, namely labor income, capital income (i.e., dividends, interest income, and other income from investments), and realized capital gains. We rely on these three income components, as the specific sub-components that are reported in the raw statistics each year vary over time. Appendix Table D6 summarizes which are the different IRS income subcomponents that we attribute to each of the three subcomponents into which we decompose adjusted gross taxable income (i.e., labor income, capital income and realized capital gains).

Table D6: Income components and IRS counterparts

Component	**IRS equivalent**
Labor income	Salaries and wages; Social security and unemployment payments
Capital income	Taxable interest; Dividends; Business and professional net income
Capital gains	Net capital gains (gains minus losses)

From the personal income tax tabulations, we can calculate for every income bracket the share of each bracket's total income that is derived from labor, capital and capital gains sources. We can thus directly apply these shares to the different percentiles by matching each percentile to its corresponding income bracket. This allows for heterogeneity in the composition of income across the income distribution, with bottom income groups

Figure D4: Income concentration in the U.S. (Price & Sommeiller vs. Our series)



Source: IRS.
Notes: This figure compares the top income distribution series we estimate with the ones previously estimated by Sommeiller and Price (2018). We run the comparisons for the 1997-2015 period, as these are the years for which both series are available. Both series are estimated using the IRS personal income tabulations for the U.S. The differences between Sommeiller and Price (2018) series and ours lies on the fact that they use tax units as the unit of observation and we use individual units. As there are more joint filers at the top of the income distribution, using tax units instead of equal-splitting these units (as we do in our methodology) increases the concentration of income at the top.

obtaining more income from labor sources relative to capital, and the reverse being true for upper income groups.

A similar procedure is used to estimate the absolute amounts of income–for both total income and its different subcomponents—that accrue to each percentile of the income distribution. For that, we multiply the different income shares by percentile, as well as those of the different subcomponents by their corresponding income total. This procedure preserves the heterogeneous composition of income along the income distribution, but can lead to aggregate labor, capital and capital gains income slightly differing from the raw data. An additional simple rescaling is implemented to harmonize these to be consistent with the raw data files. Following this procedure, we obtain pre-tax income distribution series for total income and its main subcomponents for 127 g-percentiles in each state and in each year from 2004-2022.

To build the post-tax income distribution series, we start from the federal pre-tax income distribution series. Since the raw IRS files provide information about the total personal income tax liabilities corresponding to each income bracket, we can also rely on the GPI to match the personal income tax liabilities in each bracket to its corresponding percentile. We can then estimate the post-tax income in each percentile by applying to the total pre-tax income in every percentile the average personal income tax rate corresponding to that percentile. We calculate the average personal income tax rate in each percentile as the ratio of the total personal income liabilities to the total taxable income in each percentile. Following this procedure, we obtain post-tax income distribution series for 127 g-percentiles in each year from 2004-2022. Appendix Figure D5 compares the evolution of pre- and post-tax income concentration in the U.S. for the top 10% income group and different subgroups within it. As expected, the levels of post-tax income concentration are lower than those of pre-tax income concentration, since the personal income tax system is progressive.

Appendix Table D7 presents summary statistics for U.S. pre-tax top income groups in 2010, 2019, and 2022. In particular, it reports the pre-tax income shares, as well and the mean, median, and minimum absolute pre-tax income for each income group within the top 10%. Appendix Table D3 presents summary statistics for U.S. post-tax top income groups in 2010, 2019, and 2022. In particular, it reports the post-tax income shares, as well and the mean, median, and minimum absolute post-tax income for each income group within the top 10%. The top 0.5% pre-tax income group had an average income of 11.51 million USD and concentrated 14.06% of total income in 2010. The income share went up to 17.03% in 2022 and its average income more than doubled reaching 23.49 million USD. Due to the progressivity of the personal income tax, the levels of post-tax income concentration are lower than those of pre-tax income concentration. In particular, the

top 0.5% post-tax income group had an average income of 8.56 (16.5) million USD and concentrated 12.02% (14.10%) of total income in 2010 (2022).

Table D7: Summary Statistics for U.S. Top Pre-Tax Income Groups in 2010, 2019 and 2022

			Absolute Income		
Year	Income Group	Income Share	Mean	Median	Min
2010	Top 10%	41.89%	1.71	1.02	0.74
	Top 5%	31.40%	2.57	1.62	1.02
	Top 1%	17.57%	7.19	3.62	2.36
	Top 0.5%	14.06%	11.51	6.53	3.62
	Top 0.1%	8.63%	35.32	16.89	10.48
	Top 0.01%	4.40%	180.18	85.22	52.2
2019	Top 10%	44.04%	2.48	1.47	1.02
	Top 5%	33.33%	3.75	2.35	1.47
	Top 1%	18.72%	10.53	5.35	3.53
	Top 0.5%	14.93%	16.79	9.69	5.35
	Top 0.1%	9.08%	51.08	24.76	15.42
	Top 0.01%	4.58%	257.64	123.45	75.87
2022	Top 10%	45.92%	3.17	1.75	1.22
	Top 5%	35.43%	4.89	2.92	1.75
	Top 1%	20.90%	14.42	6.77	4.39
	Top 0.5%	17.03%	23.49	12.53	6.77
	Top 0.1%	10.88%	75.07	33.31	20.32
	Top 0.01%	5.90%	406.67	178.23	107.23

Sources: IRS.
Notes: This table presents summary statistics for U.S. top pre-tax income groups for 2010, 2019, and 2022. Both pre-tax income shares and absolute pre-tax income are estimated by applying the generalized pareto interpolation (GPI) method developed by Blanchet et al. (2022). Pre-tax income shares are reported in % and absolute pre-tax income in 2022 constant million USD. This table does not include the maximum pre-tax absolute income, as we are using income tabulations, which do not provide information on the maximum absolute income at the top-end of the income distribution.

D.3.2 Income inequality simulations

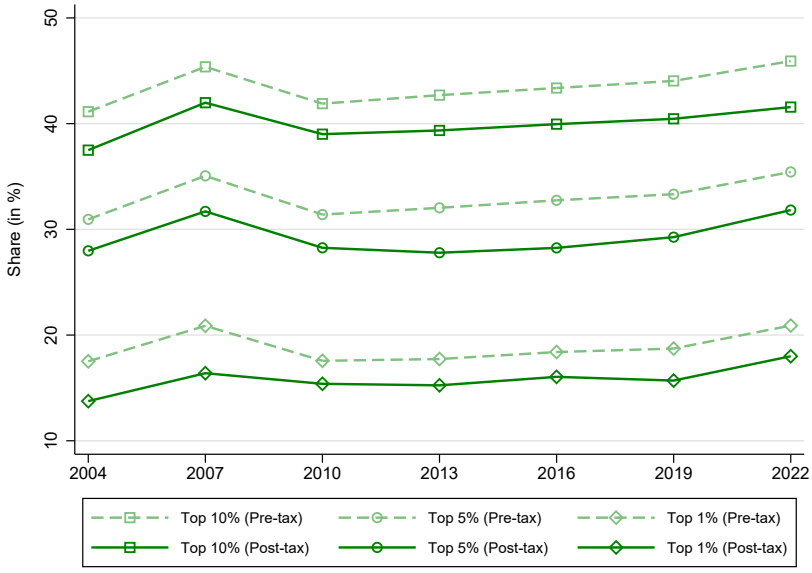
Methodology. To ensure consistency with the state-level income inequality analysis in Section 5.1, the baseline counterfactual analyses are based on the top 0.5% income group. We run five different counterfactual simulations for the post 2010-2022 period using as baseline the U.S. actual taxable income distribution derived as explained in Appendix D.3.1. To ensure that baseline pre-tax income does not fluctuate because of changes in the QSBS exemption—leading to higher or lower taxable private capital gains—we use as baseline pre-tax income for our simulations a modified version which accounts for total

Table D8: Summary Statistics for U.S. Top Post-Tax Income Groups in 2010, 2019 and 2022

			Absolute Income		
Year	Income Group	Income Share	Mean	Median	Min
2010	Top 10%	39.00%	1.39	1.01	0.58
	Top 5%	28.25%	2.01	1.11	1.01
	Top 1%	15.39%	5.48	2.52	2.11
	Top 0.5%	12.02%	8.56	5.9	2.52
	Top 0.1%	6.97%	24.8	14.54	9.5
	Top 0.01%	2.90%	103.2	59.63	38.83
2019	Top 10%	40.46%	1.96	1.11	1.02
	Top 5%	29.26%	2.83	2.18	1.11
	Top 1%	15.70%	7.59	5.06	2.53
	Top 0.5%	12.56%	12.15	8.6	5.06
	Top 0.1%	6.51%	31.48	19.46	13.1
	Top 0.01%	2.53%	122.47	74.81	49.7
2022	Top 10%	41.57%	2.43	1.19	1.09
	Top 5%	31.82%	3.72	2.51	1.19
	Top 1%	18.00%	10.52	6.04	2.55
	Top 0.5%	14.10%	16.5	11.18	6.04
	Top 0.1%	7.71%	45.07	25.71	16.86
	Top 0.01%	3.31%	193.55	108.88	70.22

Sources: IRS.
Notes: This table presents summary statistics for U.S. top post-tax income groups for 2010, 2019, and 2022. Both post-tax income shares and absolute post-tax income are estimated by applying the generalized pareto interpolation (GPI) method developed by Blanchet et al. (2022). Post-tax income shares are reported in % and absolute post-tax income in 2022 constant million USD.

Figure D5: Pre vs. Post-Tax Income Concentration in the U.S.



Source: IRS.
Notes: This figure compares the pre- and post-tax top income distribution series we estimate using the IRS Statistics of Income for the U.S. for the 2004-2022 period.

(and not only taxable) private capital gains. We start the simulations in 2010, since it is the first available post-reform year for which there is a wave of the SCF available.

The income concept that is used to build the baseline taxable income distribution series consists of the sum of labor income, capital income and realized capital gains (Appendix D.3.1). Hence, we cannot simply rely on the accumulated value of early-stage investments in each year depicted in Figure 2a to run our counterfactual income simulations. The reason is that this measure contains both unrealized and realized gains. Instead, we refine this measure so as to only capture the accumulated realized value of early-stage investments in each year. We do so by only considering the accumulated value of early-stage investments until the observed exit date. Whenever the exit date is not observed, we assume that the exit date happens five years after the investment was made. This assumption is based on the fact that the average exit time for observed exits for early-stage investments in Pitchbook is 4.7 years. We can thus obtain a time series of accumulated realized gains out of early-stage investments for the period 2004-2022, with which we can run the counterfactual income inequality simulations.

The first counterfactual assesses how much the excess returns have contributed to the rise in pre-tax income inequality. For that, we re-estimate pre-tax income inequality after subtracting the pre-tax private capital gains and adding the counterfactual pre-tax gains had HNWIs invested this money in the NASDAQ 100. This is the baseline public stock market index we use, since it is the one that puts more weight on tech companies and

thus it is closer to the portfolio of early-stage companies (Appendix Figure A7). We rerun our counterfactual simulations using other public stock market and hedge fund indices as a robustness check. The second counterfactual quantifies how much the after-tax excess returns have contributed to the rise in pre-tax income inequality. For that, we re-estimate pre-tax income inequality after subtracting the after-tax private capital gains and adding the counterfactual after-tax gains had HNWIs invested this money in the NASDAQ 100.

The third simulation assesses how much the difference in taxes payed—resulting from the QSBS exemption—has contributed to the rise in pre-tax income inequality. For that, we need to re-estimate pre-tax income inequality after subtracting the taxes payed on private capital gains and adding the taxes payed had HNWIs invested this money in the NASDAQ 100. The difference in taxes payed is driven by both the difference in tax rates between QSBS capital gains and public or other non-QSBS capital gains—tax wedge effect—and the additional amount of taxes payed by HNWIs because of excess returns—additional tax effect—. The fourth and fifth simulations thus help us to examine how have the tax wedge and the additional tax effect shaped pre-tax income inequality, respectively. For the tax wedge effect, we re-estimate pre-tax income inequality after subtracting the post-tax private capital gains and adding the post-tax private capital gains had these gains been taxed under the non-QSBS tax schedule. For the additional tax effect, we re-estimate pre-tax income inequality after subtracting the taxes payed had private capital gains been taxed under the non-QSBS tax schedule and adding the taxes payed had HNWIs invested this money in the NASDAQ 100.

The main challenge we face when carrying out the counterfactuals is how to distribute the actual and counterfactuals realized gains along the income distribution in the SCF. For the Forbes 400 individuals this is not an issue, as we can match the individuals on the list to their Pitchbook profiles, as explained in Appendix A.3. We can thus directly attribute on an individual basis to each Forbes 400 individual the realized capital gains they earn on these investments. In 2022, we are able to match up to 16.5% of the Forbes 400 individuals to their Pitchbook profiles. For assigning the remaining Pitchbook private business capital gains, we need to rely on an imputation procedure as—with the exception of the Forbes 400 individuals—we cannot match the identities of the Pitchbook investors to the identities of the anonymous households in the SCF. For that, we first need identify the population of investors in the SCF to whom we attribute the Pitchbook capital gains. We thus consider as private capital investors accredited investors (i.e., individuals whose combined current net worth with their spouse (excluding the value of their primary residence) exceeds $1 million, or whose combined current (individual) income exceeds $300,000 ($200,000)), who report owning private business wealth of a partnership or a non-S corporation type, as these tend to be the type of companies that receive early-stage financing.

To distribute the realized capital gains to these investors, we account for the possibility that investors with different levels of private business assets obtain different rates of return and thus different capital gains in proportion to their business assets. For that, we assume that the end-of-year value of accumulated private investments is proportional to their end-of-year business wealth and rank the population of investors in Pitchbook excluding the Forbes 400 individuals by their end-of-year value of accumulated investments every year into 100 percentiles. Similarly, we also rank the population of accredited investors in the SCF excluding the Forbes 400 individuals by their level of private business wealth (i.e., partnership or non-S corporation) into the same 100 percentiles. To ensure the Pitchbook distribution of end-of-year value of accumulated private investments and the SCF distribution of end-of-year private business wealth are comparable, we adjust the SCF distribution so that the median of private business wealth for our population of investors in the SCF matches the median of accumulated private private investments in Pitchbook.

The realized private capital gains from Pitchbook are then distributed proportionally to each household's income in the SCF on an annual basis. This methodology ensures heterogeneity in returns coming from the direct assignment of capital gains to the Forbes 400 individuals and from the differences in returns across the private business wealth distribution in the imputation procedure.

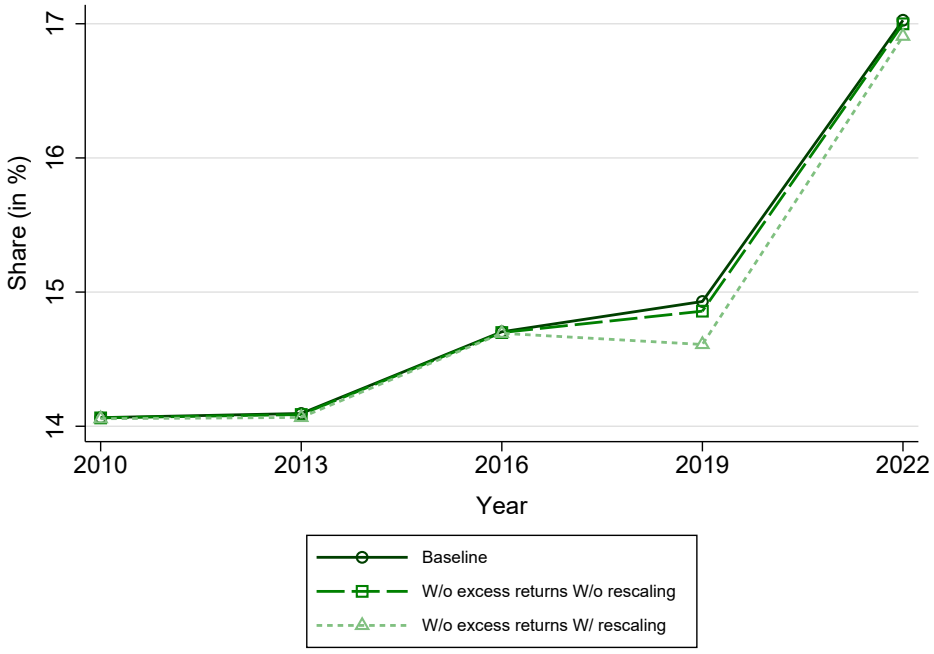
Finally, we rescale the realized private and counterfactual public capital gains we derive from Pitchbook to get a more accurate aggregate number for realized capital gains. For that, we use as a multiplier the gap we observed between the total excluded QSBS realized gains (152 billion USD) officially reported by the IRS (Abdulrauf et al., 2025), and the estimate of excluded QSBS gains (35 billion USD) we derive from Pitchbook. This implies an approximate rescaling of four times the Pitchbook capital gains. We run counterfactuals with and without rescaling capital gains for different top income groups, in particular, the top 10%, top 5%, top 1%, top 0.5%, the top 0.1%, and the Forbes 400.

Results. Given that income shares are slow-moving variables, the difference between the baseline top 0.5% pre-tax income and the counterfactual share excluding excess returns (first simulation) is not very large in absolute terms—less than 1 percentage point difference in 2019—even for the version using rescaled realized capital gains (Appendix Figure D6). However, the effects are larger when quantifying the contribution of the excess return channel for the overall growth of the pre-tax income share over the 2010-2022 and 2010-2019 period. Note that we focus on the growth over both the 2010-2022 and the 2010-2019 period, as 2022 was a particular year in which capital gain realizations in private capital markets were particularly low (Abdulrauf et al., 2025).

Appendix Table D9 compares the baseline growth rates of the top 0.5% pre-tax income share between both 2010 and 2022 and 2010 and 2019, with the growth rates of the top 0.5% pre-tax income share under the five different counterfactual scenarios described above. Column (2) shows that excess returns account for approximately 8% and 1% (36% and 4%) of the growth of the top 0.5% pre-tax income share between 2010-2022 and 2010-2019 without (after) rescaling capital gains, respectively. The effect is much lower between 2010-2019, as 2019 was a year with particularly low realizations. This finding is consistent with the null effect we find for 2019 in the state-level income inequality analysis of Section 4.3.2. Column (3) indicates that after-tax excess returns account for 6% and 0.6% (28% and 3%) of the growth of the top 0.5% pre-tax income share between 2010-2022 and 2010-2019 without (after) rescaling capital gains, respectively. The difference between the excess and after-tax excess returns counterfactual growth rates is quite similar, suggesting that the QSBS tax savings are not as important as the excess returns themselves. Indeed, column (4) reveals that tax savings account for 2% and 0.06% (8% and 0.8%) of the growth of the top 0.5% pre-tax income share between 2010-2022 and 2010-2019 without (after) rescaling capital gains, respectively. Columns (5) and (6) show that the tax savings effect is mainly coming from the additional tax effect incurred because of higher returns, rather than by the tax wedge effect per se. Our results are robust both in terms of direction and magnitude to the use of other public stock market indices, as well as the Barclay Hedge Fund and the FANG+ index (Appendix Tables D10 and D11). Taken together, the counterfactual simulations suggest that most of the contribution to pre-tax income inequality appears to be coming from excess returns and not from the QSBS tax exemption.

We further analyze whether the contribution of the excess return channel to the overall growth of pre-tax income over the 2010-2019 period is heterogeneous across the income distribution. We focus on the 2010-2019 period, as we have already shown in Table D9 that 2022 was abnormal year with very few realizations. Figure D7 shows that indeed the excess return channel is more important in explaining the growth in the pre-tax income share of billionaires, than that of the millionaires. In particular, while for the top 10-5% and the top 5-1% pre-tax income groups, excess returns account for approximately 2 to 3% (8% and 1%) of the overall growth of the their pre-tax income share over the 2010-2019 period without (after) rescaling realized capital gains, for the top 0.1%-0.01% and for the top 0.01% they account for 13 and 5% (60% and 20%), respectively. Our results are robust both in terms of direction and magnitude to the use of other public stock market indices, as well as the Barclay Hedge Fund and the FANG index (Appendix Tables D12 and D13).

Figure D6: Top 0.5% pre-tax income share: Baseline vs. Counterfactuals



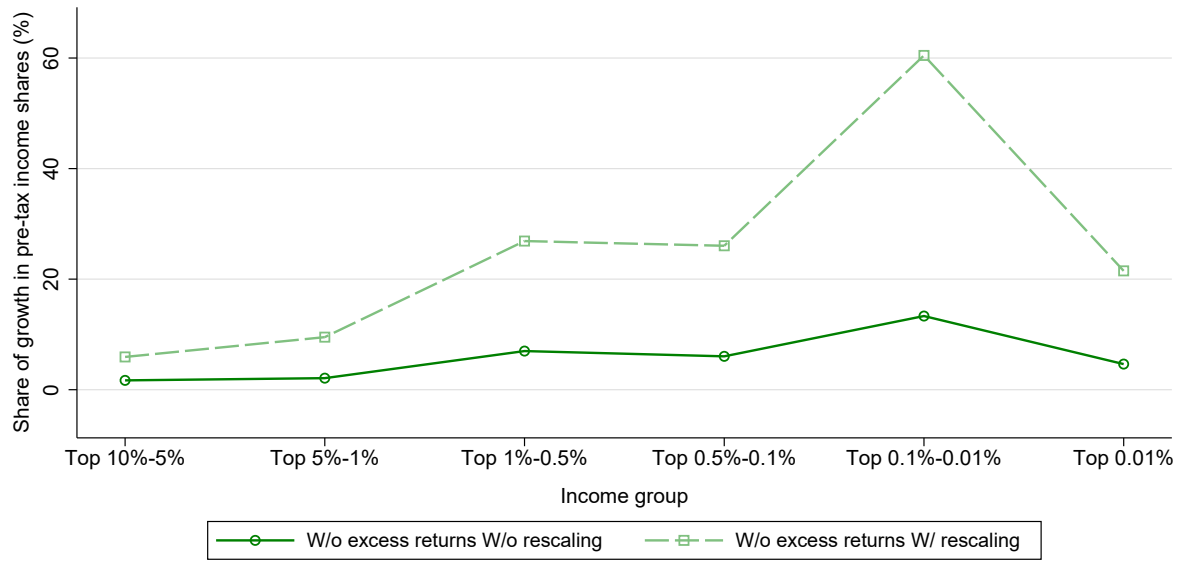
Source: SOI Income Stats, SCF, FA, Forbes, Bloomberg.
Notes: This figure compares the baseline top 0.5% pre-tax income share to the counterfactual top 0.5% pre-tax income shares excluding excess returns over the 2010-2022 period. We present the counterfactual income shares with and without rescaling realized capital gains. The counterfactual pre-tax income share is obtained by re-estimating the pre-tax income distribution after subtracting the pre-tax private capital gains and adding the counterfactual pre-tax gains had HNWIs invested this money in the NASDAQ 100.

Table D9: Counterfactuals: Growth of Top 0.5% Pre-Tax Income Share

	(1) Baseline	(2) W/o Excess Returns	(3) W/o After-Tax Excess Returns	(4) W/o Tax Savings	(5) W/o Tax Wedge	(6) W/o Additional Tax Effect
Panel A: W/o rescaling						
2010-2019						
Absolute growth	6.16%	5.66%	5.77%	6.05%	6.16%	6.04%
Share of growth	/	8.07%	6.25%	1.74%	0.12%	1.94%
2010-2022						
Absolute growth	21.06%	20.89%	20.93%	21.05%	21.06%	21.02%
Share of growth	/	0.82%	0.62%	0.06%	0.01%	0.20%
Panel B: W/ rescaling						
2010-2019						
Absolute growth	6.16%	3.94%	4.45%	5.65%	6.18%	5.62%
Share of growth	/	36.04%	27.70%	8.26%	0.35%	8.64%
2010-2022						
Absolute growth	21.13%	20.28%	20.46%	20.95%	21.16%	20.92%
Share of growth	/	3.99%	3.15%	0.84%	0.14%	0.98%

Sources: SOI Income Stats, Pitchbook, SCF, FA, Forbes 400, Bloomberg.
Notes: This table compares the baseline growth rate in the top 0.5% pre-tax income share between 2010 and 2022 and between 2010 and 2019, to the counterfactual growth rates in the top 0.5% pre-tax income share under five different counterfactual scenarios, which exclude: (1) excess returns, (2) after-tax excess returns, (3) overall tax savings, (4) tax savings arising from the tax wedge between QSBS long-term capital gains and non-QSBS long-term capital gains, and (5) tax savings arising from the additional amount of taxes payed because of excess returns. The table also reports the share of growth in the top 0.5% wealth share explained by the different counterfactuals. This share is derived as one minus the ratio between the counterfactual growth rate in the wealth share and the baseline growth rate in the wealth share. Panel A presents the counterfactual growth rates without rescaling realized capital gains, while Panel B presents the counterfactual growth rates after rescaling realized capital gains.

Figure D7: Heterogeneity in Share of Growth Explained by Excess Returns across Pre-Tax Top Income Groups (2010-2019)



Source: SCF, Pitchbook, FA, Forbes, Bloomberg.
Notes: This figure depicts how much of the 2010-2019 growth rate in the pre tax income of each of the income groups within the top 10% is accounted for by excess returns (excluding private capital gains and adding counterfactual public gains based on the NASDAQ 100 public stock market index) with and without rescaling capital gains. The share of growth for every income group is derived as one minus the ratio between the counterfactual growth rate in the income share without excess returns and the baseline growth rate in the income share. Note that whenever the growth rates are negative, we report the absolute value of one minus the ratio to ensure consistency.

Table D10: Income Inequality Counterfactuals: Non-Rescaled Excess Returns using Different Indices (2010-2019)

	(1) Baseline	(2) W/o Excess Returns	(3) W/o After-Tax Excess Returns	(4) W/o Tax Savings	(5) W/o Tax Wedge	(6) W/o Additional Tax Effect
			Panel A: S&P 500			
Absolute	6.16%	5.63%	5.75%	6.04%	6.16%	6.03%
Normalized	/	8.59%	6.64%	1.94%	0.12%	2.07%
			Panel B: RUT			
Absolute	6.16%	5.61%	5.73%	6.03%	6.16%	6.02%
Normalized	/	8.88%	6.86%	2.01%	0.12%	2.13%
			Panel C: BHFI			
Absolute	6.16%	5.59%	5.72%	6.03%	6.16%	6.02%
Normalized	/	9.25%	7.15%	2.10%	0.12%	2.22%
			Panel D: FANG			
Absolute	6.16%	5.71%	5.81%	6.06%	6.16%	6.05%
Normalized	/	7.20%	5.57%	1.61%	0.12%	1.73%

Sources: SOI Income Stats, Pitchbook, SCF, FA, Forbes 400, Bloomberg.
Notes: This table compares the baseline growth rate in the top 0.5% pre-tax income share between 2010 and 2022 and between 2010 and 2019, with the counterfactual growth rates (without rescaling realized capital gains0 in the top 0.5% pre-tax income share under five different counterfactual scenarios, which exclude: (1) excess returns, (2) after-tax excess returns, (3) overall tax savings, (4) tax savings arising from the tax wedge between QSBS long-term capital gains and non-QSBS long-term capital gains, and (5) tax savings arising from the additional amount of taxes payed because of excess returns. The table also reports the share of growth in the top 0.5% income share explained by the different counterfactuals. This share is derived as one minus the ratio between the counterfactual growth rate in the income share and the baseline growth rate in the income share.

Table D11: Income Inequality Counterfactuals: Rescaled Excess Returns using Different Indices (2010-2019)

	(1) Baseline	(2) W/o Excess Returns	(3) W/o After-Tax Excess Returns	(4) W/o Tax Savings	(5) W/o Tax Wedge	(6) W/o Additional Tax Effect
			Panel A: S&P 500			
Absolute	6.16%	3.80%	4.35%	5.61%	6.18%	5.59%
Normalized	/	38.22%	29.34%	8.80%	0.35%	9.16%
			Panel B: RUT			
Absolute	6.16%	3.73%	4.30%	5.60%	6.18%	5.58%
Normalized	/	39.40%	30.19%	9.07%	0.35%	9.42%
			Panel C: BHFI			
Absolute	6.16%	3.63%	4.22%	5.58%	6.18%	5.55%
Normalized	/	41.02%	31.48%	9.43%	0.35%	9.81%
			Panel D: FANG+			
Absolute	6.16%	4.16%	4.62%	5.70%	6.18%	5.68%
Normalized	/	32.37%	24.91%	7.41%	0.35%	7.76%

Sources: SOI Income Stats, Pitchbook, SCF, FA, Forbes 400, Bloomberg.
Notes: This table compares the baseline growth rate in the top 0.5% pre-tax income share between 2010 and 2022 and between 2010 and 2019, with the counterfactual growth rates (without rescaling realized capital gains0 in the top 0.5% pre-tax income share under five different counterfactual scenarios, which exclude: (1) excess returns, (2) after-tax excess returns, (3) overall tax savings, (4) tax savings arising from the tax wedge between QSBS long-term capital gains and non-QSBS long-term capital gains, and (5) tax savings arising from the additional amount of taxes payed because of excess returns. The table also reports the share of growth in the top 0.5% income share explained by the different counterfactuals. This share is derived as one minus the ratio between the counterfactual growth rate in the income share and the baseline growth rate in the income share.

Table D12: Counterfactuals: Growth of Non-Cumulative Top Pre-Tax Income Shares (2010-2019)

		W/o excess returns				
	Baseline	W/ NASDAQ	W/ S&P 500	W/ RUT	W/ BHFI	W/ FANG+
Panel A: W/o rescaling						
Top 10% to 5%	1.66%	1.63%	1.63%	1.62%	1.61%	1.65%
	/	1.52%	2.09%	2.36%	2.75%	0.42%
Top 5% to 1%	5.96%	6.09%	6.08%	6.08%	6.07%	6.10%
	/	2.17%	2.03%	1.95%	1.86%	2.39%
Top 1% to 0.5%	8.65%	8.05%	7.96%	7.92%	7.86%	8.21%
	/	6.97%	7.98%	8.48%	9.18%	5.10%
Top 0.5% to 0.1%	7.63%	7.17%	7.12%	7.09%	7.05%	7.26%
	/	6.03%	6.73%	7.12%	7.61%	4.84%
Top 0.1% to 0.01%	6.51%	5.64%	5.60%	5.57%	5.54%	5.72%
	/	13.37%	14.05%	14.45%	14.95%	12.20%
0.01%	4.00%	3.81%	3.82%	3.82%	3.83%	3.80%
	/	4.73%	4.56%	4.43%	4.33%	5.04%
Panel B: W/ rescaling						
Top 10% to 5%	1.79%	1.69%	1.65%	1.62%	1.60%	1.77%
	/	5.82%	8.09%	9.30%	10.68%	1.11%
Top 5% to 1%	6.06%	6.63%	6.62%	6.61%	6.60%	6.65%
	/	9.46%	9.23%	9.11%	8.97%	9.72%
Top 1% to 0.5%	9.49%	6.94%	6.57%	6.38%	6.14%	7.66%
	/	26.91%	30.76%	32.81%	35.30%	19.32%
Top 0.5% to 0.1%	7.86%	5.81%	5.59%	5.46%	5.29%	6.18%
	/	26.06%	28.93%	30.53%	32.64%	21.32%
Top 0.1% to 0.01%	6.47%	2.56%	2.37%	2.26%	2.13%	2.88%
	/	60.48%	63.37%	65.01%	67.13%	55.47%
0.01%	3.76%	2.95%	2.98%	3.00%	3.02%	2.90%
	/	21.57%	20.79%	20.13%	19.64%	22.89%

Source: SOI Income Stats, Survey of Consumer Finances, Pitchbook, FA, Forbes 400, Bloomberg, Benchmark Database.
Notes: This table compares the growth rates between the baseline non-cumulative U.S. top pre-tax income shares and the different counterfactuals excluding excess returns (without private capital gains but with any of the different indices (i.e., NASDAQ 100, S&P 500, Russell 2000, Barclay Hedge Fund, and FANG+ index)) between 2010 and 2019. The share of growth for every income group is derived as one minus the ratio between the counterfactual growth rate in the income share without excess returns and the baseline growth rate in the income share. Note that whenever the growth rates are negative, we report the absolute value of one minus the ratio to ensure consistency. Panel A reports the counterfactual growth rates without rescaling capital gains, while Panel B reports the counterfactual growth rates after rescaling capital gains.

Table D13: Counterfactuals: Growth of Cumulative Top Pre-Tax Income Shares (2010-2019)

		W/o excess returns				
	Baseline	W/ NASDAQ	W/ S&P 500	W/ RUT	W/ BHFI	W/ FANG+
Panel A: W/o rescaling						
Top 10%	5.17%	4.99%	4.96%	4.95%	4.94%	5.03%
	/	3.55%	3.99%	4.23%	4.55%	2.76%
Top 5%	6.35%	6.11%	6.09%	6.07%	6.05%	6.16%
	/	3.71%	4.14%	4.38%	4.69%	2.95%
Top 1%	6.65%	6.13%	6.09%	6.07%	6.04%	6.21%
	/	7.79%	8.43%	8.78%	9.24%	6.66%
Top 0.5%	6.16%	5.66%	5.63%	5.61%	5.59%	5.71%
	/	8.07%	8.59%	8.88%	9.25%	7.20%
Top 0.1%	5.23%	4.71%	4.69%	4.68%	4.66%	4.74%
	/	9.95%	10.29%	10.49%	10.76%	9.36%
Top 0.01%	4.00%	3.81%	3.82%	3.82%	3.83%	3.80%
	/	4.64%	4.47%	4.34%	4.24%	4.96%
Panel B: W/ rescaling						
Top 10%	5.30%	4.51%	4.42%	4.37%	4.31%	4.67%
	/	14.99%	16.70%	17.61%	18.79%	11.93%
Top 5%	6.48%	5.46%	5.35%	5.29%	5.22%	5.65%
	/	15.83%	17.48%	18.37%	19.52%	12.92%
Top 1%	6.82%	4.54%	4.36%	4.26%	4.13%	4.86%
	/	33.50%	36.14%	37.56%	39.42%	28.74%
Top 0.5%	6.16%	3.94%	3.80%	3.73%	3.63%	4.16%
	/	36.04%	38.22%	39.40%	41.02%	32.37%
Top 0.1%	5.09%	2.76%	2.68%	2.64%	2.58%	2.89%
	/	45.78%	47.29%	48.06%	49.20%	43.16%
Top 0.01%	3.76%	2.95%	2.98%	3.00%	3.02%	2.90%
	/	21.50%	20.72%	20.07%	19.58%	22.83%

Source: SOI Income Stats, Survey of Consumer Finances, Pitchbook, FA, Forbes 400, Bloomberg, Benchmark Database.
Notes: This table compares the growth rates between the baseline cumulative U.S. top pre-tax income shares and the different counterfactuals excluding excess returns (without private capital gains but with any of the different indices (i.e., NASDAQ 100, S&P 500, Russell 2000, Barclay Hedge Fund, and FANG+ index)) between 2010 and 2019. The share of growth for every income group is derived as one minus the ratio between the counterfactual growth rate in the income share without excess returns and the baseline growth rate in the income share. Note that whenever the growth rates are negative, we report the absolute value of one minus the ratio to ensure consistency. Panel A reports the counterfactual growth rates without rescaling capital gains, while Panel B reports the counterfactual growth rates after rescaling capital gains.