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Private Capital Markets and Inequality*

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Abstract

This paper examines how expanding private capital markets contribute to rising economic inequalities in the U.S. We show that the share of early-stage financing raised from U.S. high-net-worth individuals tripled from 2004 to 2022. Exploiting the expansion of the QSBS tax exclusion, we find that HNWIs' investments made startups 5.6% more likely to stay private. Counterfactual simulations reveal that HNWIs' excess returns on early-stage investments explain 26% of the growth in the top 0.5% wealth share over 2010-2022. Finally, investor entry increased incumbents' returns and encouraged further investments, generating a self-reinforcing feedback loop between private capital market growth and inequality.

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1 Introduction

Two important stylized facts have marked the last two 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, 2016, 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 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, 2026), then, all else equal, income and wealth inequality would increase. Second, insofar as HNWIs' investments provide private companies with a novel and valuable source of financing, these companies may choose to remain private for longer—if not indefinitely—further reinforcing the growth of private capital markets.

We focus on the U.S. for two main reasons. First, U.S. companies account for roughly 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 global financial crisis to incentivize HNWIs to invest in early-stage 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 companies to stay private and the dynamics of inequality in the U.S.[2] To rationalize the effects on inequality, we further calculate the returns that HNWIs earned on their early-stage investments. We compare these returns to those that they would have instead earned in public stock markets, which consist of similarly risky equities.

[1] By HNWIs, we refer to individuals who satisfy the 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) in each of the prior 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 startups." By these, we refer to investments in early-stage companies that PitchBook—our main data provider—categorizes as being in either the pre-seed, seed, early, or later stage of their growth as startups.

To carry out our analyses, we mainly use data on private capital market activity from PitchBook. This data includes information on the financing raised by private companies, the investors participating in each deal, and the changing valuation of each company across deals. For our analyses of economic inequalities, we complement PitchBook's data with distributional income and wealth statistics from the Internal Revenue Service's (IRS) Statistics of Income (SOI Tax Stats), the Survey of Consumer Finances (SCF), 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 was mainly driven by their investments in early-stage companies, which increased from $0.4 billion in 2004 to $15 billion in 2022, rather than by their investments in more mature companies. 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. early-stage companies from U.S. HNWIs tripled from 2% to 6% over the 2004-2022 period.[3] In the remainder of the paper, we therefore focus on early-stage investments, the private asset class that experienced the most growth in HNWIs' participation. We further document that the accumulated value of HNWIs' early-stage investments since 2004 reached $327 billion by 2022, which was almost double that of their counterfactual value had HNWIs instead invested in any of the NASDAQ 100, S&P 500, or Russell 2000 public stock market indices. HNWIs have thus earned excess returns on average on their early-stage investments relative to public stock markets, with this positive average driven by a highly right-skewed distribution.[4]

Second, we study the relationship between the increase in HNWIs' early-stage investments and the probability that early-stage 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 introduction in 1993, investors who held QSBS for at least five years could exclude 50% of their first $10 million

[3] Although HNWIs account for a relatively small share of the overall financing raised by early-stage companies, 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 calculate the rate of return on each company's equity using its valuation history, as recorded by PitchBook whenever the company raised new financing.

of the associated capital gains. However, in response to the 2008 global financial crisis, the U.S. federal government temporarily expanded the exclusion rate first to 75% in 2009 and then to 100% in 2010. This 100% exclusion was eventually made permanent in 2015. Exploiting company-level variation in QSBS eligibility in a difference-in-differences design, we find that the QSBS reforms increased the probability that QSBS-eligible companies raised financing from U.S. HNWIs by 2.7 percentage points—a 63% increase relative to the pre-reform probability of 4.3%. The reforms also increased the probability that QSBS-eligible companies remained active private companies by 5.2 percentage points—a 5.6% increase relative to the post-reform probability of the ineligible companies in the control group, 92.3% of which remained active private companies.

Third, we establish a link between the increase in HNWIs' early-stage investments and the rise in U.S. economic inequalities over the last two decades. We do so by further exploiting the QSBS reforms at the state level and by conducting counterfactual simulations at the national level. At the state level, we follow a two-step approach: we first estimate the effects of the QSBS reforms on resident HNWIs' early-stage investments, and we then estimate the implications of these investments for state-level income inequality. In both steps, we exploit variation across states in their 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 triggering regulation by the Securities and Exchange Commission, startups generally raise financing from individuals only if they are HNWIs (Jensen et al., 2017; Mikhail, 2022). Thus, the reforms should have increased HNWIs' investments more in states where the ex-ante number of resident HNWIs was higher. This setting has two main threats to identification. On the one hand, HNWIs may have settled in certain states specifically to get access to exclusive local investment opportunities (e.g., aspiring venture capitalists moving to California). On the other hand, the bias of resident HNWIs' early-stage investments toward local startups may have exposed them to economic shocks that varied across states. In particular, if the 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 of HNWIs residing in one state may have grown faster or slower relative to those of 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' early-stage investments in companies headquartered in their own state relative to the investments in those same companies by other 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 find that the expansion of the QSBS capital gains tax exclusion explained, on average, 12% of the overall growth in U.S. HNWIs' investments in U.S. early-stage companies between 2004-2008 and 2009-2022. In the second step, we rely on the state-level income distributions that we construct using the SOI Tax Stats and compare how the QSBS reforms affected the average income of the HNWIs in each state's top 0.5% relative to that of its bottom 99.5%. We find that the increase in HNWIs' early-stage investments widened the average income gap between the top 0.5% and bottom 99.5% by 1% of the pre-reform gap in the post-reform period. We further show that this effect was driven by the increase in the average realized capital gains income of the top 0.5%, and we provide direct evidence using PitchBook's data that this increase was explained, at least in part, by a sharp rise in their early-stage capital gains income. These results are consistent with HNWIs' excess returns on their early-stage investments relative to public stock markets contributing to rising income inequality.

We then use counterfactual simulations to quantify how much HNWIs' excess returns contributed to rising U.S. wealth inequality. We allocate HNWIs' returns on their early-stage investments based on PitchBook across the HNWIs in the SCF and the Forbes 400, rescaling them to match the total QSBS exclusions reported in tax filings. We then compare the resulting wealth distribution with a counterfactual in which HNWIs instead earned public stock market returns. We find that HNWIs' excess returns on their early-stage investments relative to the NASDAQ 100 account for 26% of the growth in the national top 0.5% wealth share between 2010 and 2022. We also find that these effects are stronger for billionaires than for millionaires.

Finally, we show that the relationship between HNWIs' increasing participation in private capital markets and rising economic inequalities generates a self-reinforcing feedback loop. To that end, we further exploit the QSBS reforms and show that new investor entry following the reforms raised the value of incumbents' investments and, in turn, induced them to invest even more in early-stage companies. We also document persistence in HNWIs' rankings within the distribution of their annual rates of return on their early-stage investments, suggesting that heterogeneity in skill may amplify the inequality in accumulated returns across early-stage investors and thereby reinforce the feedback loop.

This paper contributes to three main strands of the literature. First, we contribute to the vast 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 returns as important determinants of those dynamics (e.g., De Nardi, 2004; Jones, 2015; Saez and Zucman, 2016; De Nardi and Fella, 2017; Feiveson and Sabelhaus, 2018; Bach et al., 2020; Fagereng et al., 2020; Hubmer et al., 2020; Kuhn et al., 2020; Martínez-Toledano, 2020; Meeuwis, 2020; Cioffi, 2021; Xavier, 2021; Bauluz et al., 2022; Greenwald et al., 2022; Andersen et al., 2023; Blanchet and Martínez-Toledano, 2023; Nekoei and Seim, 2023; Gomez and Gouin-Bonenfant, 2024; Fagereng et al., 2025a;

Fagereng et al., 2025b; Gomez, 2025; Irie, 2025; Mian et al., 2025; Bauluz and Meyer, 2026). While confirming the importance of return heterogeneity, 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.

Second, 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. Existing theoretical and empirical studies on return heterogeneity have emphasized the role of entrepreneurial ability (Lucas, 1978), information (Peress, 2004), sophistication (Kacperczyk et al., 2019), and various asset class-specific factors (e.g., Calvet and Fisher, 2007; Campbell et al., 2019; Deuflhard et al., 2019; Bach et al., 2020; Fagereng et al., 2020; Xavier, 2021; Bretscher et al., 2025). Our paper is most closely related to earlier and contemporaneous studies examining differences in returns between private and public companies (Moskowitz and Vissing-Jørgensen, 2002; Kartashova, 2014; Brown and Kaplan, 2019; Brown et al., 2021; Balloch et al., 2026; Balloch and Richers, 2026), most of which have focused on documenting either the relative performance of private equity funds or overall private business wealth. In contrast, we document the outperformance of HNWIs' early-stage investments relative to public stock markets.

Finally, we contribute to the growing literature on investors in private capital markets. Most existing studies have focused on institutional investors such as pension funds and endowment plans (Lerner and Schoar, 2004; Lerner et al., 2007; Sørensen, 2007; Robinson and Sensoy, 2013; Mittal, 2022; Maurin et al., 2023). Building on the recent literature's growing interest in angel investors (Bach et al., 2022; Canipek, 2024; Lindsey and Stein, 2025; Karlsen et al., 2026), 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; Azevedo et al., 2025; Campello and Junqueira, 2025; Chen and Farre-Mensa, 2026; Fairlie et al., 2026), we are, to the best of our knowledge, the first to do so with the aim of understanding the implications of HNWIs' increasing participation in private capital markets for income and wealth inequality.

The rest of this paper is organized as follows. Section 2 describes the main datasets that we use. Section 3 documents key stylized facts. Section 4 analyzes the effects of the QSBS reforms on HNWIs' early-stage investments, companies' decisions to stay private, and state-level income inequality. Section 5 quantifies the implications of HNWIs' excess returns for U.S. wealth inequality by means of counterfactual simulations. Section 6 documents the existence of a feedback loop between HNWIs' growing participation in private capital markets and rising economic inequalities. Section 7 concludes.

2 Data

This section describes the main datasets that we use in our analyses and our procedures to validate their coverage and quality. Section 2.1 details how we use the data from PitchBook on private capital market activity to identify high-net-worth individuals' investments in private companies and calculate the returns that they earned on them. Section 2.2 describes how we combine data from multiple sources to identify the number of HNWIs residing in each U.S. state and to construct income and wealth distribution series.

2.1 Private Capital Market Activity

PitchBook. Our main source for data on private capital market activity is PitchBook, a commercial data provider that collects information on the financing raised by companies and the investors participating in each deal. 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 primary 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' investments in private capital market deals, including both investments directly into companies and those intermediated by private investment funds on behalf of their limited partners. 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, and all other investors).[7] Since we observe the amounts invested by only some of the investors in each deal, we distribute the remainder of the deal equally across all the other investors. Appendix A contains further details

[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 "venture capital" usually refers only to intermediated investments in startups, whereas "early-stage" refers to both direct and intermediated investments.

[7] PitchBook's data does not allow us to attribute pension funds' investments to their pensioners. However, this should not lead to too much undercounting of HNWIs' investments, since pension wealth makes up only a small share of the wealth of the wealthy (see Appendix Figure A2), while pension funds allocate only a small share of their assets under management to private capital markets (see https://equable.org/pension-funding-trends-2023).

about our procedure to clean PitchBook's data on private capital market activity.[8]

Second, we use PitchBook's data on companies' valuations to calculate the returns that investors earned on their early-stage investments.[9] Although returns are often calculated at the fund level (Korteweg and Nagel, 2025), we instead calculate returns at the investment level, given that 82% of U.S. HNWIs' early-stage investments in U.S. companies from 2004 to 2022 were direct rather than intermediated (see Appendix Table A2). To calculate investment-level returns, we take an approach similar to that of Korteweg and Sorensen (2010), using information on the changing valuation of each company across deals to calculate the rate of return on its equity. Specifically, we compare the company's new pre-money valuation (before accounting for the financing that it raised as part of its new deal) to its previous post-money valuation (after accounting for the 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 financing and that due to organic growth. Following this methodology, we first construct the history of the rate of return on each company's equity. We then calculate the return on each investment in each year, allowing the value of the investment to evolve over time according to the returns on the company.[10] Since we do not observe each company's valuation as part of each deal, 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 the valuations that we observe. Appendix B further details this imputation procedure, as well as other aspects of our procedure to clean PitchBook's data on valuations and our return methodology.

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[8] 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 data 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 HNWIs' investments, which are 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, Thomson Reuters VentureXpert, and Dow Jones VentureSource) used by Denes et al. (2023), based on a definition of angel investments similar to theirs (i.e., early-stage investments made as part of capitalization, crowdfunding, angel, accelerator/incubator, and seed deals or by individuals, angel groups, and accelerators/incubators). For the 1988-2018 period that Denes et al. (2023) study, they identify 206,885 angel investments in their data, while we identify 249,898 in ours.

[9] Reassuringly, the evolution of private business wealth in the U.S. based on PitchBook's data on companies' valuations is broadly in line with its evolution based on alternative sources (see Appendix Figure B1).

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

Finally, we use the data from PitchBook to distinguish between eligible and ineligible issuers of qualified small business stock, thereby making it possible to evaluate the effects of the QSBS reforms on HNWIs' 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 total financing raised. 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. Appendix C contains further details about our procedure to determine QSBS eligibility.

2.2 High-Net-Worth Individuals and Inequality

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

Suss et al. (2024) define HNWIs to resemble the Securities and Exchange Commission's definition of accredited investors.[11] According to the SEC, accredited investors are the only individuals (other than investment professionals) sophisticated enough to participate in private capital markets. Private investment funds and companies that raise financing from non-accredited investors must therefore register their securities with the SEC.[12] Thus, we assume that all the HNWIs who invested in the private capital market deals in PitchBook were accredited investors. In Appendix D.1, we validate the estimated numbers of U.S. accredited investors from the GEOWEALTH-US by comparing them to alternative estimates from the Phoenix Marketing International/MarketCast Wealth and Affluent Monitor, the Forbes 400 rich lists, the Credit Suisse/UBS Global Wealth Report, and the Survey of Consumer Finances. Our baseline measure based on the GEOWEALTH-US is

[11] For the precise definition, see footnote 1. Suss et al. (2024) estimate the wealth of individuals sampled in cross-sectional rather than longitudinal population surveys. As a result, for their income test, they consider only whether an individual's observed household income exceeded $300,000 in a given year (but not necessarily the prior two years). For their wealth test, they consider whether the individual's estimated household wealth (excluding the estimated value of their primary residence) exceeded $1 million.

[12] Specifically, private funds and companies can raise financing from up to 35 non-accredited investors before triggering SEC regulations (see https://www.sec.gov/resources-small-businesses/exempt-offerings/private-placements-rule-506b). In practice, this threshold 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 is when companies raise financing via crowdfunding (Jensen et al., 2017).

consistent with these alternative sources, both across states and over time.

Statistics of Income. To conduct our state-level inequality analyses, we build state-level income inequality series based on the personal income tax statistics from the IRS's Statistics of Income (SOI Tax Stats). We use the historical data tables that provide information on a range of personal income tax items, aggregated by state and adjusted gross income (AGI) bracket from 2004 to 2022. AGI refers to income from all sources—including labor, investment, business, and retirement income—adjusted for tax deductions.

We apply the generalized Pareto interpolation (GPI) method developed by Blanchet et al. (2022). This non-parametric approach avoids the assumptions of the Pareto approximation, which are often violated by empirical data. For each state in each year, we construct the state-level income distribution across individuals using data on tax-filing units, assuming that the reported household income of couples filing jointly was shared equally between spouses. Our constructed series are consistent with those of Sommeiller and Price (2018), who build their own series based on the same personal income tax tabulations (see Appendix Figure D2). We decompose total income along the income distribution into realized capital gains and other income (i.e., labor, dividend, interest, and other investment income) based on the information available on the income composition for each AGI bracket. We further aggregate our state-level income inequality series to build nationwide pre- and post-tax income distributions, which we use to implement the counterfactual simulations of U.S. income inequality described in Appendix D.3.1. Appendix D.2.1 further details our methodology to construct the income inequality series.

Survey of Consumer Finances. We similarly construct a series for the nationwide wealth distribution to implement the counterfactual simulations of wealth inequality in Section 5. 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 survey is updated every three years and is available from 1989 to 2022. We construct the wealth distribution for every wave of the survey from 2004 to 2022 based on a measure of net wealth—that is, the sum of private business wealth, public equity, interest-earning assets, real estate, and other financial and non-financial assets, minus all liabilities.

The SCF oversamples individuals at the top of the wealth distribution, enabling a more accurate measurement of the wealth of the wealthiest individuals. We further improve its coverage of the very top of the wealth distribution by following the Distributional Financial Accounts (DFA) methodology developed by Batty et al. (2022), which combines the SCF with the Financial Accounts of the U.S. and the Forbes 400 rich lists. Appendix D.2.2 further details our methodology to construct the wealth distribution series.

To complement our analysis of high-net-worth individuals' returns based on the data from

PitchBook, we also use the SCF to calculate U.S. households' returns on both private and public equity from 2004 to 2022, based on the methodology developed by Moskowitz and Vissing-Jørgensen (2002) and Kartashova (2014).

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 relative 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.[13] 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, with the amount of financing raised totaling $1 trillion by 2022. The post-crisis period was marked by the faster growth of early-stage financing relative to that of private equity, private debt, and real asset financing. 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 (e.g., Ma and Zimmermann, 2023; Abreu et al., 2025).[14]

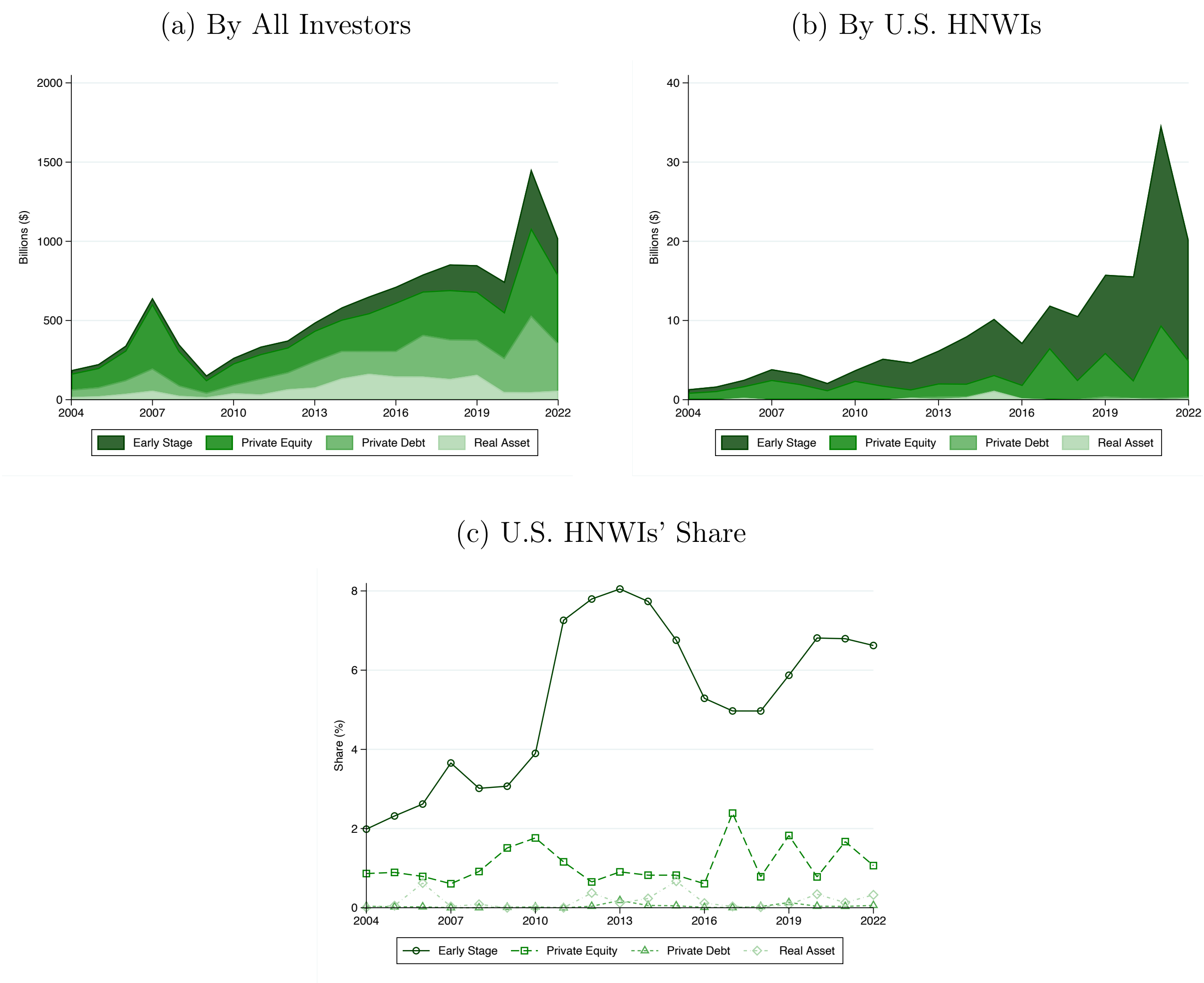
The post-crisis growth in early-stage financing was partly driven by the increase in high-net-worth individuals' early-stage investments. Figure 1b shows that the total amount invested annually by U.S. HNWIs in U.S. startups was stable around $0.4-1.4 billion from 2004 to 2008. However, this amount grew to $25 billion by 2021, before falling to $15 billion in 2022 amid the broader decline in U.S. private capital market activity (see Figure 1a). HNWIs' private equity, private debt, and real asset investments only moderately increased over the same period.[15] Thus, early-stage investments were the fastest-growing

[13] We focus on the U.S. because U.S. companies have consistently accounted for roughly half of the financing raised in global private capital markets (see Appendix Figure A3).

[14] 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 A4 shows that investments in U.S. private capital market deals as a share of U.S. gross domestic product increased from 1.5% in 2004 to 3.8% in 2022.

[15] Appendix Figure A5 shows that the early-stage share increased even among HNWIs' intermediated

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



Sources: 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 all investors' investments. Panel (b) plots U.S. high-net-worth individuals' investments. Panel (c) plots U.S. HNWIs' share of all investors' investments. The values in Panels (a) and (b) are expressed in nominal terms. Panels (b) and (c) exclude 5 private equity investments made by U.S. HNWIs that we consider outliers, as the amounts invested exceeded $1 billion.

private asset class in HNWIs' portfolios. The increase in HNWIs' early-stage investments was driven primarily by the entry of new investors (Appendix Figure A6a), who accounted for the vast majority of these investments after 2008 (Appendix Figure A6b).[16] Despite this sharp increase in HNWIs' participation, the sectoral composition of their early-stage investments remained remarkably stable throughout the period (Appendix Figure A8).

investments. Yet, it did not overtake the private equity share, consistent with Balloch and Richers (2026).

[16] To ensure that the increase in HNWIs' early-stage investments that we document was not merely driven by PitchBook's improving coverage over time, we corroborate this trend using the Survey of Consumer Finances (see Appendix Figure A7). Appendix Figure A6b further shows that there was an increase in the early-stage investments of HNWIs who first invested in or before 2008, and not just in the early-stage investments of new investors whose first investment recorded by PitchBook was after 2008.

The increase in HNWIs' early-stage investments outpaced the overall growth in early-stage financing. Figure 1c shows that U.S. HNWIs accounted for only 2% of the total amount of financing raised by U.S. early-stage 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 an 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 portfolios of the wealthy (Appendix Figure A2). Thus, a sharp increase in the private business wealth of the wealthy could significantly contribute to a rise in economic inequalities if it were to consistently yield higher returns than other more widely held forms of wealth. In Sections 4 and 5, we formally explore the link between HNWIs' increasing participation in private capital markets and the rise in U.S. economic inequalities between 2004 and 2022. We hereafter focus on early-stage investments, since this is the private asset class in which HNWIs' participation grew the most.

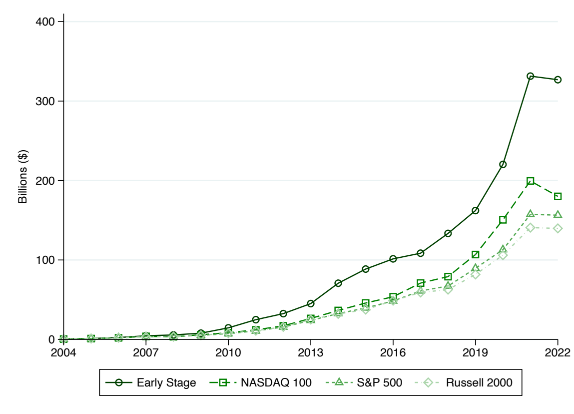
3.2 HNWIs' Excess Returns on Early-Stage Investments

We next characterize the returns that high-net-worth individuals 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, we calculate investment-level returns using PitchBook's data on companies' valuations. In our baseline analyses, we consider directly observed valuations and bankruptcies, as well as imputed valuations for companies with missing valuation data. We impute missing valuations as the predicted values from sector-specific regressions of the log valuations of each company on company and interacted company stage-year fixed effects. Since better-performing companies are more likely to raise financing—which is necessary but not sufficient for PitchBook to update their recorded valuations—we treat companies that raised financing and those that did not as two separate cases. In the first case, we estimate the imputation regressions only on observed valuations, as the valuations of companies that raised financing are more likely to reflect those of other companies that also raised financing. In the second case, we instead estimate the imputation regressions only on valuations obtained from the dynamic selection model developed by Korteweg and Sorensen (2010), as the valuations of companies that did not raise financing are more likely to reflect those of other companies that also failed to do so. This approach allows us to calculate the annual return on each early-stage investment while also accounting for selection bias. Appendix Section B.1.2 further details our imputation procedure.

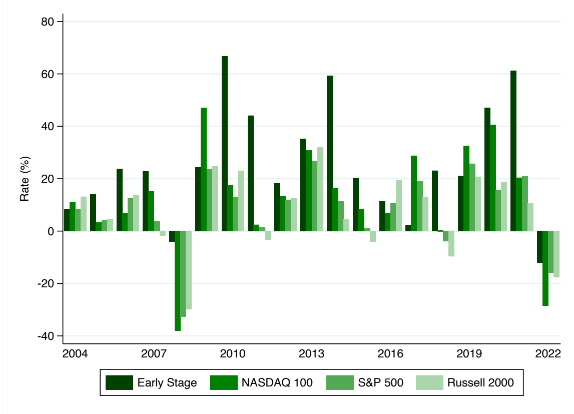
Figure 2a plots the accumulated value of U.S. HNWIs' early-stage investments in U.S. companies from 2004 to 2022 (i.e., the sum of their accumulated amounts invested and accumulated returns) by the end of each year. It also plots the accumulated value of their

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

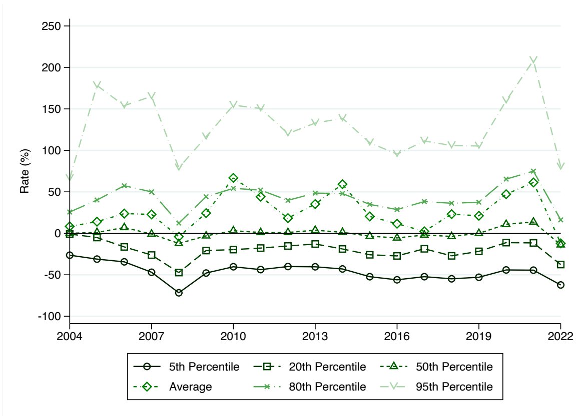
(a) Accumulated Value by End of Year



(b) Average Annual Rate of Return



(c) Distribution of Annual Rates of Return



Sources: PitchBook, Capital IQ.
Notes: This figure describes the returns that U.S. high-net-worth individuals earned on their early-stage investments in U.S. companies from 2004 to 2022, comparing them to the counterfactual returns that they would have earned had they instead invested in the total return version of any of the NASDAQ 100, S&P 500, or Russell 2000 public stock market indices. Panel (a) plots the accumulated value of HNWIs' investments by the end of each year. Panel (b) plots the average annual rate of return on these investments, defined as the ratio of the total returns on the investments during each year to their total accumulated value as of the start of the year. Panel (c) plots various percentiles of the distribution of annual rates of return across HNWIs with early-stage investments. The values in Panel (a) are expressed in nominal terms. The values in Panels (b) and (c) are based on only the investments that, as of the start of each year, had already been entered but had not yet been exited. The average rate of return in Panel (c) is the same as in Panel (b)—that is, an implicitly weighted average based on each investment's accumulated value as of the start of each year, rather than a simple average.

counterfactual investments in the total return versions of three public stock market indices: the NASDAQ 100, S&P 500, and Russell 2000. By the end of 2022, the accumulated value of HNWIs' early-stage investments was $327 billion, almost double that of their counterfactual investments in any of the public stock market indices. Thus, HNWIs earned excess returns on their early-stage investments relative to public stock markets.

We next verify the robustness of these excess returns. First, we show that they are robust to an alternative valuation sample based on only the observed valuations and bankruptcies of companies with at least two observed valuations (Appendix Figure B3a). Reassuringly, such companies explain most of the accumulated value of HNWIs' early-stage investments (Appendix Figure B3b), so that our imputation procedure's main effect is simply to account for dynamic selection by adjusting these particular companies' valuations downward. Second, the excess returns are robust to alternative assumptions regarding our imputation procedure, including more optimistic and pessimistic variants of the procedure (Appendix Figure B4a), the imputation of bankruptcies for companies that appear inactive (Appendix Figure B4b), and the application of haircuts to "unicorn" valuations (Appendix Figure B4c) that exceed $1 billion but are likely overvalued (Gornall and Strebulaev, 2020; Gahng, 2023). Finally, these excess returns are also robust to alternative counterfactuals, such as the FANG+ index of high-growth technology companies and the Barclay Hedge Fund Index (Appendix Figure B5).

To explain the divergence between the accumulated value of HNWIs' early-stage investments and those of their counterfactual investments, Figure 2b compares their average annual rates of return. We calculate these average annual rates as the ratio of the total returns on the investments during each year to their total accumulated value as of the start of the year.[17] In 14 of the 19 years from 2004 to 2022, HNWIs earned a higher rate of return on average on their early-stage investments than they would have earned on their counterfactual investments in any of the NASDAQ 100, S&P 500, or Russell 2000 public stock market indices.[18] Consistent with this result, Appendix Figure B8 shows that the rates of return on total private business equity exceeded those on public equity from 2004 to 2022, based on the methodology using the Survey of Consumer Finances developed by Moskowitz and Vissing-Jørgensen (2002) and Kartashova (2014).

These average returns mask substantial return heterogeneity across HNWIs. Figure 2c plots the 5th, 20th, 50th, 80th, and 95th percentiles of the distribution of annual rates of return across HNWIs with early-stage investments. Whereas the investor at or below the 20th percentile consistently lost money, the investor at the 95th percentile often saw the value of their early-stage investments more than double, suggesting that the distribution of returns was highly right-skewed. Moreover, the median investor earned close to zero returns, consistent with the findings of Stanley and Øvrum (2023) and Karlsen et al.

[17] Our baseline measure equals the total value to paid-in capital ratio across investments over each 1-year horizon, minus one. Since this is not an annualized rate, we also follow Phalippou (2025) to calculate the internal rate of return (IRR) across investments over the same 1-year horizon. Appendix Figure B7 shows that the IRR (which is annualized) simply amplifies any differences between HNWIs' average annual rates of return on their early-stage investments and those on their counterfactual investments.

[18] Our calculated returns are not adjusted for fees. However, this should not affect the excess returns, as intermediated investments account for only 5% of HNWIs' amounts invested (see Appendix Table A2).

(2026) that the median early-stage investment tends to yield zero returns.[19] Appendix Figures B10 and B11 document additional return heterogeneities across investment and investor types. They show that HNWIs earned higher returns on average on their direct investments than on their intermediated ones (Appendix Figure B10a) and that their direct investments outperformed those of institutional investors (Appendix Figure B11b).

HNWIs' excess returns on their early-stage investments relative to public stock markets do not necessarily reflect outperformance on a risk-adjusted basis, as they may simply reflect compensation for systematic risk. To better understand the source of these excess returns, we estimate an extended version of the dynamic selection model developed by Korteweg and Sorensen (2010) that incorporates portfolio weights. These weights account for the different amounts invested by different investor types in each company.[20] We estimate the valuation equation in the model at the monthly level from 2004 to 2022, regressing changes in companies' log valuations in excess of the log risk-free rate of return on the excess log return of the market. In addition to this valuation equation, the model features a selection equation that accounts for the fact that better-performing companies are more likely to raise financing and therefore report the valuations that we observe. Appendix Section B.1.2 further details this dynamic selection model.

We first replicate Korteweg and Sorensen (2010)'s unweighted estimates for the 1987-2005 period using the data from PitchBook. We obtain similar estimates for β, which measures riskiness relative to the market, and α, which captures performance relative to the market after accounting for risk (see Appendix Tables B2 and B3). We then turn to our weighted estimates for the 2004-2022 period in Table 1. Column (1) reports our estimates of β and α for U.S. investors' early-stage investments in U.S. companies (see Appendix Table B4 for all the estimated parameters). Our estimated coefficient on the excess return of the market is $\beta \approx 1.6$ (i.e., $\beta > 1$), confirming that early-stage investments were on average riskier than public stock markets. However, our estimate of the weighted monthly risk-adjusted excess return is $\alpha \approx 7.8\%$ (i.e., $\alpha > 0$), indicating that these investments yielded excess returns even after accounting for risk.[21] Columns (2) and (3) then compare U.S. HNWIs' risk-adjusted returns to those of U.S. institutional investors, showing that HNWIs had a higher α. Thus, on average, HNWIs invested in better-performing companies.[22] This

[19] We also find that the median investment yielded close to zero returns (see Appendix Figure B9). The dispersion of the investment-level distribution moderately exceeded that of the investor-level distribution, suggesting that investing in multiple early-stage companies helped HNWIs hedge investment-specific risks.

[20] Specifically, we weight each U.S. company's valuations by its share of each investor type's total amount invested in U.S. early-stage companies from 2004 to 2022.

[21] Our weighted estimates of α notably exceed Korteweg and Sorensen (2010)'s unweighted estimates, consistent with the best-performing companies raising the most total financing across multiple deals.

[22] HNWIs' greater α than that of institutional investors is consistent with Korteweg and Sorensen (2010)'s finding that investments in seed deals have a higher α than other early-stage investments (see the authors'

Table 1: Risk-Adjusted Returns Based on the Dynamic Selection Model (2004-2022)

	Investor Type		
	All U.S. Investors	U.S. HNWIs	U.S. Institutions
Parameter	(1)	(2)	(3)
β	1.5585***	1.4050***	1.5661***
	(0.0554)	(0.0826)	(0.0819)
α	0.0779***	0.0864***	0.0778***
	(0.0012)	(0.0012)	(0.0015)
No. observations	67,705	41,778	66,601
No. companies	20,729	12,272	20,315

Sources: PitchBook; https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.
Notes: This table reports our estimates of the key parameters of our portfolio-weighted version of the dynamic selection model developed by Korteweg and Sorensen (2010) and jointly characterized by Equations (B1) and (B2). We estimate the model at the monthly level from 2004 to 2022 separately for each of three different investor types: all U.S. investors, U.S. high-net-worth individuals, and U.S. institutional investors. β refers to the coefficient on the excess log return of the market in the valuation equation. $\alpha = \delta + 0.5\sigma^2 - 0.5\beta(1-\beta)\sigma_m^2$ refers to the monthly risk-adjusted excess return, where δ is the intercept of the valuation equation, σ is the standard deviation of the error term in the valuation equation, and σ_m is the standard deviation of the excess log return on the market. Observations refer to the observed valuations and bankruptcies of U.S. companies for which we observe valuations in at least two different months from 2004 to 2022. The regression weight on each observation reflects each company's share of each investor type's total amount invested in early-stage companies from 2004 to 2022, divided by the number of valuations for that company. The estimate of each parameter and its standard deviation are based on its mean and variance across 200 iterations of the Bayesian Gibbs sampling procedure developed by Korteweg and Sorensen (2010), after we discard 1,000 initial burn-in iterations. ***, **, and * denote statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

result is robust to including additional early-stage, size, and book-to-market factors in the dynamic selection model (Appendix Table B5) and to estimating the model only for the period after 2008 (Appendix Table B6), during which HNWIs' early-stage investments increased sharply (Figure 1b). In Section 6, we further explore potential sources—including differences in investment timing or skill—of the positive α among HNWIs.

4 Qualified Small Business Stock Reforms

This section relies on the post-2008 expansion of the U.S. federal capital gains tax exclusion on qualified small business stock as a quasi-exogenous shock to high-net-worth individuals'

Tables 5 and 8). The reason is that HNWIs allocate more of their early-stage investments to pre-seed and seed deals than institutional investors do (see Appendix Tables A2 and A3).

participation in private capital markets. Since these reforms offered HNWIs tax incentives to invest in early-stage companies, they provide a unique opportunity to evaluate the reforms' effects on HNWIs' early-stage investments and, thereby, the implications of these investments both for the companies raising financing and for economic inequalities. Section 4.1 describes the institutional setting of the QSBS reforms. Section 4.2 analyzes the reforms' effects on HNWIs' early-stage investments and on the companies in which they invested, exploiting variation in the exposure to the reforms across companies (i.e., in their eligibility to issue QSBS). Section 4.3 explores the implications of HNWIs' increased participation in private capital markets for state-level income inequality, exploiting variation in the exposure to the reforms across states (i.e., in the number of HNWIs residing in them).

4.1 QSBS Capital Gains Tax Exclusion

The U.S. federal government first introduced the capital gains tax exclusion on qualified small business stock in 1993. Set forth in Section 1202 of the Internal Revenue Code, it is a personal income tax exclusion on capital gains from the sale of QSBS. The maximum gain eligible for the exclusion is generally limited to $10 million per issuer.[23] For an investor to qualify 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 satisfy the following three conditions: (1) it must be a U.S. C corporation (and therefore be taxed separately from its owners); (2) at least 80% of the company's assets must be used in the active conduct of a qualified trade or business;[24] and (3) it must have had gross assets of $50 million or less at all times before and immediately after the QSBS was issued. Companies satisfying these requirements tend to be startups in high-growth sectors (e.g., information technology) that are attractive to early-stage investors (Polsky and Yale, 2023).[25]

Although the QSBS capital gains tax exclusion had been in place since 1993, it was only after the 2008 global financial crisis that it became attractive from a tax-saving

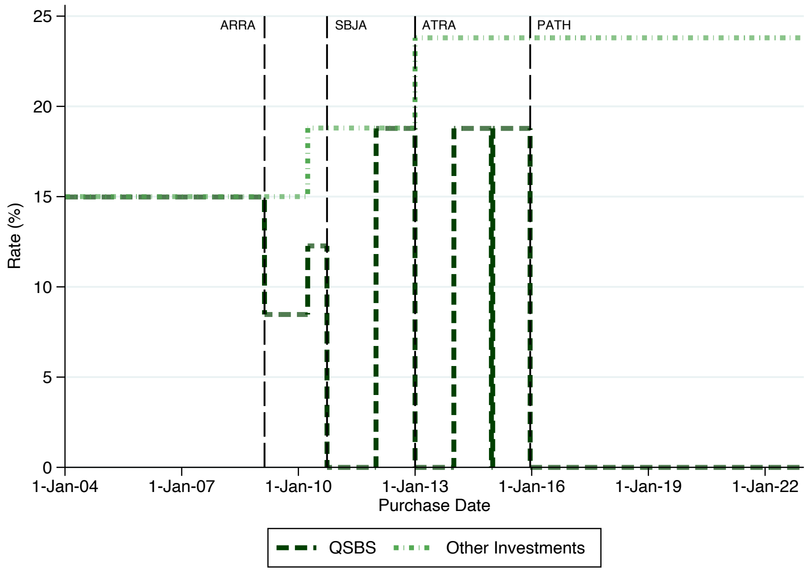
[23] More precisely, the maximum eligible gain per issuer per year is limited to the greater of (1) $10 million minus the gains previously excluded for that issuer and (2) 10 times the basis of the QSBS from that issuer sold in that year. The cumulative limit of $10 million per issuer is likely to be the relevant one in practice, since the amounts invested by early-stage investors are often small relative to their gains.

[24] Disqualified trades and businesses are determined by the Internal Revenue Service but, by statute, 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 (e.g., if the company 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 (i.e., a type of tax deduction) can be claimed, (4) operate a hotel, motel, restaurant, or similar business, or (5) are farming businesses.

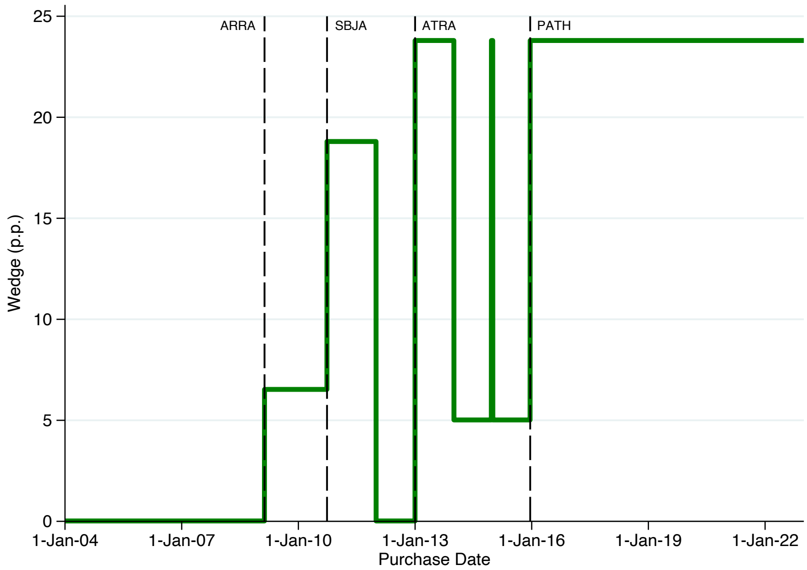
[25] The QSBS capital gains tax exclusion also varies across states due to differences in their state-level long-term capital gains tax rates, as well as in their decisions to adopt the federal tax rules on the exclusion as part of their own state-level tax rules. States either (1) fully conform with the federal tax rules and apply the same exclusion rate, (2) partially conform and apply a different rate than the federal rate, or (3) do not conform at all and fully tax QSBS capital gains at the state level (e.g., California).

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



Sources: Polsky and Yale (2023).
Notes: This figure compares the evolution of the expected federal tax rate on QSBS capital gains from 2004 to 2022 with that of the expected federal long-term capital gains tax rate on other investments. Panel (a) plots the evolution of the two expected rates (i.e., on QSBS and other investments) separately. Panel (b) plots the difference between them (i.e., the expected tax wedge, measured in percentage points). The values are the rates to which individuals expected to be subject as of their purchase date, even though the actual rates may have changed ex-post due to changes in the long-term capital gains tax rates on other investments. The vertical dashed lines indicate the different Acts that changed QSBS legislation: the American Recovery and Reinvestment Act (ARRA), the Small Business Jobs Act (SBJA), the American Taxpayer Relief Act (ATRA), and the Protecting Americans from Tax Hikes Act (PATH).

perspective.[26] Figure 3a shows that, from 2004 to 2008, 50% of the first $10 million in capital gains 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 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 introduction in 1993).[27] However, since the federal long-term capital gains tax rate on gains from other investments was only 15%, the expected federal tax wedge on QSBS capital gains—measured as the difference 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 or no incentive to favor QSBS over other investments.

With the onset of the global financial crisis in 2008, the U.S. federal government expanded the QSBS capital gains tax exclusion to support early-stage companies in raising financing. 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 to 100% until the end of 2011. These temporary expansions repeatedly expired before being retroactively extended, for example by the American Taxpayer Relief Act (ATRA) in 2013. This cycle of expirations and extensions continued until 2015, when the Protecting Americans from Tax Hikes Act (PATH) made the 100% exclusion rate permanent. Thus, the 100% exclusion rate has effectively been in place since 2010.[28] Given that the federal long-term capital gains tax rate has ranged from 15% to 24% since 2008, the expanded QSBS capital gains tax exclusion has made investments in early-stage companies considerably more attractive to U.S. high-net-worth individuals relative to other investments.

4.2 HNWIs' Early-Stage Investments and Companies

4.2.1 Effects on Investments: Company Level

We first study whether the expanded federal tax exclusion on QSBS capital gains increased high-net-worth individuals' early-stage investments in eligible issuers of QSBS. We consider

[26] Appendix Figure C1 plots the complete history of the federal tax exclusion on QSBS capital gains all the way back to its 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.

[27] We refer to *expected* tax rates since claiming the tax exclusion requires at least a five-year gap between the investment's entry and exit dates, 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's entry date. As a result, the tax wedge on QSBS capital gains can either shrink or expand after the investment decision has already been made.

[28] In 2025, the One Big Beautiful Bill Act further expanded the QSBS capital gains tax exclusion by (1) decreasing the required holding period from five to three years (with the exclusion rate set at 50% for three years and 75% for four years), (2) increasing the maximum gross assets of an eligible issuer from $50 million to $75 million, (3) increasing the maximum gain eligible from $10 million to $15 million, and (4) introducing an inflation adjustment so that both of these thresholds grow with inflation over time.

the sample of 14,224 U.S. companies that, as of the end of 2008 (i.e., the year before the first QSBS reform), had already been founded but had never gone bankrupt, become publicly listed, or been acquired. We construct an unbalanced panel from 2004 to 2022, including each company from its founding to its first bankruptcy, public offering, or acquisition. We distinguish between treated and control companies based on their QSBS eligibility. Treated companies are those that, as of the end of 2008, satisfied all three conditions of QSBS eligibility: (1) they were U.S. 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.[29] The controls are all the U.S. active private companies that failed to satisfy at least one of these conditions. Appendix Table C1 summarizes these companies' characteristics as of the end of 2008. Relative to ineligible issuers, eligible issuers of QSBS were more likely to be young, be technology companies, and raise early-stage financing from HNWIs. The inclusion as controls of companies that satisfied some but not all the eligibility conditions therefore helps address any potential imbalance.

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:

$$Y_{i,t} = \sum_{j=2004, j\neq 2008}^{2022} \beta_j \times \mathbb{1}\{t=j\} \times \text{QSBS}_i + \alpha_i + \gamma_{C(i),t} + \delta_{Q(i),t} + \zeta_{M(i),t} + u_{i,t}, \quad (1)$$

where $Y_{i,t}$ is a dummy variable for whether company i raised financing in year t from at least one U.S. HNWI, $\mathbb{1}\{t=j\}$ are year dummies, and QSBS_i is a dummy equal to 1 for treated companies and 0 for control companies. We control for company fixed effects α_i and separate year fixed effects for each dimension of QSBS eligibility as of the end of 2008—that is, $\gamma_{C(i),t}$, $\delta_{Q(i),t}$, and $\zeta_{M(i),t}$, where $C(i)$ indicates whether company i was a C corporation, $Q(i)$ indicates whether company i was active primarily in a qualified trade or business, and $M(i)$ indicates whether company i had raised no more than \$50 million in total financing. Since we exclude the coefficient β_{2008}, we interpret the parameter of interest β_j as the percentage-point change since 2008 in the probability that QSBS-eligible companies raised financing from at least one U.S. HNWI, relative to the equivalent change for ineligible issuers. We first estimate Equation (1) separately for each cohort of companies founded in each year. We then take a weighted average of the subsample-specific estimates for each year, using as the weight each founding-year cohort's share of all the companies in that year's sample. This approach addresses the issue that a single regression that pools different cohorts could generate spurious pre-trends due to the unbalancedness of the panel in the pre-period (see Appendix Section C.2 for a stylized example).

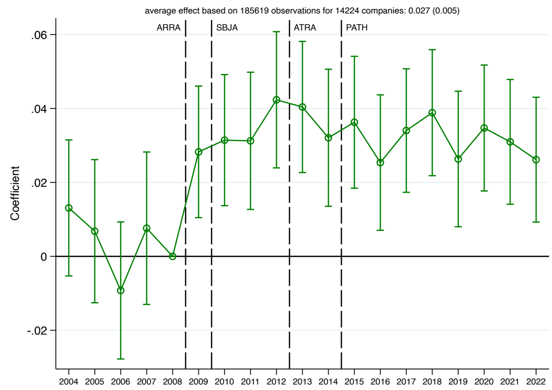
[29] We use total financing raised as a proxy for gross assets (i.e., the actual measure used to determine QSBS eligibility) since PitchBook has information on gross assets only for a limited number of companies (see Appendix Section C.1.4).

Figure 4a plots our estimates of Equation (1). The estimated coefficients for the years 2004-2007 are close to zero and statistically insignificant, indicating the absence of pre-trends. Immediately after the first QSBS reform in 2009, the probability that QSBS-eligible companies raised financing from U.S. HNWIs jumped by about 3 percentage points, with the effect remaining stable until 2022. The average effect over the entire post-reform period was 2.7 percentage points, an estimate that is statistically significant at the 1% level and corresponds to a 63% increase relative to the pre-reform probability of 4.3%. To explore which types of companies drove this average effect, Appendix Figure C2 reports the heterogeneous effects by founding-year cohort, sector, and total financing raised as of the end of 2008. We find a positive effect for all types of companies, but the only statistically significant heterogeneity is between younger companies, which were more affected, and less affected older ones. Our findings are robust to controlling for finer fixed effects (Appendix Figure C3) and to placebo tests on the probability of raising financing from non-U.S. HNWIs unaffected by the reforms (Appendix Figure C4). We also find a similar effect using an auxiliary regression discontinuity design that evaluates the effect of falling below the $50 million gross assets threshold on the subsample of 2,910 U.S. C corporations active primarily in a qualified trade or business (Appendix Figure C5).

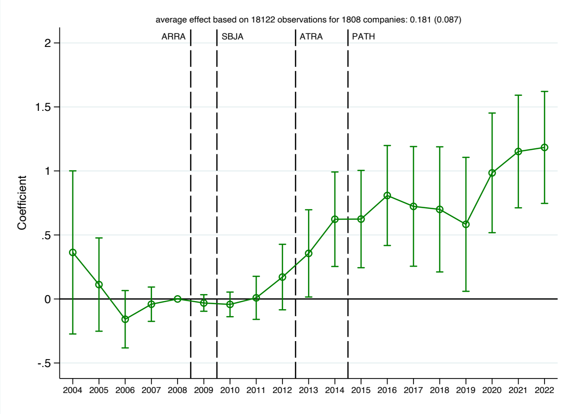
Figure 4b plots our estimates of Equation (1) when we instead consider as the outcome variable the log total amount of early-stage financing (in millions of U.S. dollars) that each company ever raised from U.S. HNWIs as of the end of each year. For this intensive-margin analysis, we consider only the 1,808 companies that had already raised some early-stage financing from U.S. HNWIs as of the end of 2008. The estimated coefficients for the pre-reform years are statistically insignificant. However, the post-reform estimates are statistically significant at the 5% level starting in 2013, indicating that QSBS-eligible companies' increased likelihood to raise financing from U.S. HNWIs after 2008 (see Figure 4a) did not immediately—but did eventually—translate into more financing raised relative to ineligible issuers. The average effect over the entire post-reform period corresponds to an 18.1% increase, which translates into about $360,000 relative to the $2 million in average total early-stage financing raised from U.S. HNWIs by the end of 2008 among the companies in our sample. One may worry that this increase in financing for QSBS-eligible companies was driven by HNWIs reallocating their investments from ineligible to eligible issuers of QSBS, violating our assumption that the QSBS reforms treated only eligible issuers. However, Appendix Figure C6 shows that ineligible issuers also saw an increase—albeit a smaller one than eligible issuers—in their financing raised from HNWIs after the reforms. Furthermore, most of the new financing from HNWIs was raised from first-time investors in early-stage companies (see Appendix Figure A6). Taken together, these results suggest that reallocation is an unlikely threat to our identification strategy.

Figure 4: Effects of the QSBS Reforms on Companies

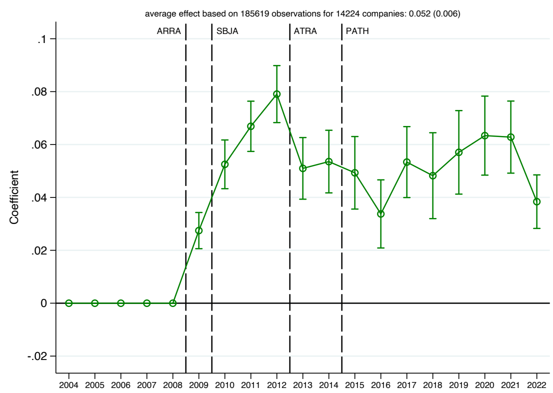
(a) Probability of Raising Early-Stage Financing from U.S. HNWIs



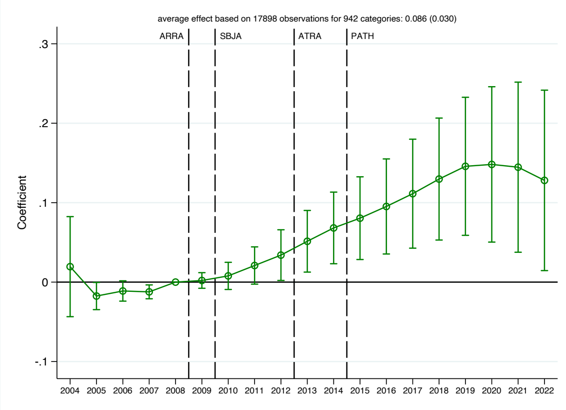
(b) Log Total Early-Stage Financing Ever Raised from U.S. HNWIs



(c) Probability of Remaining an Active Private Company



(d) Number of Active Private Companies (Based on Auxiliary Regression)



Sources: PitchBook.
Notes: This figure describes the QSBS reforms' effects on companies. Panels (a)-(c) report our estimates of 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 the effects on the log total amount of early-stage financing (in millions of U.S. dollars) that QSBS-eligible companies ever raised from U.S. HNWIs. Panel (c) plots the effects on QSBS-eligible companies' probability of remaining an active private company. In contrast, Panel (d) plots the effects on the number of active private companies based on an auxiliary Poisson pseudo-maximum likelihood regression that resembles Equation (1). Specifically, it regresses the number of active private companies in each category in each year on a dummy variable for whether the companies in that category were QSBS-eligible, as well as on category and year fixed effects. We cluster standard errors at the company or category level and report the 95% confidence interval for each estimate.

4.2.2 Effects on Companies' Real Outcomes

We next study how the increase in early-stage financing raised from U.S. high-net-worth individuals affected QSBS-eligible companies' real outcomes—namely, the probability of remaining an active private company, the number of active private companies, and the number of employees. We first consider as the outcome variable of Equation (1) a dummy variable for whether company i was still an active private company as of the end of year t—that is, whether it had not yet gone bankrupt, become publicly listed, or been acquired. Figure 4c shows that QSBS-eligible companies were, on average, 5.2 percentage points more likely to remain active private companies relative to ineligible issuers over the entire post-reform period. Since the ineligible issuers' post-reform probability of remaining an active private company was 92.3%, treated companies experienced a 5.6% increase in this probability relative to control companies. Appendix Figure C7 shows that most of this effect was explained by a decline in QSBS-eligible companies' probability of being acquired, while the rest was attributable to smaller declines in their probabilities of going bankrupt or becoming publicly listed. Thus, the financing raised from U.S. HNWIs helped startups avoid failure and remain independent rather than be acquired or go public.

A key limitation of Figure 4c is that it does not allow us to test for pre-trends, as we restrict our sample to active private companies as of the end of 2008—that is, companies that had neither gone bankrupt, nor become publicly listed, nor been acquired. To test for pre-trends, we therefore consider an auxiliary regression similar to Equation (1) that allows for the entry and exit of companies. Specifically, we regress the number of active private companies in category i in year t on a dummy variable QSBS_i for whether that category of companies was QSBS-eligible, on category fixed effects α_i, and on separate year fixed effects $\gamma_{C(i),t}$, $\delta_{Q(i),t}$, and $\zeta_{M(i),t}$ for each dimension of QSBS eligibility.[30] We define 942 categories based on the triple interaction of (1) whether a company was organized as a C corporation, (2) the one of 157 possible industries in which it was primarily active, and (3) whether it had raised no more than \$5 million, \$5–50 million, or more than \$50 million in total financing as of the end of the previous year, $t-1$. This specification makes it possible to evaluate whether the QSBS reforms led to greater net entry in QSBS-eligible categories relative to ineligible ones. Reassuringly, Figure 4d shows evidence of limited pre-trends and a sharp post-reform increase in the number of active private companies in QSBS-eligible categories. This finding is consistent with the result in Figure 4c that existing QSBS-eligible issuers were more likely to remain active private companies.

Finally, we do not find any evidence that the reforms affected the number of employees at QSBS-eligible companies (see Appendix Figure C8), using PitchBook's limited data on companies' payrolls. This result is noteworthy given that some of the reforms—e.g., the Small Business Jobs Act—were motivated partly by job-creation objectives.

Taken together, we find that QSBS-eligible issuers raised more early-stage financing from U.S. HNWIs and survived longer as active private companies, consistent with the growth

[30] To ensure consistency with the analysis in Figure 4c, in which we weight all companies equally, each company must receive equal weight. We therefore estimate this regression using the Poisson pseudo-maximum-likelihood (PPML) estimator, which implicitly weights each category in each year by the number of active private companies that it contains in that year (Santos Silva and Tenreyro, 2006).

in U.S. private capital markets (see Figure 1). In Section 6, we examine whether the reforms also increased these issuers' valuations, generating a self-reinforcing feedback loop in which HNWIs enriched by private capital markets become even more active participants in those markets, further expanding private capital market activity as a result.

4.3 HNWIs' Early-Stage Investments and Income Inequality

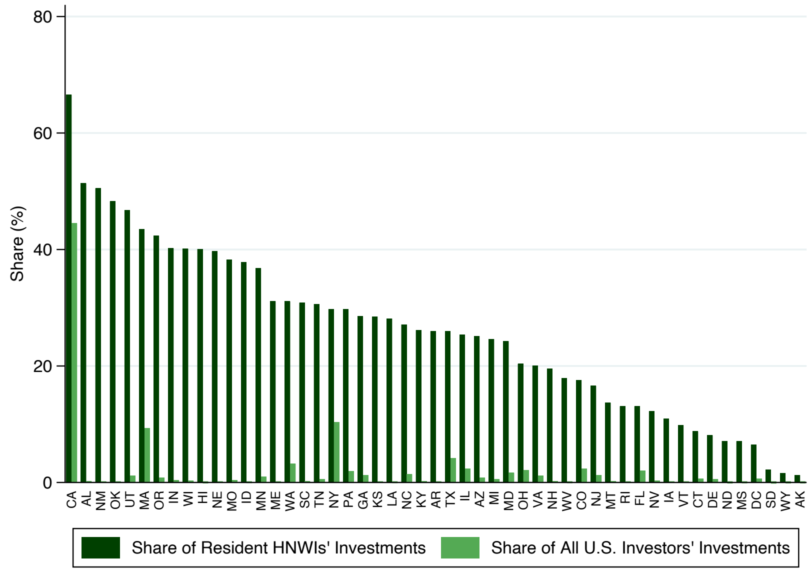
4.3.1 Effects on Investments: State Level

To assess the implications of high-net-worth individuals' increased participation in private capital markets for economic inequalities, we begin by analyzing the effects of the QSBS reforms on HNWIs' early-stage investments at the state level. Since the reforms constituted common shocks to the HNWIs residing in all U.S. states, we expect that they increased HNWIs' early-stage investments more in states where there were more resident HNWIs ex ante. We therefore use the number of accredited investors residing in each state in 2008 from the GEOWEALTH-US (see Section 2.2) as a state-level exposure measure. Appendix Figure C9 visualizes the intuition behind this 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 (on the horizontal axis) and the average across years (on the vertical axis) of resident HNWIs' total annual amount invested (Appendix Figure C9a) and the number of resident HNWIs investing (Appendix Figure C9b) in U.S. early-stage companies. The steepening of the slopes of these relationships after 2008 is consistent with our earlier evidence at the company level that the QSBS reforms increased HNWIs' early-stage investments (see Section 4.2.1).

There are two main factors that may confound the number of resident HNWIs in 2008 as a valid measure of the state-level exposure to the federal QSBS reforms. First, HNWIs may have settled in certain states specifically to get access to exclusive local investment opportunities (e.g., aspiring venture capitalists moving to California). Such selection into residing in certain states 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 their own state than the share of all U.S. investors' (i.e., both HNWIs' and institutional investors') early-stage investments allocated to those same companies.[31] Second, this home bias in HNWIs' early-stage investments risks contaminating our state-level exposure measure by correlating it with other local economic shocks. If startups in

[31] Appendix Figure C10 shows that HNWIs exhibited similar home bias during the pre-reform period from 2004 to 2008. Appendix Figure C11 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 (i.e., where Silicon Valley is located) and, to a lesser extent, Massachusetts and New York.

Figure 5: In-State Share of Resident HNWIs' Early-Stage Investments (2004-2022)



Sources: PitchBook.
Notes: This figure compares the share of each state's resident HNWIs' early-stage investments allocated to companies headquartered in their own state to the share of all U.S. investors' (i.e., both HNWIs' and institutional investors') early-stage investments allocated to those same companies from 2004 to 2022.

states with more resident HNWIs were exposed to different economic shocks than those in states with fewer resident HNWIs, then HNWIs' early-stage investments in states with more resident HNWIs may have grown faster for reasons unrelated to the QSBS reforms.

To address these potential confounders, we compare "treated" resident HNWIs' early-stage investments in companies headquartered in their own state to other "untreated" investors' investments in those same companies, namely, resident institutional investors, non-resident institutional investors, and non-resident HNWIs. First, the institutional investors residing in each state may face the same shocks to their early-stage investment opportunities as the state's resident HNWIs. However, since these resident institutional investors do not pay capital gains taxes, the QSBS reforms should not have affected them. Appendix Figure C12 visualizes the intuition for using resident institutional investors as a control group, showing that resident HNWIs' in-state early-stage investments grew by more than those of resident institutional investors after 2008. Second, like resident institutional investors, non-resident institutional investors should have similarly been unaffected by the reforms. Finally, the reforms are unlikely to have affected the relationship between the ex-ante number of HNWIs residing in each state and non-resident HNWIs' early-stage investments in companies headquartered in that state. Thus, in addition to the comparisons that we make across states and years, this third dimension of comparison across investor types ensures that we do not attribute to the reforms changes in resident HNWIs' early-stage investments that we also observe in those of unaffected investor types.

We formally analyze the effects of the QSBS reforms on resident HNWIs' early-stage investments at the state level using the following regression specification:

$$\ln Y_{s,g,t} = \sum_{j=2004, j\neq 2008}^{2022} \beta_j \times \mathbb{1}\{t=j\} \times T_g \times \ln X_{s,2008} + \alpha_{s,g} + \gamma_{g,t} + \delta_{s,t} + \zeta_{g,t} \times W_{s,t} + u_{s,g,t}, \quad (2)$$

where $\ln Y_{s,g,t}$ is the log total millions of U.S. dollars invested in startups headquartered in state s by investors of type g in year t;[32] $\mathbb{1}\{t=j\}$ are year dummies; T_g is a dummy variable equal to 1 for resident HNWIs and 0 for the three other investor types, namely, resident institutional investors, non-resident institutional investors, and non-resident HNWIs; and $\ln X_{s,2008}$ is the log number of HNWIs residing in state s in 2008. Since we include interacted state-investor type fixed effects $\alpha_{s,g}$, investor type-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 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 investor type and year.[33] Since we exclude the coefficient β_{2008}, we interpret the parameter of interest β_j as the change since 2008 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 for resident HNWIs, partialing out the average change across the three other investor types and thereby isolating the effect of the QSBS reforms on resident HNWIs' early-stage investments.

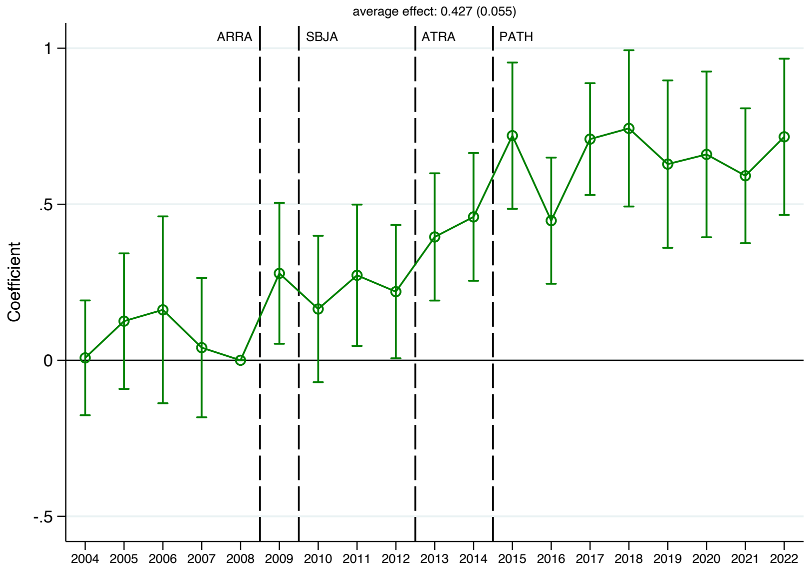
Figure 6 plots our baseline estimates of Equation (2), replacing $\ln Y_{s,g,t}$ with $\ln(1+Y_{s,g,t})$ as the outcome variable to ensure a balanced panel.[34] The estimated coefficients for the years 2004-2007 are statistically insignificant and exhibit no pre-trends. We find an immediate increase in resident HNWIs' early-stage investments in 2009, after the first

[32] We consider the total—rather than average—amount invested by each investor type since we observe the number of *potential* investors only for HNWIs (in the GEOWEALTH-US) but not for institutional investors (of which, in the data from PitchBook, we observe only those that *actually* invested).

[33] The expected state-level long-term capital gains tax wedge on QSBS accounts for heterogeneous exposure across states and investor types 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 early-stage investments.

[34] Appendix Figures C13a and C13b show that the dynamic pattern of our baseline estimates is robust to estimating Equation (2) using $\ln Y_{s,g,t}$ as the outcome variable (i.e., while dropping the 13% of observations for which $Y_{s,g,t}=0$) and to using the PPML estimator. The reason we prefer using $\ln(1+Y_{s,g,t})$ as the outcome variable in our baseline analysis is that the PPML estimator implicitly assigns larger weights to observations for which $Y_{s,g,t}$ is larger (Santos Silva and Tenreyro, 2006). As a result, even on the subsample of observations for which $Y_{s,g,t}>0$, our OLS estimates using $\ln Y_{s,g,t}$ as the outcome variable differ from our PPML estimates (see Appendix Figures C13c and C13d). 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 consider a logged outcome variable while retaining observations for which $Y_{s,g,t}=0$.

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



Sources: PitchBook, GEOWEALTH-US.
Notes: This figure describes the effects of the QSBS reforms on resident high-net-worth individuals' early-stage investments, reporting our estimates of Equation (2). To ensure a balanced panel, we replace $\ln Y_{s,g,t}$ with $\ln(1 + Y_{s,g,t})$ as the outcome variable. We cluster standard errors at the state level and report the 95% confidence interval for each estimate.

temporary expansion of the federal tax exclusion on QSBS capital gains. We find even further increases in 2013-2014, when the 100% exclusion was repeatedly—but still only temporarily—renewed. Our estimated effect reaches its peak when the 100% exclusion was made permanent in 2015, remaining near this elevated level until 2022. The dynamic effects that we estimate are therefore in line with the actual timing of the introduction of the reforms. Appendix Figure C14 shows that the increase in resident HNWIs' early-stage investments was driven by an increase in the number of resident HNWIs investing, rather than by an increase in their average amount invested, consistent with the entry of new investors being the key margin of adjustment (see Appendix Figure A6). These results are also in line with the increase in QSBS-eligible companies' probability of raising financing from U.S. HNWIs (see Section 4.2.1).

Our results are robust to using alternative vectors of control variables (Appendix Figure C15a) and state-level exposure measures (Appendix Figure C15b), including the number of residents ranked in the Forbes 400 rich list in 2008. They are also robust to dropping California and eight other states with major technology hubs (Appendix Figure C15c) and extending Equation (2) to consider resident HNWIs' early-stage investments in companies headquartered in states other than their own (Appendix Figure C15d). Reassuringly, the increase in the triple-difference parameters identified by Equation (2) was driven by an increase in the difference-in-differences parameters for resident HNWIs, rather than by a decrease for other investors (Appendix Figure C16a)—exactly as we would have expected, since resident HNWIs were the only "treated" investors with respect to our state-level exposure measure. Furthermore, our triple-difference estimates are robust to considering resident institutional investors as the only "untreated" investors (Appendix Figure C16b).

Finally, based on our baseline estimates of Equation (2), we quantify the scale of the QSBS reforms' effects relative to the overall growth in HNWIs' early-stage investments. We start by replacing the dynamic coefficient β_j in Equation (2) with a single post-reform coefficient $\beta_{j:j>2008}$, which we estimate to be 0.427. Given the 6.4 million HNWIs residing in the U.S. in 2008, this estimate implies that the QSBS reforms explained $(\sum_s X_{s,2008})^{\beta_{j:j>2008}} = (6.4 \times 10^6)^{0.427} \approx \$800$ million of the increase in HNWIs' early-stage investments between the average pre-reform year and the average post-reform year. Since the overall growth was \$6.9 billion (i.e., from an average of \$0.9 billion in the pre-reform period to \$7.8 billion in the post-reform period), we find that $0.8/6.9 \approx 12\%$ of the increase in HNWIs' early-stage investments after 2008 was attributable to the QSBS reforms.[35]

4.3.2 Implications for State-Level Income Inequality

In the first step of our state-level analysis in Section 4.3.1, we showed that resident HNWIs' early-stage investments increased more after the QSBS reforms in states that had more resident HNWIs ex ante. As the second step, we now examine whether economic inequalities also rose more in those same states, consistent with the excess returns that HNWIs earned on their early-stage investments relative to public stock markets (see Section 3.2). Ideally, we would study the implications of these investments for state-level wealth inequality, since HNWIs typically hold early-stage investments for about five years before realizing their accumulated returns as capital gains income (see Appendix Section B.2.2). However, to the best of our knowledge, there is no granular data on state-level wealth inequality for the U.S. that are based on observed wealth. We therefore instead focus on the implications of the QSBS reforms for state-level income inequality.

For that, we rely on the personal income tax statistics from the Internal Revenue Service's SOI Tax Stats to construct the taxable income distribution across individuals for each state in each year (see Section 2.2 and Appendix Section D.2.1). We further decompose the total taxable income earned by the individuals in each income group into realized capital gains income and other income. We divide each state-level income distribution

[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 of the QSBS reforms *before* summing across states. In this case, the larger weights assigned by the PPML estimator to observations for which $Y_{s,g,t}$ is larger become appropriate, since we are quantifying how the effect for each state contributes to the nationwide increase. If we use the PPML estimate of 0.285 for the average effect over the entire post-reform period (see Appendix Figure C13b), then the share of the overall growth in HNWIs' early-stage investments attributable to the QSBS reforms rises moderately to $(\sum_s X_{s,2008}^{0.285}) \ / \ 6.9 = 18\%$.

into 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. We split the top percentile in this way because average income differs drastically within it. For each 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 U.S. dollars of each component of total taxable income $Y_{s,g,t}$ earned by the individuals in that group.[36]

We then analyze the implications of the QSBS reforms on state-level income inequality using the following regression specification:

$$\ln Y_{s,g,t} = \sum_{j=2004, j\neq 2008}^{2022} \beta_j \times \mathbb{1}\{t = j\} \times T_g \times \ln X_{s,2008} + \alpha_{s,g} + \gamma_{g,t} + \delta_{s,t} + \zeta_{d(g),t} \times W_{s,t} + u_{s,g,t}, \quad (3)$$

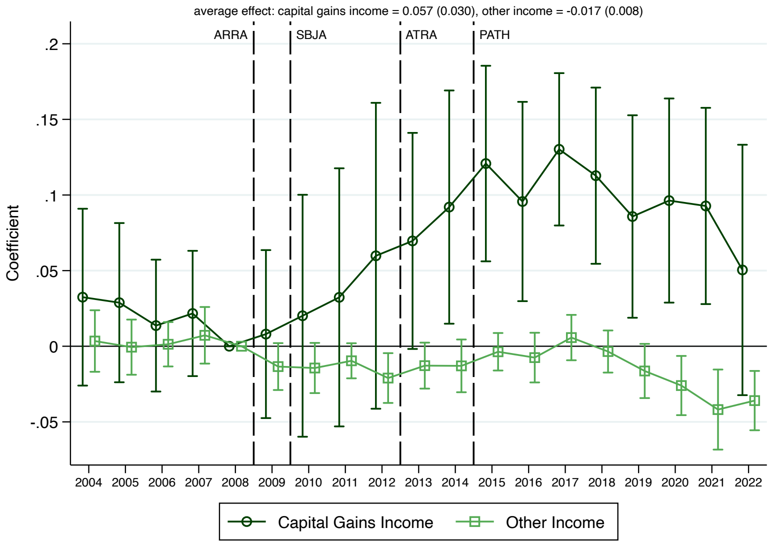
where $\ln Y_{s,g,t}$ is the log average taxable income for each income component (i.e., capital gains or other income) earned by the individuals in income group g of state s in year t; $\mathbb{1}\{t = j\}$ are year dummies; T_g is a dummy variable equal to 1 for income groups in the top 0.5% (i.e., $g > 99.5$) of the state-level income distribution and 0 for those in the bottom 99.5%; $\ln X_{s,2008}$ is the log number of HNWIs residing in state s in 2008; $W_{s,t}$ is a vector of control variables (i.e., the expected state-level long-term capital gains tax wedge on QSBS and the state-level house price index) whose effects $\zeta_{d(g),t}$ are the same for all income groups g within decile d of the state-level income distribution; and $\alpha_{s,g}$, $\gamma_{g,t}$, and $\delta_{s,t}$ are interacted state-income group, income group-year, and state-year fixed effects, respectively. We split the state-level income distribution at the 99.5th percentile because, in every state in every year, the average total income of the individuals in every income group above the 99.5th percentile exceeded \$200,000—that is, the individual income threshold for qualifying as an accredited investor (see footnote 1). We can therefore confidently make the assumption that, across all states and years, individuals ranked in the top 0.5% of their own state's income distribution were always accredited investors and, hence, more likely to participate in private capital markets. Since we exclude the coefficient β_{2008}, we interpret the parameter of interest β_j as reflecting how the average (log) income gap between the top 0.5% and bottom 99.5% of the state-level income distribution evolved since 2008 in states with more resident HNWIs.

Figure 7a plots our baseline estimates of Equation (3) for both realized capital gains

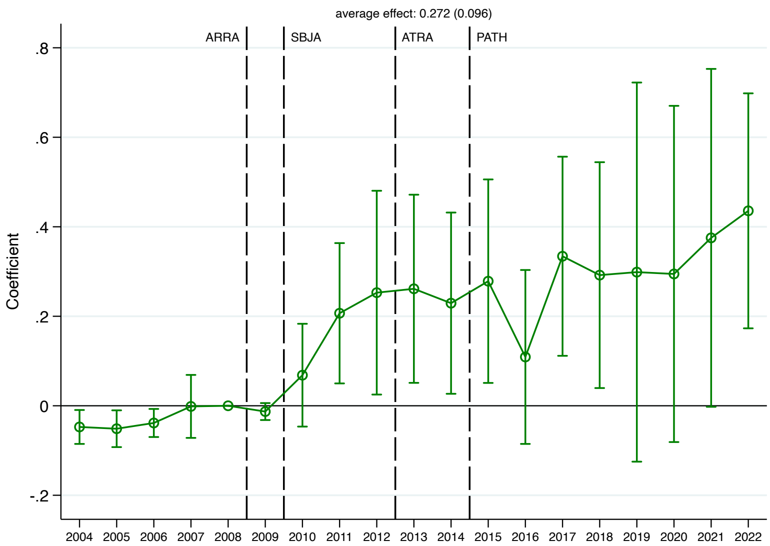
[36] Taxable income may exclude up to the first \$10 million of the capital gains income realized from the sale of QSBS excludable under the QSBS capital gains tax exclusion (see Section 4.1). Using the data from PitchBook, we find that the excludable share of the total capital gains income earned by U.S. HNWIs from QSBS was only 18% from 2004 to 2022 (see Appendix Section D.3 for details on this calculation). We therefore expect the majority of QSBS capital gains to be classified as taxable income.

Figure 7: Implications of the QSBS Reforms for State-Level Income Inequality

(a) Capital Gains Income vs. Other Income



(b) Early-Stage Capital Gains Income



Sources: SOI Tax Stats, PitchBook, GEOWEALTH-US.
Notes: This figure describes the implications of the QSBS reforms on state-level income inequality. Panel (a) reports our estimates of Equation (3), considering the log average capital gains and other income of each income group as the outcome variables. Each income group's regression weight corresponds to its share of the state's population. Panel (b) considers the log capital gains income of the HNWIs residing in each state in each year from their early-stage investments in 2004-2022 that yielded positive returns (based on PitchBook) per individual ranked in the top 0.5% of the state-level income distribution (based on the SOI Tax Stats). It regresses this outcome variable on the number of HNWIs residing in the state in 2008, as well as on state and year fixed effects. To ensure a balanced panel, we replace $\ln Y_{s,g,t}$ with $\ln[1 + \max(Y_{s,g,t}, 0)]$ as the outcome variable. We cluster standard errors at the state level and report the 95% confidence interval for each estimate. In Panel (b), the confidence intervals widen in the post-reform relative to the pre-reform period because $Y_{s,g,t} = 0$ more frequently before 2008. Since we consider only early-stage investments from 2004 to 2022, there are more exits in later years.

income and other income. We replace $\ln Y_{s,g,t}$ with the transformation $\ln[1+\max(Y_{s,g,t},0)]$ to ensure a balanced panel. We consider this transformation because realized capital gains income is negative for 4% of the state-income group-year observations.[37] The estimated coefficients for the pre-reform years are statistically insignificant. The average capital gains income gap between the top 0.5% and bottom 99.5% began gradually increasing with the introduction of the first QSBS reform in 2009. This increasing trend continued until stabilizing around 2014, which was the first year—five years after the first reform—in which HNWIs could claim the expanded federal tax exclusion on their QSBS capital gains.[38] In contrast to these estimates for realized capital gains income, which are statistically significant at the 5% level from 2014 to 2019, the estimates for the gap in average other income between the top 0.5% and bottom 99.5% are never statistically significant in these years. The estimates for other income become consistently statistically significant only starting in 2020, when our state-level exposure measure may have become confounded by local shocks to labor income inequality related to the COVID-19 pandemic (e.g., Autor et al., 2024). We further explore heterogeneity for the capital gains income gap and find that the gap widened by more if we focus on the top 0.1% (see Appendix Figure C18).

We evaluate the robustness of our estimates for the capital gains income gap along two dimensions. First, we show that our results are robust to using alternative vectors of control variables (Appendix Figure C19). Second, we find that the increase in the gap between the top 0.5% and bottom 99.5% was driven by an increase in the top 0.5%'s average realized capital gains income rather than a decrease in that of the bottom 99.5% (Appendix Figure C20)—exactly as we would have expected, since the QSBS reforms mainly affected HNWIs in the top 0.5%.

Since the SOI Tax Stats does not isolate the component of total realized capital gains income attributable to early-stage investments, we consider an auxiliary regression that also uses the data from PitchBook based on the same $\ln[1+\max(Y_{s,g,t},0)]$ transformation as in Figure 7a. Specifically, we calculate the average early-stage realized capital gains income of the top 0.5% in each state in each year, dividing its resident HNWIs' total realized capital gains income from their early-stage investments in 2004-2022 that yielded positive returns (based on PitchBook) by the number of individuals in the top 0.5% (based on the SOI Tax Stats).[39] We then estimate a difference-in-differences regression related

[37] Our results are robust to alternatively considering the inverse hyperbolic sine transformation that can, albeit imperfectly, handle negative values (see Appendix Figure C17).

[38] The fact that the capital gains income gap already began increasing in 2009 may seem at odds with the requirement that QSBS be held for at least five years to be able to claim the tax exclusion. However, this dynamic pattern is consistent with the QSBS reforms immediately inducing new investor entry (see Sections 4.2.1 and 4.3.1) that increased the value of incumbents' investments (see Section 6.1).

[39] We focus on investments that yielded positive returns so that we do not have to treat zero returns and negative returns the same when using the $\ln[1+\max(Y_{s,g,t},0)]$ transformation. Nevertheless, Appendix

Table 2: Implications of the QSBS Reforms for State-Level Income Inequality (2014-2019)

	Capital Gains	Other Income	Early Stage	Total Income
	(1)	(2)	(3)	(4)
Number of resident HNWIs in 2008	0.102***	-0.007	0.257***	0.016**
	(0.030)	(0.006)	(0.110)	(0.006)
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
No. observations	45,339	45,339	357	45,339

Sources: SOI Tax Stats, PitchBook, GEOWEALTH-US.
Notes: This table describes the implications of the QSBS reforms on state-level income inequality in 2014-2019 relative to 2008. Columns (1), (2), and (4) report our estimates of Equation (3), considering the log average capital gains, other, and total income, respectively, of each income group as the outcome variables. Each income group's regression weight corresponds to its share of the state's population. Column (3) considers the log capital gains income of the HNWIs residing in each state in each year from their early-stage investments in 2004-2022 that yielded positive returns (based on PitchBook) per individual ranked in the top 0.5% of the state-level income distribution (based on the SOI Tax Stats). It regresses this outcome variable on the number of HNWIs residing in the state in 2008, as well as on state and year fixed effects. To ensure a balanced panel, we replace $\ln Y_{s,g,t}$ with $\ln[1+\max(Y_{s,g,t},0)]$ as the outcome variable. Since the outcome variables are in logs, the estimated coefficients in Columns (1) and (2) do not add up to that in Column (4). Observations refer to those at the state-income group-year level, which is why there are fewer observations in Column (3). We cluster standard errors at the state level. ***, **, and * denote statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

to Equation (3), regressing the log average early-stage capital gains income of the top 0.5% in each state in each year on the number of HNWIs residing in the state in 2008, as well as on state and year fixed effects. Figure 7b reports our estimates. Reassuringly, the dynamic pattern for the top 0.5%'s average early-stage capital gains income is similar to that for the average capital gains income gap between the top 0.5% and bottom 99.5% (see Figure 7a). This result suggests that the rise in the top 0.5%'s total capital gains income was driven, at least in part, by the increase in their early-stage capital gains income.

Finally, we analyze the implications of this increase in the top 0.5%'s early-stage capital gains income for total income inequality between the top 0.5% and bottom 99.5% of the state-level income distribution. Table 2 reports our estimate of the average coefficient on the number of resident HNWIs in 2008 for different outcome variables, comparing only the years 2014-2019 (i.e., the period covering at least five years after the first QSBS reform but before the COVID-19 pandemic) to 2008 (i.e., the year before the first reform). This

Figure C21 shows that our baseline estimates are robust to also considering resident HNWIs' realized capital gains income from their early-stage investments in 2004-2022 that yielded negative returns.

comparison focuses on the years in which HNWIs could claim the expanded federal capital gains tax exclusion on QSBS capital gains, drops the confounded pandemic years, and ignores the noise from statistically insignificant pre-trends. Columns (1)-(3) summarize our findings from Figure 7: after the QSBS reforms, the gap between the top 0.5% and bottom 99.5% increased in terms of their capital gains income but not their other income, driven by the increase in the top 0.5%'s early-stage capital gains income. Additionally, Column (4) shows that this increase was large enough to affect the average total income gap between the top 0.5% and bottom 99.5%, indicating that the increase in HNWIs' early-stage investments increased state-level income inequality. The estimated coefficient of 0.016 in Column (4) corresponds to a 1% increase relative to the pre-reform gap in the post-reform period.[40] In Section 5, we build on this state-level result to quantify the implications for wealth inequality at the national level.

5 Wealth Inequality Counterfactual Simulations

The analyses in Section 4.3.2 assess the implications of HNWIs' increasing participation in private capital markets for state-level income inequality. However, they consider only the role of realized capital gains at the state level, ignoring that of unrealized capital gains as well as the distributional implications at the national level. To therefore understand the broader contribution of HNWIs' excess returns on their early-stage investments relative to public stock markets to rising economic inequalities, we next consider both realized and unrealized capital gains and focus on wealth inequality at the national level. In this section, we run partial-equilibrium counterfactual simulations to quantify how HNWIs' increased early-stage investments have shaped U.S. wealth inequality over the last two decades. Our counterfactual simulations assume that HNWIs would have instead chosen to invest in public stock markets, which consist of similarly risky equities. Section 5.1 details our methodology. Section 5.2 presents our main results. In Appendix D.3.1, we further assess by means of counterfactual simulations the implications of the excess return channel and the tax savings associated with the QSBS reforms for U.S. income inequality.

5.1 Methodology

To carry out our counterfactual simulations of wealth inequality, we construct U.S. wealth distribution series following the Distributional Financial Accounts (DFA) methodology developed by Batty et al. (2022). The authors combine the Survey of Consumer Finances

[40] In Section 4.3.1, we regress HNWIs' early-stage investments on the number of resident HNWIs in 2008 using Equation (2), estimate that the coefficient on the latter is 0.427, and quantify that the QSBS reforms increased HNWIs' early-stage investments by \$0.8 billion per year relative to a pre-reform baseline of \$0.9 billion. Given that the log average income for the top 0.5% and bottom 99.5% of the U.S. income distribution in 2008 was 3.574 and 7.085 (in log points), respectively, the QSBS reforms widened the average income gap between the top 0.5% and bottom 99.5% by $(0.016/0.427) \times (0.8/0.9) \ / \ (7.085 - 3.574) \approx 1\%$.

with the Financial Accounts of the U.S. to build wealth distribution series consistent with macroeconomic aggregates. We follow the authors in further improving 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 analyses of state-level income inequality in Section 4.3.2—which focus on the top 0.5% income groups at the state level—our baseline counterfactual analyses focus on the top 0.5% wealth group at the national level. Appendix Table D4 reports summary statistics for the top wealth groups in the U.S. in 2010 and 2022. The top 0.5% wealth group had an average net wealth of $31 million and owned 22% of total net wealth in 2010. The wealth share for this group increased to 23% by 2022, with its average net wealth growing substantially and reaching $48 million.

We run the simulations for 2010-2022 (i.e., the years after the first QSBS reform), using HNWIs' private and counterfactual public capital gains—both realized and unrealized—as derived from PitchBook and described in Appendix B.[41] In particular, we reconstruct the wealth distribution series by replacing the accumulated private capital gains with the accumulated counterfactual capital gains had HNWIs instead invested in the total return version of the NASDAQ 100. We use the NASDAQ 100 as our baseline counterfactual, since among major U.S. public stock market indices, it puts the most weight on high-growth technology companies and, therefore, most closely resembles the sectoral composition of HNWIs' early-stage investments (see Appendix Figure A8). Our baseline counterfactual analyses focus 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 about 94% of the individuals to whom we assign capital gains belong to the top 10% wealth group.

The main challenge that we face when implementing the counterfactual simulations is how to distribute the accumulated private and counterfactual public capital gains along the wealth distribution in the SCF. This is not an issue for the HNWIs ranked in the Forbes 400 rich lists, since we observe HNWIs' names in PitchBook (see Appendix Section A.3.3). We thus directly attribute to the Forbes 400 HNWIs their accumulated capital gains on their own early-stage and counterfactual investments. For the remaining HNWIs in PitchBook, we rely on an imputation procedure to identify households in the SCF to which to assign those HNWIs' accumulated capital gains. Specifically, we consider the accredited investors in the SCF who are full or partial owners of a C corporation or partnership, and we rank them into 100 percentiles based on their private business wealth. We similarly rank the non-Forbes 400 HNWIs in PitchBook into 100 percentiles based on the accumulated value of their early-stage investments. We then match percentiles

[41] We start the counterfactual simulations in 2010, since it is the first post-reform year for which there is a wave of the SCF available. By "private" and "public" capital gains, we refer to U.S. HNWIs' returns on their early-stage investments in U.S. companies from 2004 to 2022 and on their counterfactual investments in public stock markets, respectively.

across the two populations and, within each percentile, distribute HNWIs' accumulated capital gains in each year in proportion to private business wealth. This methodology allows for return heterogeneity both from the direct assignment of capital gains to the Forbes 400 HNWIs and from the differences in returns along the distribution of private business wealth across the non-Forbes 400 HNWIs. Appendix D.3.2 further details our methodology to carry out our wealth inequality counterfactual simulations.

Finally, although PitchBook provides a rich snapshot of HNWIs' early-stage investments and their returns on them, it does not necessarily capture the universe of such investments. We may therefore be missing part of HNWIs' accumulated private capital gains. This issue of missing data is of particular importance to the counterfactual simulations that we run in this section, as we aim to quantify the overall effect of HNWIs' increased early-stage investments on wealth inequality. To overcome this challenge, we rescale both the private and counterfactual public capital gains so that HNWIs' total claimable tax exclusions on QSBS capital gains as derived from PitchBook match the total claimed exclusions as reported in the IRS's aggregated personal income tax data (Abdulrauf et al., 2025). Specifically, we use as a multiplier the ratio of the total claimed exclusions according to the IRS ($152 billion) to the total claimable exclusions as derived from PitchBook ($28 billion) from 2012 to 2022 (see Appendix Figure D4). This calculation yields a rescaling of about 5.4 times HNWIs' accumulated capital gains. In what follows, we present results with and without rescaling. Appendix D.3.2 further details our rescaling methodology.

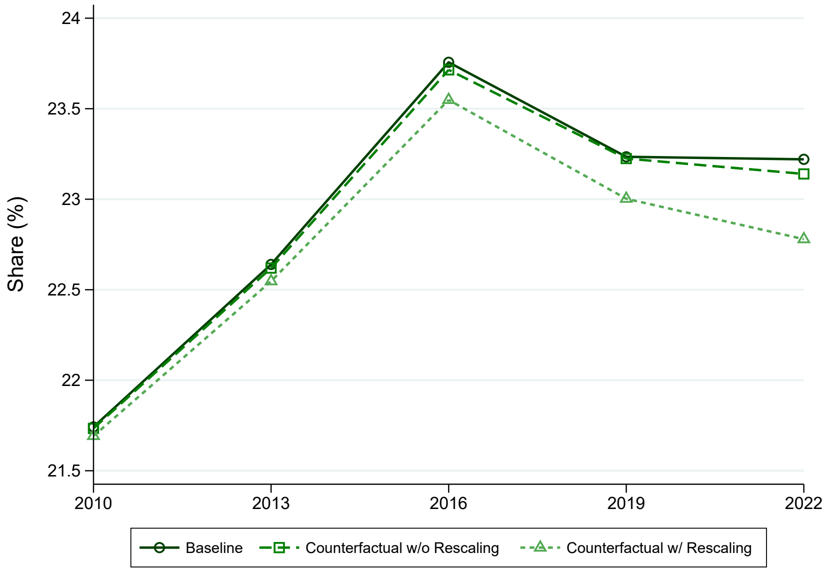
Our counterfactual simulations should be interpreted as partial-equilibrium exercises. In particular, they abstract from general-equilibrium effects that could arise from HNWIs' increasing participation in private capital markets, including spillovers to other asset markets, changes in companies' financing and listing decisions, and broader effects on labor income. However, these forces are unlikely to substantially alter the magnitude of our estimates. First, potential spillovers to public stock markets are likely to be limited, given that the accumulated value of HNWIs' early-stage investments from 2004 to 2022 ($327 billion) represented less than 1% of the total market capitalization of all U.S. public companies ($40 trillion) in 2022 (see https://data.worldbank.org/indicator/CM.MKT.LCAP.CD?locations=US). Second, in our state-level analyses, the rise in the income gap between the top 0.5% and bottom 99.5% was driven by capital gains rather than by other income (see Figure 7a), although this evidence does not rule out labor-income effects within or across other parts of the distribution. Finally, we do not account for private capital gains accruing to households indirectly through pension funds, since PitchBook's data does not allow us to link pension funds to pensioners. Nevertheless, this omission is also unlikely to substantially affect our estimates: pension wealth accounts for only a small share of wealthy households' portfolios (Appendix Figure A2), and pension funds allocate only a small share of their assets under management to private capital markets (see https://equable.org/pension-funding-trends-2023). Taken together, these considerations suggest that general-equilibrium adjustments and indirect pension-fund exposures are unlikely to materially change the magnitude of our partial-equilibrium estimates.

5.2 Results

Figure 8 compares the baseline top 0.5% wealth share to the counterfactual top 0.5% wealth share from 2010 to 2022 under scenarios without HNWIs' excess returns on their early-stage investments—that is, if they had instead invested in the NASDAQ 100. Given that wealth shares are slow-moving variables, the difference between the baseline and counterfactual series is not very large in absolute terms—with a difference of less than 1 percentage point in 2022—even for the version using rescaled capital gains. However, the effects are much larger when quantifying the contribution of the excess return channel to the overall growth of the top 0.5% wealth share over this period. Table 3 compares the baseline growth rate of the top 0.5% wealth share from 2010 to 2022 to the counterfactual growth rates. As expected, the growth rate of the top 0.5% wealth share is lower in the counterfactual scenarios in which HNWIs had invested in public stock markets (6.5% without rescaling, 5.0% with rescaling) than in the baseline scenario in which they invested in early-stage companies (6.8%). HNWIs' excess returns on their early-stage investments relative to public stock markets thus account for $0.3/6.8 \approx 5\%$ ($1.8/6.8 \approx 26\%$) of the overall growth in the top 0.5% wealth share from 2010 to 2022 without (with) rescaled capital gains. Our results are robust in terms of both direction and magnitude to the use of the S&P 500, Russell 2000, and FANG+ public stock market indices as well as the Barclay Hedge Fund Index as alternative counterfactuals (Appendix Table D10).

We find that the heterogeneity in private capital gains across the wealth distribution exacerbated the effect of the excess return channel: under homogeneous returns, private capital gains would have accounted for only 2% (13%) of the overall growth in the top 0.5% wealth share from 2010 to 2022 without (with) rescaled capital gains (Appendix Table D11). We further assess the sensitivity of the excess return channel for wealth inequality to alternative assumptions about the rescaling, namely, about the missing amounts invested and their corresponding rates of return. Specifically, Appendix Figure D7 illustrates the contribution of the channel to the overall growth of the top 0.5% wealth share under homogeneous returns and alternative assumptions about the rescaling. We find that PitchBook would have to be missing about $1.8 trillion of U.S. HNWIs' early-stage investments in U.S. companies—or 16 times U.S. HNWIs' total early-stage investments in U.S. companies as observed in PitchBook (see Appendix Table A2)—for the excess return channel to have zero effect on wealth inequality. If we divide the $1.8 trillion of U.S. HNWIs' missing investments by their 6% share of all early-stage investments in U.S. companies (see Figure 1b), this implies $30.4 trillion of missing investments

Figure 8: Top 0.5% Wealth Share: Baseline vs. Counterfactuals



Sources: Survey of Consumer Finances, Financial Accounts, Forbes, PitchBook, Capital IQ.
Notes: This figure compares the baseline top 0.5% wealth share to the counterfactual top 0.5% wealth share from 2010 to 2022 constructed by replacing private capital gains with counterfactual capital gains based on the total return version of the NASDAQ 100 public stock market index. We present the counterfactuals both with and without rescaled capital gains. We construct the baseline wealth distribution series by combining the SCF with the Financial Accounts of the U.S. and the Forbes 400 rich lists following the Distributional Financial Accounts methodology developed by Batty et al. (2022).

Table 3: Growth of Top 0.5% Wealth Share: Baseline vs. Counterfactuals (2010-2022)

		Counterfactual: NASDAQ 100	
	Baseline	Without Rescaling	With Rescaling
	(1)	(2)	(3)
Absolute growth rate	6.80%	6.47%	5.01%
Share of growth rate	/	4.85%	26.32%

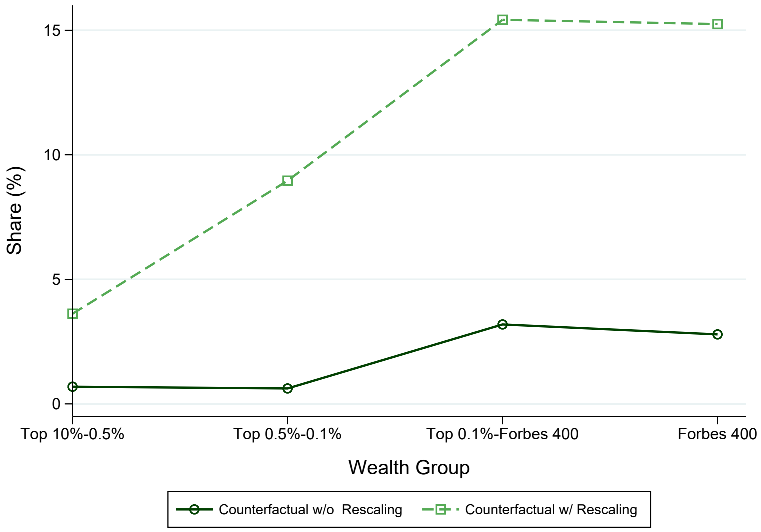
Sources: Survey of Consumer Finances, Financial Accounts, Forbes, PitchBook, Capital IQ.
Notes: This table compares the baseline growth rate of the top 0.5% wealth share to the counterfactual growth rate of the top 0.5% wealth share from 2010 to 2022 by replacing private capital gains with counterfactual capital gains based on the total return version of the NASDAQ 100 public stock market index. The table also reports the share of the overall 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 of the wealth share without excess returns and the baseline growth rate of the wealth share. We present the counterfactuals both with and without rescaled capital gains. We construct the baseline wealth distribution series by combining the SCF with the Financial Accounts of the U.S. and the Forbes 400 rich lists following the Distributional Financial Accounts methodology developed by Batty et al. (2022).

from all investors—which, compared to the $1.9 trillion of observed investments from all investors in PitchBook (see Figure 1a), seems implausible. Thus, any deviations from the assumptions about the rescaling underlying our baseline counterfactuals are unlikely to overturn the positive contribution of the excess return channel to wealth inequality.

We further analyze whether the contribution of the excess return channel to the overall growth of top wealth shares from 2010 to 2022 was heterogeneous across the wealth distribution. Figure 9 shows that the excess return channel was more important in explaining the growth in the wealth share of billionaires than that of millionaires. In particular, for the top 0.1%-Forbes 400 and the Forbes 400 wealth groups, private capital gains account for approximately 3% (15%) of the overall growth of their wealth shares over the 2010-2022 period without (with) rescaled capital gains. In contrast, for the top 10%-0.5% and the top 0.5%-0.1% wealth groups, private capital gains only account for 1% (4% and 9%, respectively) of the overall growth of their wealth shares over the 2010-2022 period without (with) rescaled capital gains. Our results are robust in terms of both direction and magnitude to the use of other public stock market indices and the Barclay Hedge Fund Index as alternative counterfactuals (Appendix Tables D12 and D13). Taken together, these findings are consistent with private business wealth being highly concentrated at the top of the wealth distribution (see Figure A2) and with HNWIs' excess returns on their early-stage investments relative to public stock markets (see Section 3.2).

Finally, we emphasize that HNWIs' increased early-stage investments contributed to the rise in wealth inequality over the 2010-2022 period only because they yielded excess returns relative to public stock markets over this period. If public stock markets had instead outperformed HNWIs' early-stage investments, then wealth inequality would—all else equal—have likely declined. In Table 4, we provide supporting evidence for this argument by running an alternative counterfactual simulation to the one in Table 3. In particular, we now consider what would have happened if, in every year from 2010 to 2022, HNWIs had instead earned the excess returns relative to the NASDAQ 100 that they actually earned in 2023. We thus replace the actual private capital gains with the counterfactual private capital gains such that the excess returns reflect those in 2023. We consider the excess returns in 2023 to run this alternative counterfactual simulation because HNWIs' early-stage investments particularly underperformed public stock markets in that year. Under this alternative scenario, HNWIs' private capital gains from their early-stage investments would have slowed down by 2% (11%) the overall growth of the top 0.5% wealth share over the 2010-2022 period without (with) rescaled capital gains. These analyses therefore illustrate that HNWIs' increasing participation in private capital markets can—all else equal—either increase or decrease economic inequalities, depending on whether these markets outperform or underperform public stock markets.

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



Sources: Survey of Consumer Finances, Financial Accounts, Forbes, PitchBook, Capital IQ.
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% was accounted for by excess returns (i.e., by replacing private capital gains with counterfactual public gains based on the total return version of the NASDAQ 100 public stock market index). The share of growth for every wealth group is derived as one minus the ratio between the counterfactual growth rate of the wealth share without excess returns and the baseline growth rate of the wealth share. Whenever the growth rate is negative (e.g., for the top 0.5%-0.1%), we report the absolute value of one minus the ratio to ensure consistency. We present the counterfactuals both with and without rescaled capital gains. We construct the baseline wealth distribution series by combining the SCF with the Financial Accounts of the U.S. and the Forbes 400 rich lists following the Distributional Financial Accounts methodology developed by Batty et al. (2022).

Table 4: Growth of Top 0.5% Wealth Share:
Baseline vs. Counterfactuals assuming 2023 Excess Returns (2010-2022)

		Counterfactual: 2023 Excess Returns	
	Baseline	Without Rescaling	With Rescaling
	(1)	(2)	(3)
Absolute growth rate	6.80%	6.95%	7.57%
Share of growth rate	/	-2.21%	-11.32%

Sources: Survey of Consumer Finances, Financial Accounts, Forbes, PitchBook, Capital IQ.
Notes: This table compares the baseline growth rate of the top 0.5% wealth share to the counterfactual growth rate of the top 0.5% wealth share from 2010 to 2022 by replacing actual private capital gains with counterfactual private capital gains, had HNWIs instead earned in every year the excess returns relative to the total return version of the NASDAQ 100 that they actually earned in 2023. The table also reports the share of the overall 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 of the wealth share without excess returns and the baseline growth rate of the wealth share. The shares are negative in this case because the counterfactual growth rate is higher than the baseline growth rate. We present the counterfactuals both with and without rescaled capital gains. We construct the baseline wealth distribution series by combining the SCF with the Financial Accounts of the U.S. and the Forbes 400 rich lists following the Distributional Financial Accounts methodology developed by Batty et al. (2022).

6 Feedback Loop

In this section, we explore whether the relationship between high-net-worth individuals' increasing participation in private capital markets and rising economic inequalities generates a self-reinforcing feedback loop. If successful early-stage investors reinvest their returns into new early-stage investments, or if some HNWIs are particularly skilled at early-stage investing, then the rising inequality generated by expanding private capital markets may further expand those markets in a way that reinforces the rise in inequality. Section 6.1 exploits the QSBS reforms (see Section 4.1) to show that HNWIs respond to the growing valuations of the startups in which they have invested by expanding their early-stage investments. Section 6.2 documents persistence in HNWIs' rankings in the distribution of annual returns on their early-stage investments, suggesting that heterogeneity in skill may amplify the inequality in accumulated returns across early-stage investors.

6.1 Implications of Valuation Growth for Incumbent Investors

In Section 4.2, we show that the QSBS reforms increased QSBS-eligible companies' likelihood to raise early-stage financing from U.S. high-net-worth individuals and, in turn, to remain active private companies. We now examine the implications of these reforms for the valuations of QSBS-eligible companies, as well as for the HNWIs who had invested in them before the reforms. In both cases, we estimate the following regression specification:

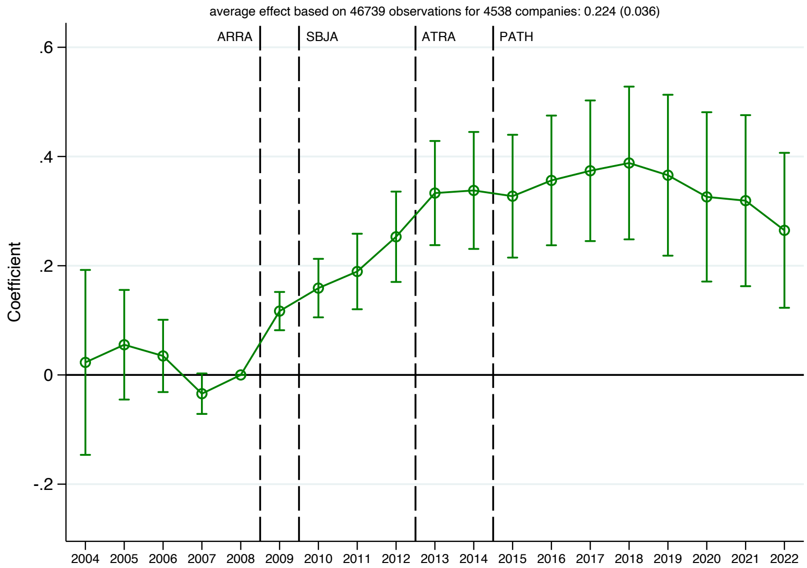
$$\ln Y_{i,t} = \sum_{j=2004, j\neq 2008}^{2022} \beta_j \times \mathbb{1}\{t=j\} \times \mathrm{QSBS}_i + \alpha_i + \gamma_t + u_{i,t}. \tag{4}$$

When we use Equation (4) to study the effects of the reforms on companies' valuations, the outcome variable $\ln Y_{i,t}$ is the log valuation of company i at the end of year t; QSBS_i indicates whether company i was QSBS-eligible as of the end of 2008 (see Section 4.2.1 and Appendix Section C.1); and α_i denotes standard company fixed effects. When we instead use Equation (4) to study the effects of the reforms on HNWIs who had already invested in early-stage companies before the reforms, the outcome variable $\ln Y_{i,t}$ is the log total amount that HNWI i ever invested in U.S. early-stage companies as of the end of year t; QSBS_i is the share of i's total amount invested—as of the end of 2008—that had been invested in QSBS-eligible companies;[42], and α_i denotes standard investor fixed effects. In both cases, $\mathbb{1}\{t = j\}$ are year dummies and γ_t denote standard year fixed effects. Figure 10 plots the estimates of Equation (4) for each of these two cases.

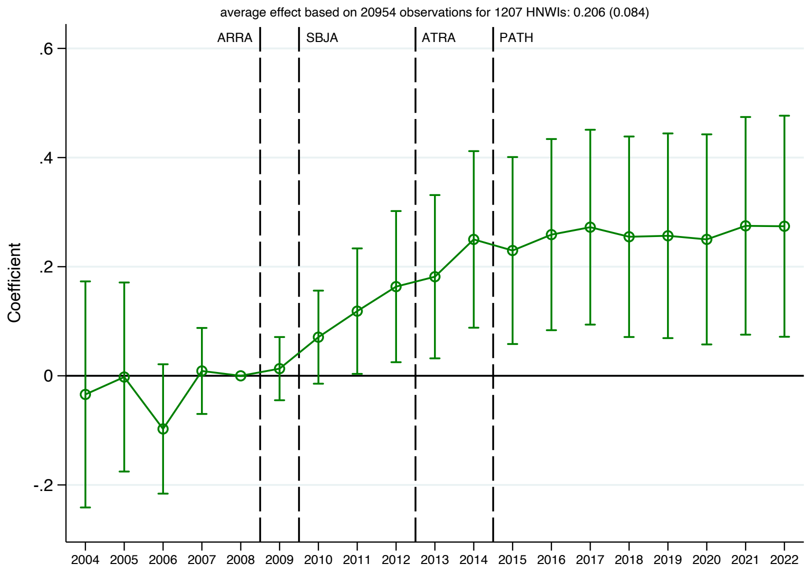
[42] If i had invested in both eligible and ineligible issuers of QSBS before the reforms, then $\mathrm{QSBS}_i \in (0, 1)$. Of the 1,207 U.S. HNWIs for whom we can define QSBS_i, 519 (43%) had invested in only eligible issuers, 340 (28%) had invested in only ineligible issuers, and 348 (29%) had invested in both. Because the amount invested is missing for some investments, we impute it—only for the purpose of calculating QSBS_i—using the mean amount invested across U.S. HNWIs' early-stage investments in U.S. companies in each year.

Figure 10: Implications of the QSBS Reforms for Companies' Valuations and Investors

(a) Company's Log Valuation



(b) Investor's Log Total Amount Ever Invested in U.S. Early-Stage Companies



Sources: PitchBook.
Notes: This figure describes the implications of the QSBS reforms on companies' valuations and investors, based on our estimates of Equation (4). Panel (a) plots the estimates from a company-level regression of each company's log valuation as of the end of each year on a dummy variable for QSBS eligibility as of the end of 2008, as well as on company and year fixed effects. Panel (b) plots the estimates from an investor-level regression of each U.S. HNWI's log total amount ever invested in U.S. early-stage companies as of the end of each year on the share of their investments as of the end of 2008 that had been invested in QSBS-eligible companies, as well as on investor and year fixed effects. We cluster standard errors at the company or investor level and report the 95% confidence interval for each estimate.

Figure 10a shows that, following the QSBS reforms, the valuations of eligible issuers grew relative to those of ineligible issuers. We find no evidence of statistically significant pre-trends before the reforms. Valuations began to grow gradually in 2009 and stabilized at their higher levels from 2013 onward. The average estimate over the post-reform period, statistically significant at the 1% level, implies an average increase in valuation of 22.4%.

Figure 10b shows that HNWIs already invested in QSBS-eligible companies responded to the growing value of the QSBS that they held by expanding their early-stage investments. Relative to U.S. HNWIs that had invested only in ineligible issuers before the QSBS reforms, those that had invested only in eligible issuers increased their total amount invested in U.S. early-stage companies by 20.6% on average over the post-reform period. The dynamic pattern closely resembles that in Figure 10a, consistent with the growing value of QSBS encouraging HNWIs to invest further. Thus, new investor entry following the QSBS reforms increased the value of incumbents' early-stage investments and, in turn, induced those incumbents to expand their investments. More broadly, these findings suggest that the inequality arising from the expansion of private capital markets may itself fuel the further expansion of those markets, generating a self-reinforcing feedback loop.

6.2 HNWIs' Skill in Early-Stage Investing

We finally examine whether skill heterogeneity further reinforces the feedback loop between private capital markets and inequality by amplifying differences in accumulated returns across the high-net-worth individuals participating in those markets. For that, we estimate the persistence of a U.S. HNWI's rank in the distribution of their annual rates of return on their early-stage investments in U.S. companies, using the following regression specification:

$$R_{i,t} = \beta \times R_{i,t-1} + \alpha + \gamma_i + u_{i,t}, \tag{5}$$

where $R_{i,t} \in (0,1]$ is HNWI i's rank in the distribution of annual rates of return in year t; β is a parameter that measures rank persistence; α is a constant; and γ_i is an optional investor fixed effect. If β decreases when we include γ_i in Equation (5), then persistence must be driven to some extent by time-invariant investor characteristics such as skill.

Table 5 reports our estimates of Equation (5). Column (1) shows that, based on a sample of 78,350 consecutive-year pairs from 2004 to 2022 for 12,895 investors, we estimate β to be 0.426. In other words, an HNWI who ranked 10 percentiles higher in the raw rate of return distribution for year $t-1$ is expected to rank about 4 percentiles higher in the distribution for year t. However, Column (2) shows that this estimate falls by 60% to 0.170 if we include investor fixed effects, implying that time-invariant investor characteristics account for a large share of the persistence reported in Column (1).

Table 5: Persistence of HNWIs' Rank in Annual Rate of Return Distribution (2004-2022)

	Rank at t			
	Raw Return		Risk-Adjusted Return	
	(1)	(2)	(3)	(4)
Rank at $t-1$	0.426***	0.170***	0.452***	0.139***
	(0.004)	(0.004)	(0.004)	(0.004)
Constant	Yes	Yes	Yes	Yes
Investor fixed effects	No	Yes	No	Yes
No. observations	78,350	78,350	78,350	78,350
No. investors	12,895	12,895	12,895	12,895

Sources: PitchBook; https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.
Notes: This table reports our estimates of Equation (5). Using investor-year observations from 2004 to 2022, we regress each U.S. HNWI's rank in year t in the distribution across U.S. HNWIs of their annual rates of return on their early-stage investments in U.S. companies on the same investor's rank in the distribution for year $t-1$. We calculate risk-adjusted rates of return based on investor-specific regressions of each investor's log return in year t in excess of the risk-free rate on the excess log return of the market in t, partialing out the effect of this one factor. We cluster standard errors at the investor level. ***, **, and * denote statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Since these time-invariant characteristics include not only skill but also risk preferences, Columns (3) and (4) of Table 5 repeat this exercise but now based on the distribution of risk-adjusted rates of return. For that, we estimate HNWI i-specific regressions of i's log return in year t in excess of the risk-free rate on the excess log return of the market in t, partialing out the effect of this one factor. We then rank HNWIs in each year in terms of their risk-adjusted log rate of return, which equals the sum of the estimated constant and residual from this regression. Including investor fixed effects leads to a similar decline in our estimate of the persistence of risk-adjusted returns from 0.452 to 0.139, suggesting that heterogeneous risk preferences are unlikely to be driving the documented persistence.

Instead, persistence may reflect skill in early-stage investing—HNWIs that are already successful early-stage investors are likely to remain successful in the future. On the one hand, this means that inequality *within* private capital markets is likely to grow over time, potentially at the same time as inequality *between* HNWIs and the individuals excluded from those markets is already growing (see Section 5). In this way, the rising inequality generated by HNWIs' increasing participation in private capital markets may become increasingly concentrated among a smaller and smaller group of early-stage investors, thereby amplifying inequality. On the other hand, skill heterogeneity implies that certain HNWIs' high degree of skill in early-stage investing may be an important source of their excess returns on their early-stage investments relative to public stock markets (see Section 3.2). This finding would complement that of Balloch et al. (2026), who find that the skill of the financial advisors hired by HNWIs to manage their commitments to private investment funds is an important determinant of their returns on those commitments.

7 Conclusion

This paper shows that the growth of private capital markets has implications that extend beyond how companies raise financing and why fewer companies reach public markets. In particular, it also reshapes who gains access to high-growth investment opportunities, with potentially important consequences for the distribution of income and wealth. Our evidence suggests that the increased participation of high-net-worth individuals in private capital markets is a relatively recent development, rather than a longstanding feature of the U.S. financial system. This shift has helped sustain companies' ability to remain private while concentrating a larger share of capital gains among those already positioned at the top of the income and wealth distribution. More broadly, the paper points to a novel channel through which the growth of private capital markets and rising economic inequalities may become mutually reinforcing: unequal access to private asset classes generates unequal returns, and those returns, in turn, support further unequal participation and thus further concentration.

These findings also suggest that the link between private capital markets and inequality may become more salient in the years ahead. If companies continue to stay private for longer, and if access to their equity remains concentrated among wealthy investors, then a growing share of aggregate capital gains may accrue before ordinary households can participate via public stock markets. In that sense, recent developments may still represent only an early stage of a broader structural shift. As a recent Wall Street Journal article on the "invitation-only" private stock markets for the wealthy underscores, access to the most sought-after private share sales remains highly restricted, even as these markets continue to expand and policymakers debate widening access.[43]

Having said that, democratizing private markets does not need to mechanically reduce inequality. Rather, its distributional consequences will depend on whether newly admitted investors gain exposure to the same deals, at the same terms, and with the same net returns as incumbent wealthy investors, or if they are instead channeled into vehicles with different fees, liquidity, governance, and performance characteristics. Pension funds

[43] See https://www.wsj.com/finance/investing/private-stock-market-growth-bb71bde1.

could play a particularly important role in this process, since they may provide a channel through which broader segments of the population can gain indirect exposure to private asset classes. Whether such exposure mitigates or reinforces inequality, however, will depend on the returns ultimately passed through to households.

Our analysis also highlights important data limitations. Private capital market activity remains difficult to systematically observe in the U.S. There is no comprehensive administrative source covering the full universe of private investments and companies, and commercial datasets require a range of imputations for missing investor amounts and company valuations. While the consistency of our results across sources is reassuring, better data on the universe of private market transactions would greatly improve our ability to measure the magnitude of these forces. More comprehensive information on HNWIs' overall portfolios would be especially valuable, since understanding how private investments interact with investors' broader holdings is essential for assessing the full contribution of private capital markets to income and wealth concentration.

Future research could build on these findings in at least two directions. First, extending the analysis beyond the U.S. would help establish how differences in financial structure, tax regimes, pension systems, and the organization of private capital markets shape the relationship between the growth of those markets and the rise in economic inequalities. Second, a general-equilibrium framework could capture mechanisms that lie beyond the scope of this paper, including companies' endogenous financing and listing decisions, equilibrium effects on asset prices and employment, changes in the allocation of savings across asset classes, and the resulting implications for the distribution of income and wealth. We hope that these questions will stimulate further research on the broader economic and distributional consequences of private capital market growth.

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ONLINE APPENDIX

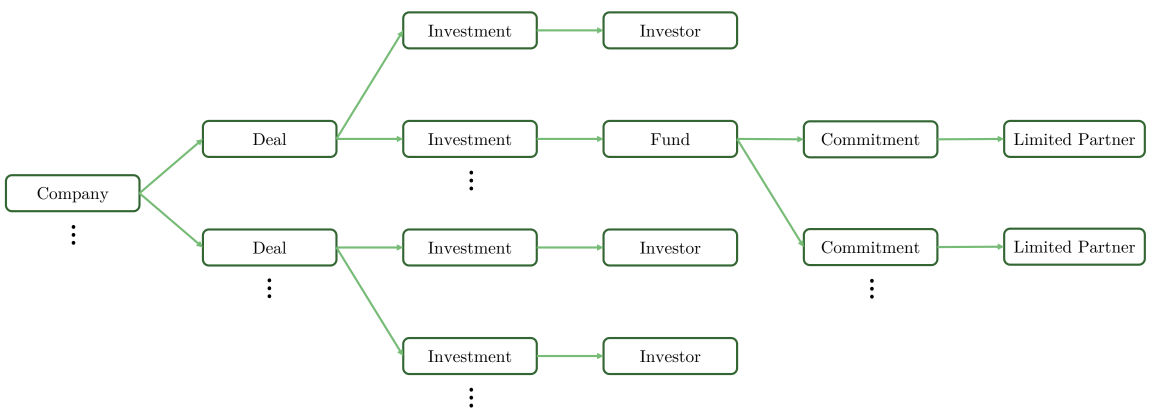
Appendix A: Private Capital Market Activity

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

A.1 Structure of the Data

The data from PitchBook 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 an investor was a fund investing 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 from PitchBook on Private Capital Market Activity



Sources: PitchBook.
Notes: This figure illustrates the structure of the data from PitchBook on private capital market activity. 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. Both versions are missing PitchBook's data on companies that are two years old or older but that have never raised financing. When a company appears in both of our versions of the data, we consider its information only from the more recent version.

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 completed or that are not yet completed. We also drop a further 224,751 (9.5%) with unknown completion dates, since our analyses depend on the timing of deals. 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 investing on behalf of their 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 gives us a total of 7,835,220 (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 (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). 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, lenders, and credit facilities.

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 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 general partner stakes. Debt deals refer to loans, bonds, convertible debt, and mezzanine financing. 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, 44,974 (2.1%) as real asset deals, and 683,998 (32.6%) as other deals.

A.2.2 Lender and Facility 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 credit 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 (HNWIs) and institutional investors. HNWIs refer to individuals, angel groups, and family offices. 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 lists.

A.3.1 High-Net-Worth Individuals

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

A.3.2 Founders

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

A.3.3 Forbes 400

We directly observe the names of individuals and family offices in the data from PitchBook. By comparing these names to the names of the individuals ranked in the Forbes 400 rich lists, as well as the names of these individuals' family offices (if publicly known), we match HNWIs across the two datasets. Since multiple individuals ranked in the Forbes 400 rich lists may invest through—and therefore be matched to—the same family office in the data from PitchBook, our matching procedure increases the number of investments by U.S. HNWIs by 1,123. Of the 168,664 investments made by U.S. HNWIs, 8,310 (4.9%) were made by a U.S. HNWI ranked in the Forbes 400 rich list in the year of the investment.

A.4 Calculation of Amounts Invested

To calculate the amount invested by each investor as part of each investment, 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 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 as part of each deal as the product of these shares and 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%) deals, the amount of equity financing for 575,530 (27.4%) deals, and the amount of debt financing for 173,446 (8.3%) deals. We use the accounting identity that total financing equals the sum of equity and debt financing to impute the third amount whenever we observe the other two. We observe all three amounts for only 22,012 (1.0%) deals, of which 21,049 (95.6%) satisfy this identity. When we observe only one—or none—of these deal-level amounts, we additionally draw on investment-level data on the amount raised from each investor. For example, if we observe only the total amount of financing at the deal level, we impute the amount of debt financing as the sum of the debt raised from investors and the amount of equity financing as the difference between the total amount and this sum. Using this approach, we impute the amount of equity financing 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 and Limited Partner Shares

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 Amount Invested

For direct investments, we calculate the amount invested as part of each investment as the product of the size of each deal and 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.

A.5 Preqin

We follow an analogous 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 we do not observe such investments in our version of the data from Preqin (updated as of July 24, 2025).

Table A1 confirms PitchBook's claim that it has better coverage of private capital market activity than Preqin (see https://www.pitchbook.com/compare/pitchbook-vs-preqin). In terms of the total amounts of early-stage and private equity financing raised by U.S. companies from 2004 to 2022, the amounts captured by PitchBook are only moderately larger than those captured by Preqin. However, in terms of the numbers of early-stage and private equity investments made by U.S. high-net-worth individuals in U.S. companies, the numbers captured by PitchBook are an order of magnitude larger than those captured by Preqin. Thus, our focus on high-net-worth individuals' increasing participation in private capital markets requires us to use the data from PitchBook over that from Preqin.

Table A1: PitchBook's vs. Preqin's Coverage of U.S. 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
	(1)	(2)	(3)	(4)
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 coverage of early-stage and private equity investments in U.S. companies from 2004 to 2022. Using the data from PitchBook, we define high-net-worth individuals as individuals, angel groups, and family offices. Using the data from Preqin, we instead define high-net-worth individuals only as family offices, since this data does not list individuals or angel groups as investor types. The values in Columns (1) and (2) are expressed in nominal terms.

A.6 Additional Results

Table A2: U.S. HNWIs' Early-Stage Investments in U.S. Companies (2004-2022)

	Investments			
	Count		Amount Invested	
	Number	Share	Billions of $	Share
	(1)	(2)	(3)	(4)
Total	105,542	1.000	112.88	1.000
incl. direct	86,249	0.817	107.17	0.949
incl. intermediated	19,293	0.183	5.71	0.051
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. 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. founders	2,464	0.023	3.64	0.032
incl. non-founders	103,078	0.977	109.24	0.968
incl. Forbes 400	5,278	0.050	14.36	0.127
incl. non-Forbes 400	100,264	0.950	98.52	0.873
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

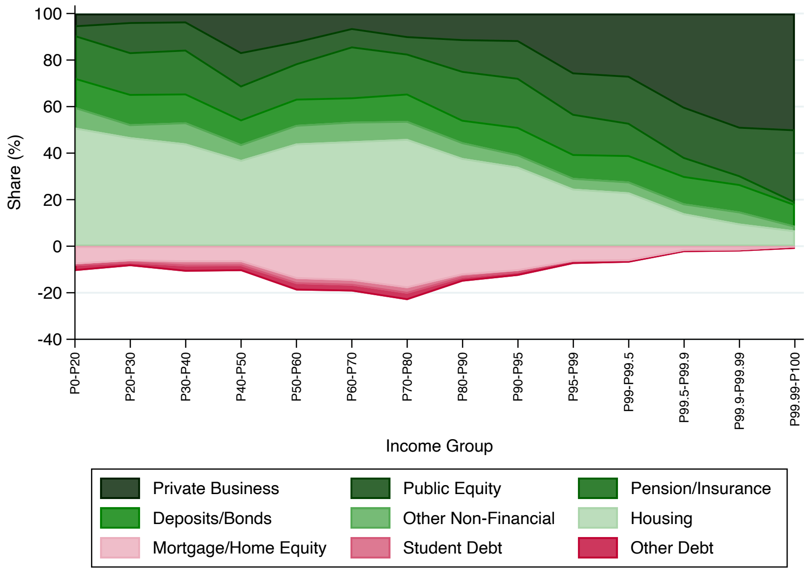
Sources: PitchBook.
Notes: This table summarizes U.S. high-net-worth individuals' early-stage investments in U.S. companies from 2004 to 2022. The values in Column (3) are expressed in nominal terms.

Table A3: U.S. Institutions' Early-Stage Investments in U.S. Companies (2004-2022)

	Investments			
	Count		Amount Invested	
	Number	Share	Billions of $	Share
	(1)	(2)	(3)	(4)
Total	1,095,641	1.000	1228.49	1.000
incl. direct	334,333	0.305	905.20	0.737
incl. intermediated	761,308	0.695	323.29	0.263
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. 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. other institutions	907	0.001	1.35	0.001
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	0.030	51.10	0.042

Sources: PitchBook.
Notes: This table summarizes U.S. institutional investors' early-stage investments in U.S. companies from 2004 to 2022. The values in Column (3) are expressed in nominal terms.

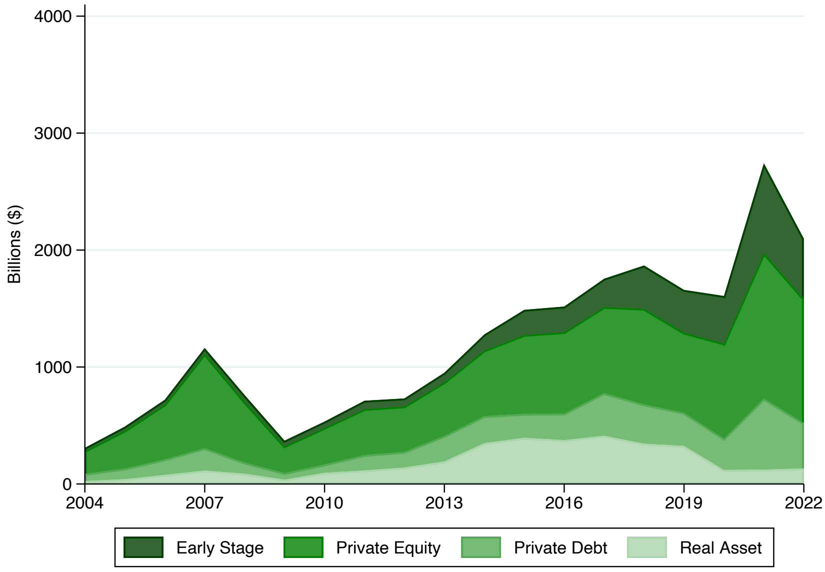
Figure A2: Asset Composition of Net Wealth along the U.S. Income Distribution (2022)



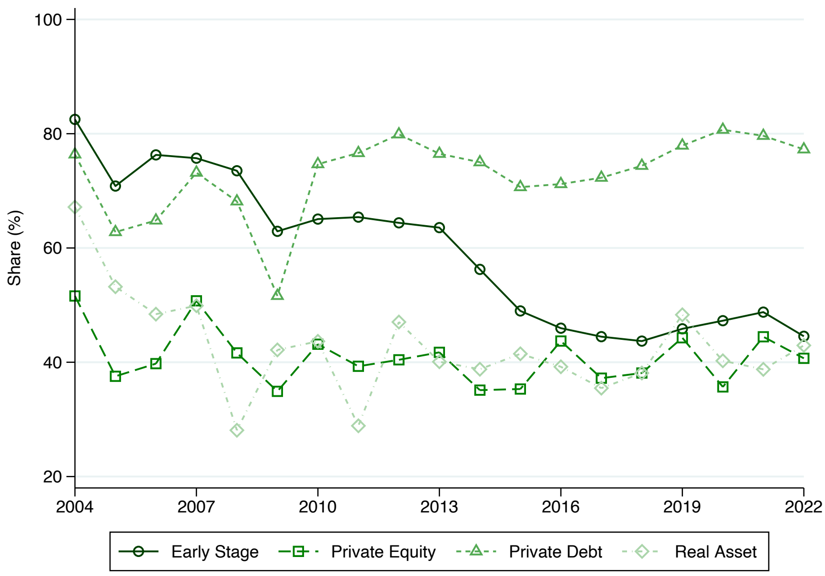
Sources: Survey of Consumer Finances.
Notes: This figure describes the asset composition of net wealth along the U.S. income distribution in 2022, based on household-level information from the 2022 wave of the SCF.

Figure A3: Investments in Global Private Capital Market Deals

(a) Investments in All Companies

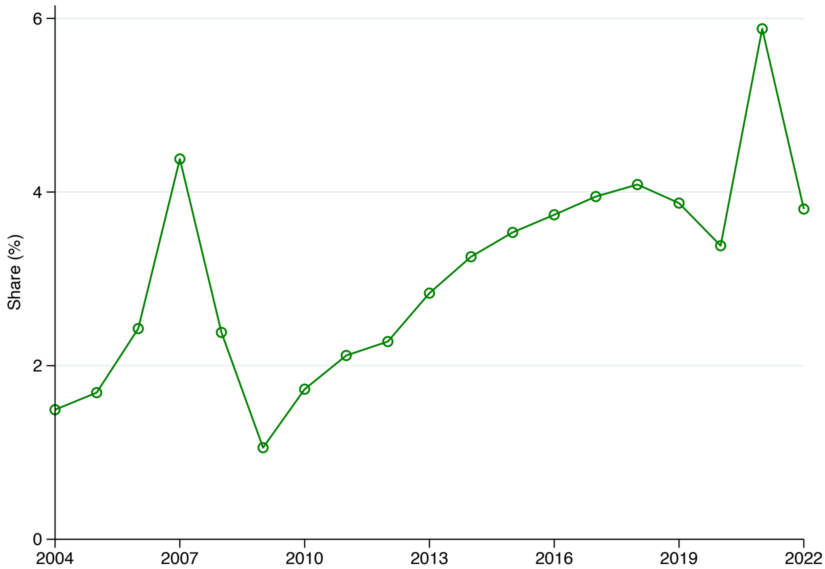


(b) U.S. Companies' Share



Sources: PitchBook.
Notes: This figure describes the evolution of investments in private capital market deals for all companies globally from 2004 to 2022. Panel (a) plots the investments. Panel (b) plots the share of these investments that were invested in U.S. companies. The values in Panel (a) are expressed in nominal terms.

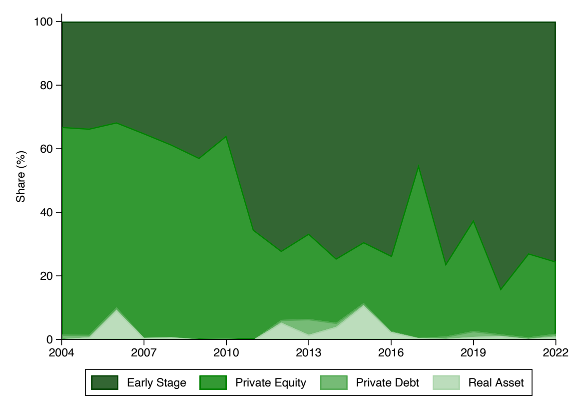
Figure A4: Investments in U.S. Private Capital Market Deals as a Share of U.S. GDP



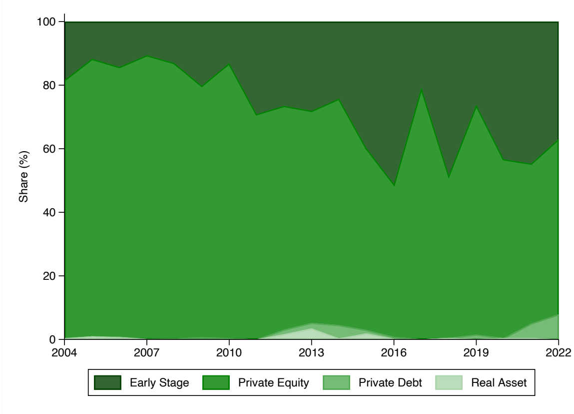
Sources: PitchBook, Bureau of Economic Analysis.
Notes: This figure describes the evolution of investments in private capital market deals for U.S. companies as a share of U.S. gross domestic product from 2004 to 2022.

Figure A5: Private Asset Composition of U.S. HNWIs' Investments

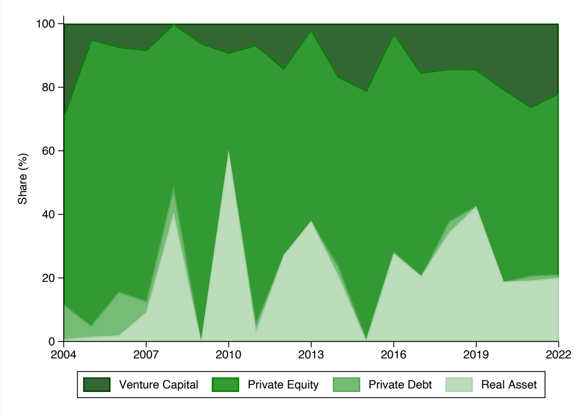
(a) All Investments



(b) Intermediated Investments



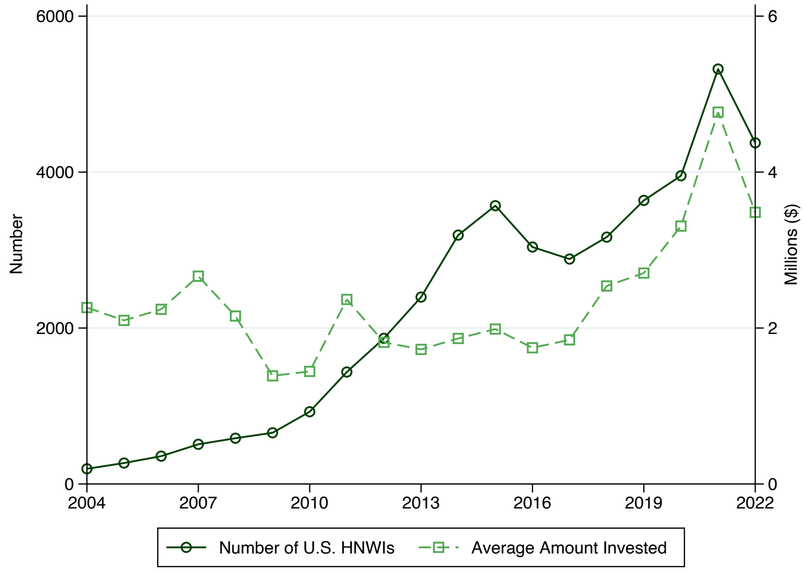
(c) Fund Commitments



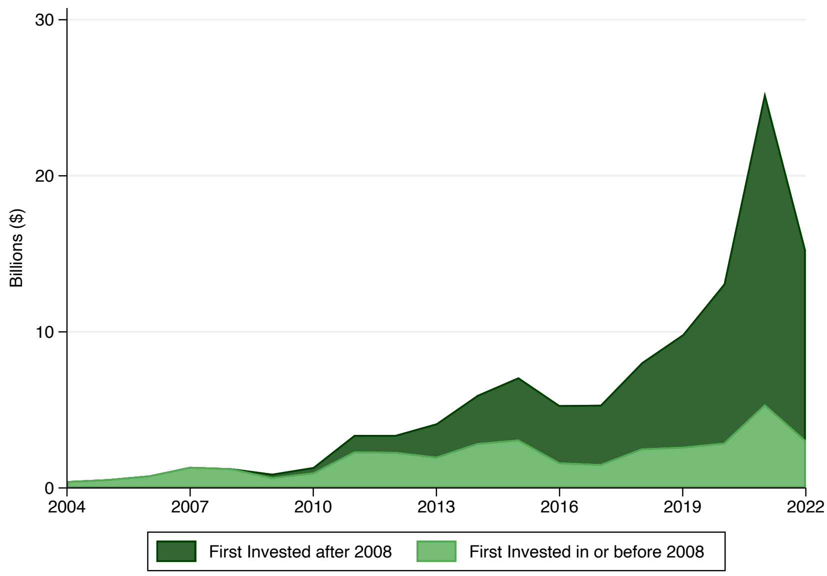
Sources: PitchBook.
Notes: This figure describes the evolution of the composition of U.S. high-net-worth individuals' investments across private asset classes. Panel (a) plots all investments in U.S. companies. Panel (b) plots only intermediated investments in U.S. companies. Panel (c) plots fund commitments. The deal categories in Panels (a)-(b) are those described in Appendix Section A.2. The fund categories in Panel (c) are based on PitchBook's own classification of fund types.

Figure A6: U.S. HNWIs' Early-Stage Investments in U.S. Companies

(a) Number of Investors vs. Average Amount Invested

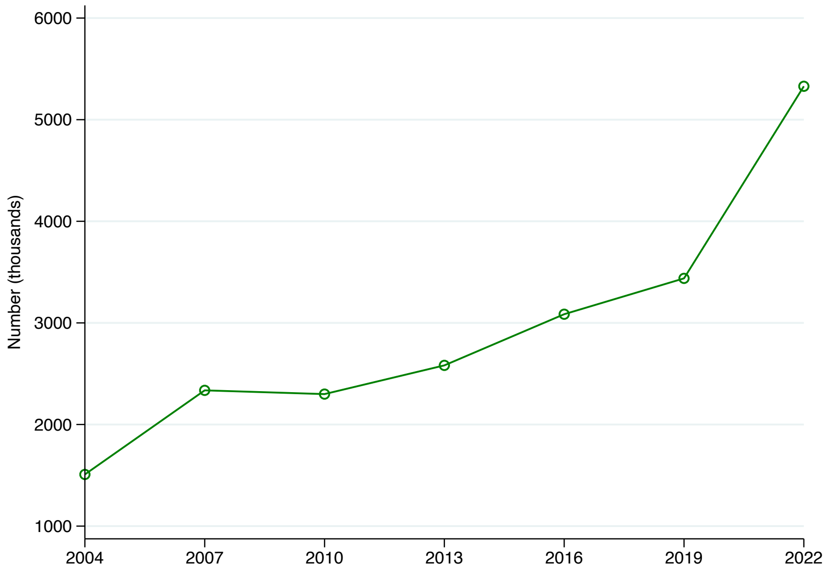


(b) Total Amounts Invested by New vs. Incumbent Investors



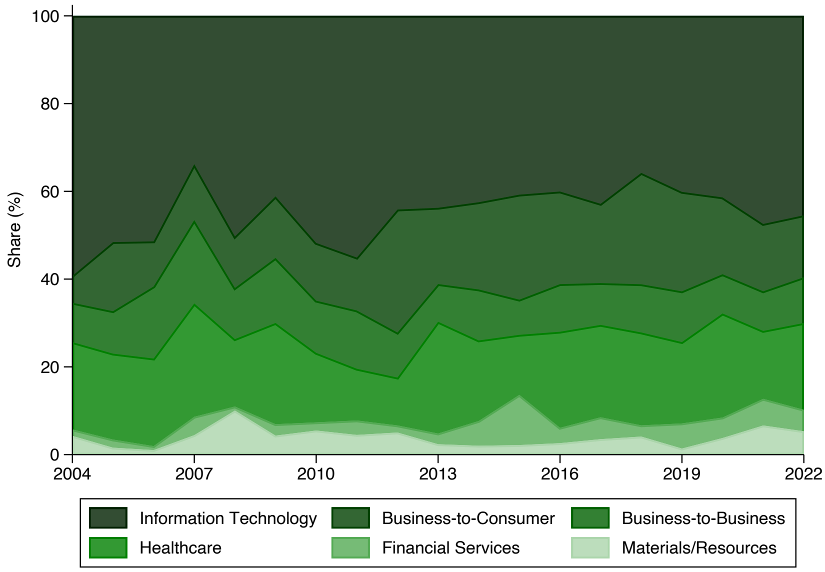
Sources: PitchBook.
Notes: This figure describes the evolution of U.S. high-net-worth individuals' early-stage investments in U.S. companies from 2004 to 2022. Panel (a) plots the number of U.S. HNWIs investing in U.S. early-stage companies and their average amount invested. Panel (b) plots the decomposition of U.S. HNWIs' total amount invested in U.S. early-stage companies into the total amounts invested by those who first invested after 2008 and those who first invested in or before 2008, respectively. The values plotted on the right axis in Panel (a) and those plotted in Panel (b) are expressed in nominal terms.

Figure A7: 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 reported having private business wealth from 2004 to 2022, based on household-level information from the 2004-2022 waves of the SCF. We define accredited investors as households whose net wealth (excluding the value of their primary residence) exceeded $1 million, married households whose combined income exceeded $300,000, and single households whose individual income exceeded $200,000. We define private business wealth as wealth in the form of partnerships or other private corporations (e.g., C corporations).

Figure A8: Sectoral Composition of U.S. HNWIs' Early-Stage Investments



Sources: PitchBook.
Notes: This figure describes the evolution of the sectoral composition of U.S. high-net-worth individuals' early-stage investments in U.S. companies from 2004 to 2022.

Appendix B: Return Methodology

In this appendix, we describe our procedure to clean the data from PitchBook on companies' valuations as well as our methodology to calculate returns. Section B.1 details how we clean the data on observed valuations and bankruptcies, impute valuations when they are missing, and construct alternative valuation samples to verify the robustness of our analyses. Section B.2 explains how we calculate returns. Section B.3 reports additional results related to high-net-worth individuals' returns on their early-stage investments.

B.1 Valuations

To be able to calculate the return on each investment in each company in each year, we first construct the history of the rate of return on the company's equity using information on its changing valuation—whether observed or imputed—across deals.

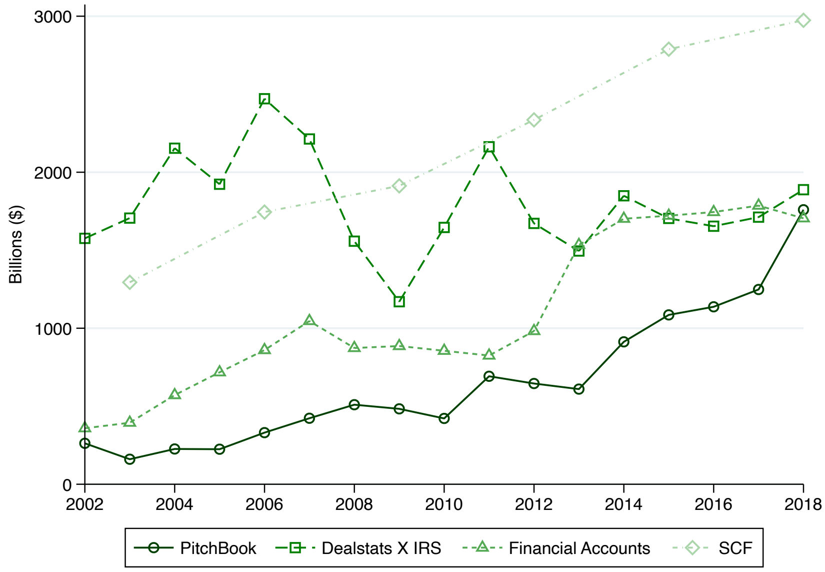
B.1.1 Observed Valuations and Bankruptcies

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

Despite our main analysis spanning the period from 2004 to 2022, information on companies' valuations after 2022 can still be informative about their valuations in or before 2022. We therefore focus on the 323,987 deals from 2004 to 2024 for U.S. companies that ever raised early-stage financing between 2004 and 2022 and whose investors had not yet exited as part of a bankruptcy, public offering, or acquisition. Of these, we obtain the company's pre-money valuation from 91,828 (28.3%) deals and its post-money valuation from 97,826 (30.2%) deals. Among the 98,045 deals with information on the company's pre-money valuation or post-money valuation, 6,436 (6.6%) have information on only one of these two valuations. In these cases, we calculate the other valuation using the identity that the amount of financing raised by the company as part of the deal equals the difference between its post-money and pre-money valuations. For this calculation, we consider only the equity financing raised by the company as part of deals that we would expect to have increased the assets on its balance sheet. For example, this excludes buyout deals.

Finally, 18,691 (5.8%) of the deals identify when the company went bankrupt without specifying its valuation after the bankruptcy. In these cases, we follow Korteweg and

Figure B1: Private Business Wealth in the U.S.



Sources: PitchBook; Campbell and Robbins (2025).
Notes: This figure compares the evolution of private business wealth in the U.S. from 2002 to 2018 across alternative sources. The PitchBook series is based on the observed valuations and bankruptcies of active private C corporations (as defined in Appendix Section C.1) with at least one observed valuation, which is the type of private business wealth for which information is available from the other sources. We extrapolate forward each company's valuation either until its next valuation or until it first went bankrupt, became publicly listed, or was acquired. The Dealstats X IRS, Financial Accounts, and Survey of Consumer Finances (SCF) series are from Campbell and Robbins (2025). According to the authors, the difference in trends between their estimates of the Dealstats X IRS series and the Financial Accounts/SCF series 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 values are expressed in nominal terms.

Sorensen (2010) and assume that the company's valuation fell to 10% of its previous post-money valuation. This gives us a total of 116,736 observed valuations and bankruptcies.

To validate PitchBook's data on companies' valuations, Appendix Figure B1 compares the evolution of private business wealth in the U.S. based on PitchBook to its evolution based on alternative sources. To focus on companies similar to those considered in these other sources, we consider only the observed valuations and bankruptcies of active private C corporations (as defined in Appendix Section C.1) with at least one observed valuation. Reassuringly, the trends based on PitchBook resemble those based on the other sources.

B.1.2 Imputed Valuations and Selection Bias

To construct the history of the rate of return on each company's equity, we would ideally observe the company's valuation in all years. Yet, not only are valuations missing from many of the deals that we observe, but also companies do not raise financing in every year—meaning that PitchBook cannot update their recorded valuations at an annual frequency. We overcome these challenges by imputing missing valuations, based on the predicted values from sector-specific regressions of the log valuations of each company on company and interacted company stage-year fixed effects. Since better-performing companies are more likely to raise financing—and therefore have their recorded valuations updated by PitchBook—we treat companies that raised financing and those that did not as two separate cases. In the first case, we estimate the imputation regressions only on observed valuations, as the valuations of companies that raised financing are more likely to reflect those of other companies that also raised financing. In the second case, we instead estimate the imputation regressions only on valuations obtained from the dynamic selection model developed by Korteweg and Sorensen (2010), as the valuations of companies that did not raise financing are more likely to reflect those of other companies that also failed to do so. Since the valuations obtained from this dynamic selection model account for selection bias as an intermediate step, the predicted values from the imputation regressions estimated on these valuations in the final step of our imputation procedure also account for this same bias. We now provide details on Korteweg and Sorensen (2010)'s dynamic selection model, our estimation of it, and our imputation regressions.

Dynamic Selection Model. Korteweg and Sorensen (2010) attempt to account for the fact that a company's true valuation is likely to be higher in the years in which it is observed than in the years in which it is not. The authors argue that a "dynamic selection problem arises because valuations of [early-stage] companies are observed only when the companies receive funding or have exit events," which "are more frequent for well-performing companies" that "are more likely to subsequently survive." To overcome this problem, they develop a dynamic selection model that is jointly characterized by a valuation equation and a selection equation, the former of which imposes the following structure on how a company's valuation evolves over time:

$$v_{i,t} = v_{i,t-1} + r_t + \beta(r_t^m - r_t) + \delta + \epsilon_{i,t} \tag{B1}$$

Equation (B1) states that company i's log valuation $v_{i,t}$ in period t equals the sum of its log valuation $v_{i,t-1}$ in period $t-1$, the log risk-free rate r_t, the excess log return of the market $r_t^m - r_t$ multiplied by a measure of systematic risk β, a constant term δ, and an error term $\epsilon_{i,t}$. In other words, the excess change in the company's valuation $r_{i,t} - r_t := (v_{i,t} - v_{i,t-1}) - r_t$ is an affine function of the excess return of the market plus an idiosyncratic shock. The following selection equation then introduces a latent selection variable $w_{i,t}$ whose value must be positive (i.e., $w_{i,t} > 0$) for us to actually observe $v_{i,t}$:

$$w_{i,t} = \gamma_v v_{i,t} + \gamma_0' z_{i,t} + \eta_{i,t} \tag{B2}$$

Equation (B2) states that the latent selection variable is a linear function of company

i's log valuation $v_{i,t}$ and a vector of observable characteristics $z_{i,t}$ plus an idiosyncratic shock $\eta_{i,t}$. In $z_{i,t}$, Korteweg and Sorensen (2010) include the company's previous log valuation $v_{i,t-1}$ and a quadratic function of the number of periods since its valuation was last observed. The authors further assume that the coefficient on $v_{i,t-1}$ is γ_v, matching that on $v_{i,t}$, such that selection is ultimately determined by the change in the company's valuation $r_{i,t}$. Positive selection would then imply $\gamma_v > 0$.

To verify this positive selection and correct for it, Korteweg and Sorensen (2010) jointly estimate Equations (B1) and (B2) using a Bayesian Gibbs sampling procedure under the additional assumptions $\epsilon_{i,t} \sim N(0, \sigma^2)$ and $\eta_{i,t} \sim N(0, 1)$. This procedure involves five steps. First, initialize the model's parameters and their prior distributions. Second, use a forward-filtering backward-sampling algorithm to simulate the path of each company's missing valuations. Third, simulate the path of the company's latent selection variables. Fourth, use Bayesian linear regressions to sample the parameters from their posterior distributions. Finally, iterate the second through fourth steps until the estimated parameters have converged. Ultimately, the procedure yields bias-corrected estimates of not only β, δ, σ, γ_v, and γ_0 but also the negatively selected values of $v_{i,t}$ that are missing.

Our key insight is that we can use these valuations obtained from the dynamic selection model in our procedure to impute missing valuations. When we estimate our regressions on observed valuations to impute missing valuations for companies in the years in which they raised financing, we assume that the missing valuations in this first case are positively selected in the same way as the observed valuations. When we instead estimate our regressions on valuations obtained from the dynamic selection model to impute missing valuations for companies in the years in which they did not raise financing, we assume that the missing valuations in this second case are negatively selected in the same way as the valuations obtained from the model. By separately treating these two cases, our imputation procedure therefore accounts for selection bias.[44]

Estimation of the Dynamic Selection Model. We implement the Bayesian Gibbs sampling procedure developed by Korteweg and Sorensen (2010) to estimate their dynamic selection model using PitchBook's data on companies' valuations, as well as data on market returns (see https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html). From PitchBook, we use the observed valuations and bankruptcies of U.S. companies for which we observe valuations in at least two different months from 2004 to

[44] There are two reasons why we do not simply use the valuations obtained from the dynamic selection model as imputed valuations. First, doing so would require assuming that the valuations missing in years in which companies raised financing were negatively selected in the same way as the valuations missing in the years in which they did not raise financing, which we think would be too conservative. Second, the dynamic selection model yields estimates of missing valuations for only the companies whose valuations are observed at least twice, which is only a subset of all the companies whose valuations we are missing.

2024, jointly estimating Equations (B1) and (B2) at the monthly level from 2004 to 2024.

To allow for potential heterogeneity in selection bias, we estimate Equations (B1) and (B2) separately for four different sectors: business-to-business/business-to-consumer products and services, business/financial software, other information technology, and all other industries (including financial services, healthcare, and materials/resources). We define these sectors in this way so that they all end up being of approximately equal size.

When simulating the path of company i's missing valuations, as well as that of its latent selection variables, we consider the period from the first month $\underline{t}_i$ in which we observe its valuation to the last month $\bar{t}_i$ in which its investors exited their early-stage investments in the company as part of a bankruptcy, public offering, or acquisition. If we observe no such exit event, then we set $\bar{t}_i$ equal to December 2024. After initializing $v_{i,t} = \ln(1) = 0$ at $t = \underline{t}_i$, we iteratively update $v_{i,t'}$ as of the company's next observed valuation or bankruptcy at $t' > t$ (i.e., until $t = \bar{t}_i$). Specifically, we add to $v_{i,t}$ the log ratio of the company's new pre-money valuation $V_{i,t'}^{Pre}$ to its previous post-money valuation $V_{i,t}^{Post}$:

$$v_{i,t'} \coloneqq v_{i,t} + \ln\left(\frac{V_{i,t'}^{Pre}}{V_{i,t}^{Post}}\right) \tag{B3}$$

Equation (B3) defines company i's valuation in a way that accounts for any dilution of its incumbent investors' shares between the deals at t and t', respectively. Thus, it isolates the component of the growth in the company's valuation attributable to organic growth.

Table B1 reports our estimates of β, δ, σ, γ_v, and γ_0 based on 200 iterations of the Bayesian Gibbs sampling procedure, after we discard 1,000 initial burn-in iterations that prove sufficient for convergence. For every sector, our estimate of γ_v is positive and statistically significant, indicating positive selection. To obtain bias-corrected estimates of companies' missing valuations, we calculate the mean across iterations of each company's simulated log valuation in each month and exponentiate it. The resulting monthly valuations are more than we need to calculate the annual rate of return on each company's equity. Thus, as the valuations obtained from the dynamic selection model on which we estimate our imputation regressions, we keep only those for each month in which the company raised financing and for the last month of each year in which it did not raise financing.

Imputation Regressions. Given either observed valuations or the valuations obtained from the dynamic selection model, we regress company i's log dilution-adjusted valuation at date t on interacted company stage s-year y fixed effects and company fixed effects:

$$v_{i,t} = \beta_{s(i,t),y(t)} + \alpha_i + u_{i,t} \tag{B4}$$

Table B1: Estimates of the Dynamic Selection Model (2004-2024)

	Sectors			
	B2B/B2C	B/F Software	Other IT	Other
Parameter	(1)	(2)	(3)	(4)
β	1.0358***	1.1172***	0.9961***	1.0165***
	(0.0879)	(0.0526)	(0.0575)	(0.0403)
δ	-0.0183***	-0.0109***	-0.0155***	-0.0104***
	(0.0011)	(0.0009)	(0.0008)	(0.0005)
σ	0.3603***	0.3508***	0.3580***	0.3113***
	(0.0020)	(0.0030)	(0.0012)	(0.0013)
γ_v	0.3526***	0.4263***	0.3708***	0.3475***
	(0.0041)	(0.0062)	(0.0060)	(0.0040)
$\gamma_{0,0}$	-2.1232***	-2.2387***	-2.1834***	-2.1884***
	(0.0071)	(0.0099)	(0.0090)	(0.0064)
$\gamma_{0,1}$	0.0210***	0.0263***	0.0274***	0.0218***
	(0.0004)	(0.0008)	(0.0005)	(0.0004)
$\gamma_{0,2}$	-0.0002***	-0.0002***	-0.0002***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
No. observations	19,445	20,836	17,104	22,843
No. companies	6,003	6,321	5,271	6,837

Sources: PitchBook; https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.
Notes: This table reports our estimates of the parameters of the dynamic selection model jointly characterized by Equations (B1) and (B2). We estimate the model at the monthly level from 2004 to 2024 separately for four different sectors: business-to-business and business-to-consumer products and services (B2B/B2C), business and financial software (B/F Software), other information technology (Other IT), and all other industries (Other). β refers to the coefficient on the excess log return of the market in the valuation equation. δ refers to the intercept of the valuation equation. σ refers to the standard deviation of the error term in the valuation equation. γ_v refers to the coefficient on the excess log change in the company's valuation in the selection equation. $\gamma_{0,0}$, $\gamma_{0,1}$, and $\gamma_{0,2}$ refer to the coefficients on the constant, linear, and squared terms, respectively, of the quadratic function of the number of periods since the company's last observed valuation in the selection equation. Observations refer to the observed valuations and bankruptcies of U.S. companies for which we observe valuations in at least two different months from 2004 to 2024. The estimate of each parameter and its standard deviation are based on its mean and variance across 200 iterations of the Bayesian Gibbs sampling procedure developed by Korteweg and Sorensen (2010), after we discard 1,000 initial burn-in iterations. ***, **, and * denote statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Since there are too few valuations in 2004 and 2024 for certain stages, we group together 2004 with 2005 and 2024 with 2023 for all stages when estimating Equation (B4).

We classify companies into six stages of maturity based on the number of times that they raised early-stage financing (other than in the form of grants) as of the end of date t: zero or one, two, three, four, five, and six or more times. Separately for each of the four sectors (i.e., business-to-business/business-to-consumer products and services, business/financial software, other information technology, and all other industries) for which we estimated the dynamic selection model, we estimate Equation (B4) twice, first using only observed valuations and then using only the valuations obtained from the dynamic selection model.

Based on the resulting estimates, we predict the log valuation $\hat{v}_{i,t} := \hat{\beta}_{s(i,t),y(t)}$ of company i as of each deal and as of December 31 of each year in which it did not raise financing. To account for the company's observed valuations, we adjust this prediction by adding $\hat{\alpha}_{i,t'(i,t)} := v_{i,t'(i,t)} - \hat{v}_{i,t'(i,t)}$ to it, where $t'(i,t)$ denotes the date closest to t as of which we can compute this adjustment for company i. We then impute the change in i's valuation as of each t as the ratio of $\exp(\hat{v}_{i,t} + \hat{\alpha}_{i,t'(i,t)})$ to its previous value. Thus, we construct two series of the imputed change in the company's valuation as of each date: an optimistic series based on the positively selected observed valuations, and a pessimistic series based on the negatively selected valuations obtained from the dynamic selection model.

The final step of our imputation procedure is to decide, as of each date, from which series to take the imputed change in the company's valuation. To illustrate our decision rule, assume for simplicity that we observe the company's valuation at date t but not at the next date t'. If the company raised financing at t', then we take the imputed change in its valuation at t' from the optimistic series. If instead the company did not raise financing at t', then we take the imputed change in its valuation at t' from the pessimistic series. Given whichever imputed change in the company's valuation, we then impute its valuation at t' as the product of its valuation at t and the imputed change. We iterate this procedure first forward and then backward to impute the company's valuation in every year from its year of founding to the year in which its investors exited their investments in the company. In this way, our imputation procedure accounts for selection bias while still allowing us to either observe or impute at least two valuations for 85,738 companies, almost four times as many as the 24,485 companies for which we only observe at least two valuations.

B.1.3 Alternative Valuation Samples

Our baseline valuation sample consists of 828,321 total valuations, of which 98,045 (11.8%) are observed valuations, 18,691 (2.3%) are observed bankruptcies, and 711,585 (85.9%) are valuations imputed based on our baseline imputation procedure. To verify the robustness of our analysis, we also consider the following alternative valuation samples.

Observed Valuations and Bankruptcies Only. As a benchmark against which to analyze the effects of our imputation procedure on our analysis, we consider an alternative valuation sample consisting of only the observed valuations and bankruptcies of companies with at least two observed valuations. We set this threshold at two because it is the minimum number of observed valuations necessary to be able to construct the history of the rate of return on a company's equity based on only its observed valuations.

Optimistic and Pessimistic Variants of the Imputation Procedure. Our baseline imputation procedure features a rule to decide between imputing a company's missing valuation as of each date based on only positively selected observed valuations (if the company raised financing as of that date) and doing so based on only negatively selected valuations obtained from the dynamic selection model (if the company did not raise financing in the year of that date). As variants of our imputation procedure, we consider alternative valuation samples in which we impute all missing valuations using either only observed valuations or the valuations obtained from the dynamic selection model.

Imputed Bankruptcies. To account for the potential incompleteness of PitchBook's data on companies' bankruptcies, we consider an alternative valuation sample in which we additionally impute bankruptcies for companies that appear inactive. If a company last raised financing in or before 2022, then we assume that it went bankrupt one year after its last deal (i.e., as long as that deal was not already an exit event). If our baseline imputation procedure already sufficiently adjusts such companies' valuations downward, then adding imputed bankruptcies is not likely to have a major effect on our analysis.

Haircut Valuations. We also account for "unicorn" valuations that exceed $1 billion but are likely overvalued, as documented by Gornall and Strebulaev (2020) and Gahng (2023). The latter author estimates that the average unicorn is overvalued by 56%, which accounts for the overstatement of both its price per share (since the value of preferred shares likely exceeds that of common shares) and its number of shares outstanding (since this often includes authorized but unissued shares in employee option pools). If a unicorn's true valuation is overstated by 56%, then its true valuation is $\frac{1}{1.56} \approx 64\%$ of its observed valuation. We therefore consider an alternative valuation sample in which we apply a haircut of 36% to all the valuations of each unicorn after it first achieved this status.

B.2 Returns

We use the history of each company's valuations to calculate returns. We first calculate returns at the company level, namely, the history of the rate of return on the company's equity. We then translate this into returns at the investment level, namely, the return on each investment in the company in each year.

B.2.1 Company-Level Returns

For each company i, we calculate the number of days $D_{i,t,t'} := t' - t > 0$ between its valuations as of dates t and t', respectively. We also calculate the percent change from i's previous post-money valuation $V_{i,t}^{Post}$ at t to its new pre-money valuation $V_{i,t'}^{Pre}$ at t':

$$r_{i,t,t'} := \frac{V_{i,t'}^{Pre} - V_{i,t}^{Post}}{V_{i,t}^{Post}} \tag{B5}$$

Assuming that the company's valuation compounds at a constant daily rate between the two dates, we convert $r_{i,t,t'}$ from Equation (B5) into a daily compounded rate for date t:

$$(1 + r_{i,t})^{D_{i,t,t'}} := 1 + r_{i,t,t'} \implies r_{i,t} = -1 + \exp\left[\frac{\ln(1 + r_{i,t,t'})}{D_{i,t,t'}}\right] \tag{B6}$$

Equation (B6) defines the rate of return on each company's equity on every date from its first valuation after December 31, 2003 to its last valuation before January 1, 2025. We also assume $r_{i,t} = 0$ for all dates preceding its first and following its last valuation.

B.2.2 Investment-Level Returns

To calculate the return on each investment in each year, we require additional information on the amounts that investors invested, as well as on the dates on which they entered and exited their investments. We exclude 281 grant investments from this analysis, since investors do not actually earn returns on this type of early-stage investment.

Amounts Invested. Based on the approach described in Appendix Section A.4.3, we calculate the amount invested as part of investment j, $Amount_j$, for 93,618 of the early-stage investments by U.S. high-net-worth individuals in U.S. companies from 2004 to 2022. We exclude 11,643 investments for which we are unable to calculate the amount invested.

Entry Dates. We define the entry date of investment j, $Entry_j$, as the completion date of the deal as part of which the investor made that investment.

Observed Exit Dates. To define the exit date of investment j in company i, $Exit_j$, we use PitchBook's data on the investors exiting their investments in the company as part of each deal. Specifically, we identify $Exit_j$ as the first date (after $Entry_j$) on which the investor that made investment j is recorded as having exited any of their investments in company i. We observe this date for 36,771 (39.3%) of the 93,618 early-stage investments made by U.S. high-net-worth individuals in U.S. companies between 2004 and 2022. For 39,654 investments (42.4%), we also observe that, after company i's final early-stage deal, the company either went bankrupt, became publicly listed, or was acquired. In such cases, we therefore define $Exit_j$ as the earlier of the first such event and the date on which we directly observe that the investor exited any of their investments in the company.

Imputed Exit Dates. For the 49,240 investments (52.6%) for which we do not observe an exit date, we set $Exit_j$ equal to the date five years after $Entry_j$. This imputation is motivated by the fact that the mean time to exit among the 44,378 investments (47.4%) for which we do observe an exit date is 4.7 years, as well as the possibility that PitchBook's data is more complete on investments than on exits. Absent the imputation, we would risk overstating the returns that investors accumulated on investments that may have already exited but for which we do not observe the exit date (see Appendix Figure B6).

Annual Returns. We calculate the rate of return on investment j in year y as:

$$r_{j,y}^{\text{Annual}} := -1 + \prod_{t=\max(1/1/y, Entry_j)}^{\min(12/31/y, Exit_j)} (1 + r_{i(j),t}), \tag{B7}$$

where the product is taken over all the days in year y in which j is held. If either $Entry_j$ or $Exit_j$ occurs during year y, then $r_{j,y}^{\text{Annual}}$ in Equation (B7) is not an annualized rate of return, since the investment was held for only part of the year. To annualize it, we first define the number of days during year y for which investment j was held as:

$$D_{j,y} := \sum_{t=1/1/y}^{12/31/y} 1\{Entry_j \leq t < Exit_j\} \tag{B8}$$

Given Equation (B8), the daily rate of return $r_{j,y}$ is then:

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

Given also the number of days in year y, $D_y \in \{365, 366\}$, the annualized rate is finally:

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

We next calculate returns in U.S. dollars. Given the amount invested as part of investment j ($Amount_j$), the year y_j of its entry date ($Entry_j$), and its annual rate of return ($r_{j,y}^{\text{Annual}}$) from Equation (B7), we calculate the accumulated value of j by the end of year y as:

$$Value_{j,y} := \begin{cases} Amount_j & \text{if } y = y_j - 1 \\ Amount_j \times \prod_{t=y_j}^{y} (1 + r_{j,t}^{\text{Annual}}) & \text{if } y \geq y_j \end{cases} \tag{B11}$$

The return in U.S. dollars on investment j in year y is then:

$$Return_{j,y} := Value_{j,y} - Value_{j,y-1} \tag{B12}$$

Counterfactual Returns. We construct a counterfactual return for investment j by replacing $r_{i(j),t}$ in Equation (B7) with the rate of return on some index, r_t^{Index}. The rate of return that a counterfactual investment in the index would have earned in year y is:

$$r_{j,y}^{\text{Counterfactual}} := -1 + \prod_{t=\max(1/1/y, Entry_j)}^{\min(12/31/y, Exit_j)} (1 + r_t^{\text{Index}}) \tag{B13}$$

Similarly, replacing $r_{j,y}^{\text{Annual}}$ in Equation (B11) with $r_{j,y}^{\text{Counterfactual}}$ from Equation (B13), we calculate the accumulated value of counterfactual investment j by the end of year y as:

$$Value_{j,y}^{\text{Counterfactual}} := \begin{cases} Amount_j & \text{if } y = y_j - 1 \\ Amount_j \times \prod_{t=y_j}^{y} (1 + r_{j,t}^{\text{Counterfactual}}) & \text{if } y \geq y_j \end{cases} \tag{B14}$$

The counterfactual return in U.S. dollars in year y is then:

$$Return_{j,y}^{\text{Counterfactual}} := Value_{j,y}^{\text{Counterfactual}} - Value_{j,y-1}^{\text{Counterfactual}} \tag{B15}$$

B.2.3 Average Returns

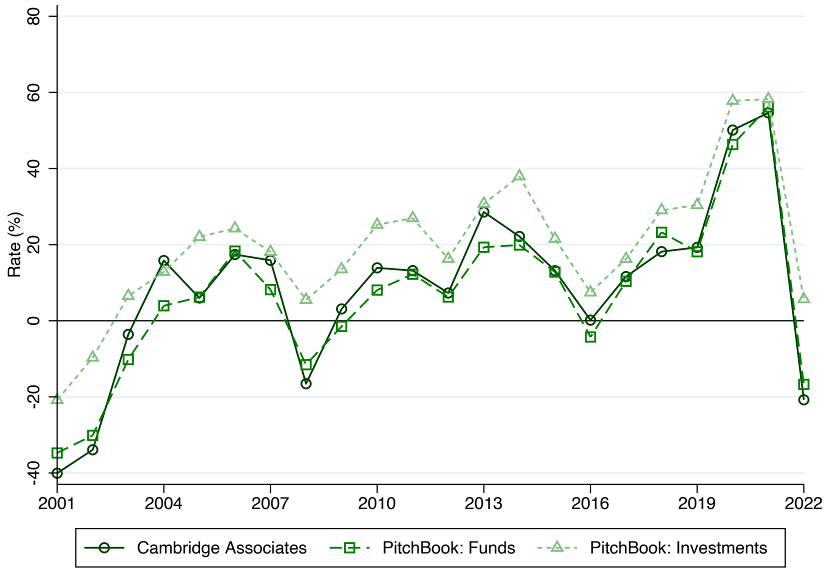
We finally aggregate investment-level returns to characterize average returns. Specifically, given Equations (B11) and (B12), we calculate the ratio of the total returns on the investments during year y to their total accumulated value as of the start of y:

$$r_y^{TVPI} := \frac{\sum_j Return_{j,y}}{\sum_j Value_{j,y-1}} \tag{B16}$$

r_y^{TVPI} in Equation (B16) equals the total value to paid-in capital ratio (TVPI) over a 1-year horizon, minus one. Since TVPI is not an annualized measure, we also calculate an average annualized rate of return that assigns an appropriate weight to each investment based on both its net asset value (NAV) at the start of each year and the number of days for which the investor held it during the year. Given the first day $First_{j,y} := \max\{0, Entry_j - 1/1/y\}$ of year y on which the investor held investment j, we calculate the (NAV-to-NAV) internal rate of return (IRR) over a 1-year horizon as described by Phalippou (2025):

$$\sum_j \left(-\frac{Value_{j,y-1}}{(1+r_y)^{First_{j,y}}} + \frac{Value_{j,y}}{(1+r_y)^{First_{j,y}+D_{j,y}}} \right) = 0 \implies r_y^{IRR} := -1 + (1+r_y)^{D_y} \tag{B17}$$

Figure B2: 1-Year Internal Rate of Return on U.S. Venture Capital Funds



Sources: PitchBook, Cambridge Associates.
Notes: This figure compares the 1-year NAV-to-NAV internal rate of return (Phalippou, 2025) on U.S. venture capital funds from 2001 to 2022 across alternative sources. The Cambridge Associates series is based on its U.S. Venture Capital Index. The fund-level PitchBook series is based on the quarterly cash flows and net asset values (NAV) of funds. The investment-level PitchBook series is based on the early-stage investments intermediated by U.S. venture capital funds from 1998 to 2022, considering only the observed valuations and bankruptcies from 1998 to 2024 of companies with at least two observed valuations, as well as only observed exits.

Substituting Equations (B14) and (B15) into Equations (B16) and (B17), we also calculate the average annual rates of return on the counterfactual investments in the index.

To validate our return methodology, Appendix Figure B2 compares the 1-year IRR on U.S. venture capital funds based on PitchBook to that based on the Cambridge Associates U.S. Venture Capital Index. We reproduce the Cambridge Associates series using PitchBook's data on funds' quarterly cash flows and NAVs. We then apply our return methodology to the early-stage investments intermediated by funds from 1998 to 2022, based on only observed valuations and bankruptcies from 1998 to 2024, as well as only observed exits. Reassuringly, our investment-level measure (which is gross of fees) closely tracks but always exceeds the reproduced Cambridge Associates series (which is net of fees).

B.3 Additional Results

Table B2: Risk-Adjusted Returns (1987-2005): Korteweg and Sorensen (2010)

		One-Factor Model			
		OLS	GLS	DSM	
Parameter	Period	(1)	(2)	(3)	(4)
β	1987-2005	2.0766*** (0.1003)	2.2906*** (0.1166)	2.7510*** (0.1127)	
	1987-1993				0.3814 (0.6710)
	1994-2000				2.5005*** (0.2047)
	2001-2005				1.0855*** (0.1837)
α	1987-2005		0.0681	0.0326*** (0.0021)	
	1987-1993				0.0159*** (0.0060)
	1994-2000				0.0580*** (0.0030)
	2001-2005				-0.0269*** (0.0031)
δ	1987-2005	−0.0286*** (0.0013)	-0.0167*** (0.0019)	-0.0563*** (0.0016)	
	1987-1993				-0.0387*** (0.0055)
	1994-2000				-0.0332*** (0.0029)
	2001-2005				-0.0926*** (0.0029)
σ	1987-2005	1.3695	0.4156	0.4109*** (0.0050)	
	1987-1993				0.3296*** (0.0118)
	1994-2000				0.4185*** (0.0053)
	2001-2005				0.3622*** (0.0088)
No. observations		5,501	5,501	5,501	5,501
No. companies		1,934	1,934	1,934	1,934

Sources: Korteweg and Sorensen (2010).
Notes: This table reports the estimates of the parameters of the one-factor dynamic selection model reported in Tables 1-3, 6, and 8 of Korteweg and Sorensen (2010). β ("RMRF") refers to the coefficient on the excess log return of the market in the valuation equation. α refers to the monthly risk-adjusted excess return. δ ("Intercept") refers to the intercept of the valuation equation. σ ("Sigma") refers to the standard deviation of the error term in the valuation equation. Observations refer to valuations. ***, **, and * denote statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table B3: Risk-Adjusted Returns (1987-2005): Replication

		One-Factor Model			
		OLS	GLS	DSM	
Parameter	Period	(1)	(2)	(3)	(4)
β	1987-2005	2.2213*** (0.0752)	2.3751*** (0.1025)	2.1816*** (0.1054)	
	1987-1993				0.7854 (1.3918)
	1994-2000				2.5453*** (0.1533)
	2001-2005				1.5207*** (0.1211)
α	1987-2005		0.0694	0.0486*** (0.0013)	
	1987-1993				0.0132 (0.0140)
	1994-2000				0.0949*** (0.0032)
	2001-2005				0.0183*** (0.0019)
δ	1987-2005	–0.0103*** (0.0007)	-0.0040*** (0.0012)	-0.0418*** (0.0011)	
	1987-1993				-0.0391*** (0.0148)
	1994-2000				-0.0097*** (0.0026)
	2001-2005				-0.0484*** (0.0014)
σ	1987-2005	1.1650	0.3745	0.4189*** (0.0020)	
	1987-1993				0.3150*** (0.0348)
	1994-2000				0.4488*** (0.0057)
	2001-2005				0.3630*** (0.0041)
No. observations		7,868	7,868	7,868	7,868
No. companies		2,721	2,721	2,721	2,721

Sources: PitchBook; https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.
Notes: This table replicates Korteweg and Sorensen (2010), reporting our estimates of the parameters of the dynamic selection model jointly characterized by Equations (B1) and (B2), estimated at the monthly level from 1987 to 2005. β refers to the coefficient on the excess log return of the market in the valuation equation. α refers to the monthly risk-adjusted excess return. δ refers to the intercept of the valuation equation. σ refers to the standard deviation of the error term in the valuation equation. Observations refer to the observed valuations and bankruptcies of U.S. companies for which we observe valuations in at least two different months from 1987 to 2005. The estimate of each parameter and its standard deviation are based on its mean and variance across 200 iterations of the Bayesian Gibbs sampling procedure developed by Korteweg and Sorensen (2010), after we discard 1,000 initial burn-in iterations. ***, **, and * denote statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table B4: Risk-Adjusted Returns (2004-2022): All Estimated Parameters

	Investor Type		
	All U.S. Investors	U.S. HNWIs	U.S. Institutions
Parameter	(1)	(2)	(3)
β	1.5585***	1.4050***	1.5661***
	(0.0554)	(0.0826)	(0.0819)
α	0.0779***	0.0864***	0.0778***
	(0.0012)	(0.0012)	(0.0015)
δ	0.0159***	0.0169***	0.0158***
	(0.0008)	(0.0012)	(0.0010)
σ	0.3497***	0.3712***	0.3496***
	(0.0024)	(0.0026)	(0.0042)
γ_v	0.5243***	0.4957***	0.5347***
	(0.0083)	(0.0080)	(0.0098)
$\gamma_{0,0}$	-2.0755***	-2.1285***	-2.0712***
	(0.0108)	(0.0164)	(0.0150)
$\gamma_{0,1}$	0.0364***	0.0392***	0.0360***
	(0.0009)	(0.0012)	(0.0012)
$\gamma_{0,2}$	-0.0004***	-0.0005***	-0.0004***
	(0.0000)	(0.0000)	(0.000)
No. observations	67,705	41,778	66,601
No. companies	20,729	12,272	20,315

Sources: PitchBook; https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.
Notes: This table reports our estimates of the parameters of our portfolio-weighted version of the dynamic selection model developed by Korteweg and Sorensen (2010) and jointly characterized by Equations (B1) and (B2), estimated at the monthly level from 2004 to 2022 separately for each of three different investor types: all U.S. investors, U.S. high-net-worth individuals, and U.S. institutional investors. β refers to the coefficient on the excess log return of the market in the valuation equation. $\alpha = \delta + 0.5\sigma^2 - 0.5\beta(1-\beta)\sigma_m^2$ refers to the monthly risk-adjusted excess return, where σ_m is the standard deviation of the excess log return on the market. δ refers to the intercept of the valuation equation. σ refers to the standard deviation of the error term in the valuation equation. γ_v refers to the coefficient on the excess log change in the company's valuation in the selection equation. $\gamma_{0,0}$, $\gamma_{0,1}$, and $\gamma_{0,2}$ refer to the coefficients on the constant, linear, and squared terms, respectively, of the quadratic function of the number of periods since the company's last observed valuation in the selection equation. Observations refer to the observed valuations and bankruptcies of U.S. companies for which we observe valuations in at least two different months from 2004 to 2022. The regression weight on each observation reflects each company's share of each investor type's total amount invested in early-stage companies from 2004 to 2022, divided by the number of valuations for that company. The estimate of each parameter and its standard deviation are based on its mean and variance across 200 iterations of the Bayesian Gibbs sampling procedure developed by Korteweg and Sorensen (2010), after we discard 1,000 initial burn-in iterations. ***, **, and * denote statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Table B5: Risk-Adjusted Returns (2004-2022): Multi-Factor Models

	Investor Type							
	U.S. HNWIs				U.S. Institutions			
Parameter	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	1.4050***	1.0582***	1.3893***	1.1807***	1.5661***	1.2742***	1.4717***	1.1938***
	(0.0826)	(0.1303)	(0.0856)	(0.1469)	(0.0819)	(0.1063)	(0.0907)	(0.1039)
β_{ES}		0.2081***		0.2324***		0.2436***		0.2580***
		(0.0195)		(0.0185)		(0.0187)		(0.0233)
β_{SMB}			0.1988	-0.0604			0.1789	0.0217
			(0.1932)	(0.2116)			(0.1828)	(0.1541)
β_{HML}			0.3975***	0.4177***			0.2896***	0.3501***
			(0.1209)	(0.0982)			(0.0960)	(0.1223)
α	0.0864***	0.0768***	0.0873***	0.0736***	0.0778***	0.0654***	0.0799***	0.0642***
	(0.0012)	(0.0021)	(0.0020)	(0.0021)	(0.0015)	(0.0015)	(0.0012)	(0.0013)
No. obs.	41,778	41,778	41,778	41,778	66,601	66,601	66,601	66,601
No. comp.	12,272	12,272	12,272	12,272	20,315	20,315	20,315	20,315

Sources: PitchBook; https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.
Notes: This table reports our estimates of the parameters of our portfolio-weighted version of the dynamic selection model developed by Korteweg and Sorensen (2010) and jointly characterized by Equations (B1) and (B2). We estimate the model at the monthly level from 2004 to 2022 separately for U.S. high-net-worth individuals and U.S. institutional investors. β refers to the coefficient on the excess log return of the market in the valuation equation. β_{ES} refers to the coefficient on the early-stage factor, defined as the monthly change in the log total early-stage financing raised by U.S. companies. β_{SMB} refers to the coefficient on the size factor, defined as the return premium on publicly listed companies with small market capitalizations relative to those with big market capitalizations. β_{HML} refers to the coefficient on the book-to-market factor, defined as the return premium on publicly listed companies with high book-to-market ratios relative to those with low book-to-market ratios. $\alpha = \delta + 0.5\sigma^2 - 0.5\tilde{\beta}' diag(\Sigma) + 0.5\tilde{\beta}'\Sigma\tilde{\beta}$ refers to the monthly risk-adjusted excess return, where $\tilde{\beta}$ is the vector of coefficients on the factors, δ is the intercept of the valuation equation, σ is the standard deviation of the error term in the valuation equation, and Σ is the covariance matrix of the factors. Observations refer to the observed valuations and bankruptcies of U.S. companies for which we observe valuations in at least two different months from 2004 to 2022. The regression weight on each observation reflects each company's share of each investor type's total amount invested in early-stage companies from 2004 to 2022, divided by the number of valuations for that company. The estimate of each parameter and its standard deviation are based on its mean and variance across 200 iterations of the Bayesian Gibbs sampling procedure developed by Korteweg and Sorensen (2010), after we discard 1,000 initial burn-in iterations. ***, **, and * denote statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

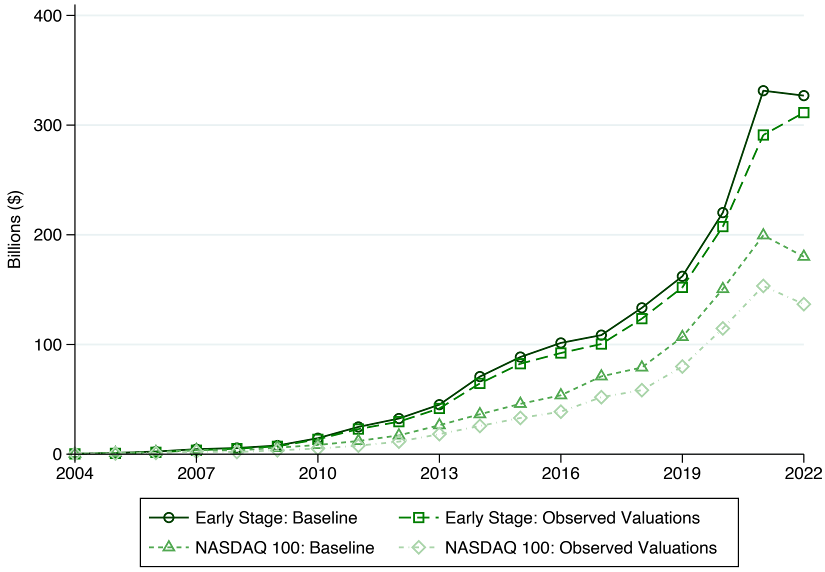
Table B6: Risk-Adjusted Returns (2009-2022): Multi-Factor Models

	Investor Type							
	U.S. HNWIs				U.S. Institutions			
Parameter	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	1.6410***	1.2788***	1.6116***	1.4199***	1.6097***	1.3123***	1.5598***	1.2426***
	(0.1166)	(0.0869)	(0.1201)	(0.1838)	(0.0857)	(0.0917)	(0.1051)	(0.1168)
β_{ES}		0.1734***		0.1869***		0.2074***		0.2611***
		(0.0190)		(0.0094)		(0.0138)		(0.0193)
β_{SMB}			0.1020	-0.0277			0.1727	-0.0092
			(0.2385)	(0.2400)			(0.2022)	(0.1768)
β_{HML}			0.3825***	0.4276***			0.2875***	0.4024***
			(0.0890)	(0.0824)			(0.0852)	(0.0740)
α	0.0814***	0.0738***	0.0830***	0.0716***	0.0815***	0.0688***	0.0813***	0.0674***
	(0.0018)	(0.0014)	(0.0015)	(0.0024)	(0.0015)	(0.0014)	(0.0013)	(0.0017)
No. obs.	37,850	37,850	37,850	37,850	58,256	58,256	58,256	58,256
No. comp.	11,466	11,466	11,466	11,466	18,425	18,425	18,425	18,425

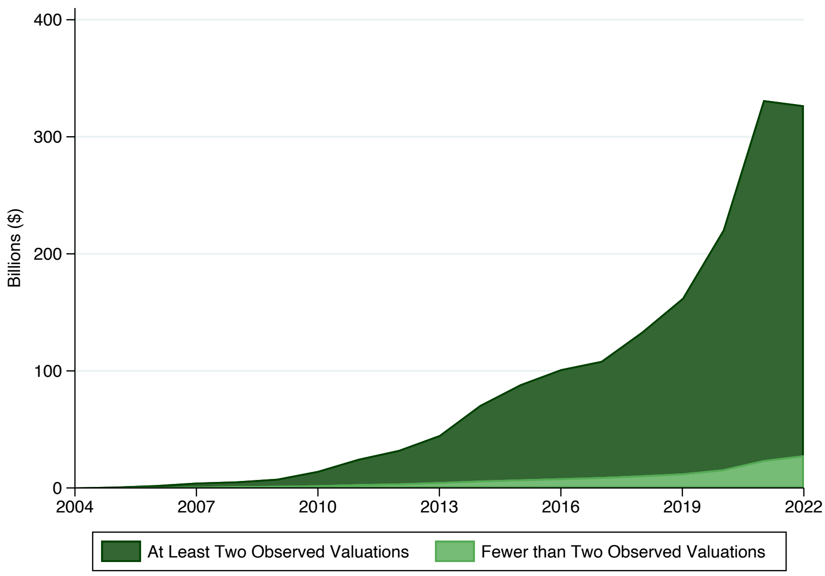
Sources: PitchBook; https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.
Notes: This table reports our estimates of the parameters of our portfolio-weighted version of the dynamic selection model developed by Korteweg and Sorensen (2010) and jointly characterized by Equations (B1) and (B2). We estimate the model at the monthly level from 2009 to 2022 separately for U.S. high-net-worth individuals and U.S. institutional investors. β refers to the coefficient on the excess log return of the market in the valuation equation. β_{ES} refers to the coefficient on the early-stage factor, defined as the monthly change in the log total early-stage financing raised by U.S. companies. β_{SMB} refers to the coefficient on the size factor, defined as the return premium on publicly listed companies with small market capitalizations relative to those with big market capitalizations. β_{HML} refers to the coefficient on the book-to-market factor, defined as the return premium on publicly listed companies with high book-to-market ratios relative to those with low book-to-market ratios. $\alpha = \delta + 0.5\sigma^2 - 0.5\tilde{\beta}' diag(\Sigma) + 0.5\tilde{\beta}'\Sigma\tilde{\beta}$ refers to the monthly risk-adjusted excess return, where $\tilde{\beta}$ is the vector of coefficients on the factors, δ is the intercept of the valuation equation, σ is the standard deviation of the error term in the valuation equation, and Σ is the covariance matrix of the factors. Observations refer to the observed valuations and bankruptcies of U.S. companies for which we observe valuations in at least two different months from 2009 to 2022. The regression weight on each observation reflects each company's share of each investor type's total amount invested in early-stage companies from 2009 to 2022, divided by the number of valuations for that company. The estimate of each parameter and its standard deviation are based on its mean and variance across 200 iterations of the Bayesian Gibbs sampling procedure developed by Korteweg and Sorensen (2010), after we discard 1,000 initial burn-in iterations. ***, **, and * denote statistical significance at the 99%, 95%, and 90% confidence levels, respectively.

Figure B3: Accumulated Value of HNWIs' Early-Stage Investments: Observed Valuations

(a) Alternative Series based on Only Observed Valuations



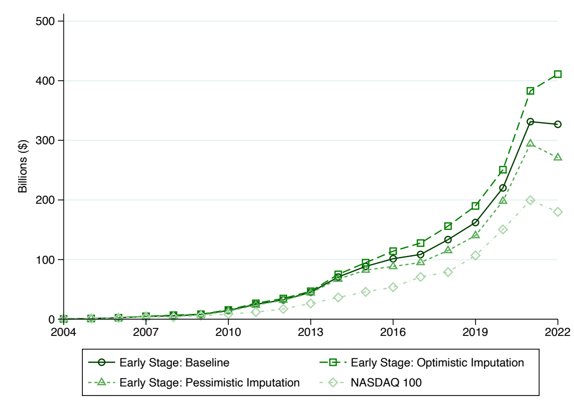
(b) Decomposition of Baseline Series



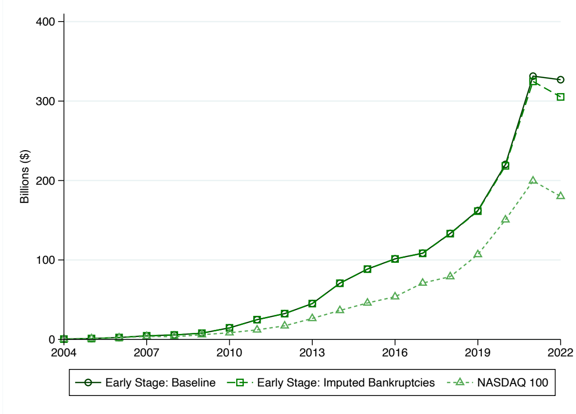
Sources: PitchBook, Capital IQ.
Notes: This figure describes the excess returns that U.S. high-net-worth individuals earned on their early-stage investments in U.S. companies from 2004 to 2022 relative to the total return version of the NASDAQ 100. Panel (a) plots the accumulated value of HNWIs' investments by the end of each year, comparing the baseline series based on the observed and imputed valuations for all companies to an alternative series based on only the observed valuations (and bankruptcies) for companies with at least two observed valuations. Panel (b) decomposes the baseline series into the parts attributable to the companies with at least two observed valuations and to those with fewer than two observed valuations, respectively. The values are expressed in nominal terms.

Figure B4: Accumulated Value of HNWIs' Early-Stage Investments: Alternative Samples

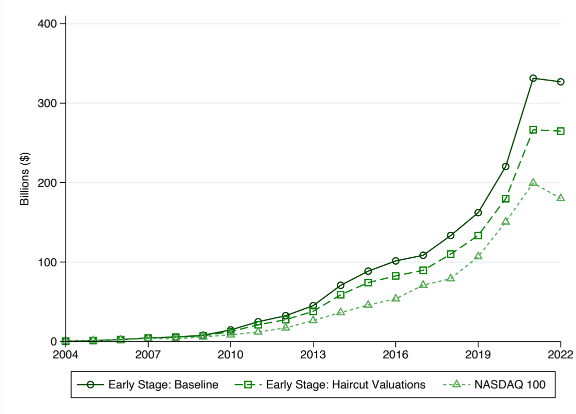
(a) Alternatively Imputed Valuations



(b) Imputed Bankruptcies

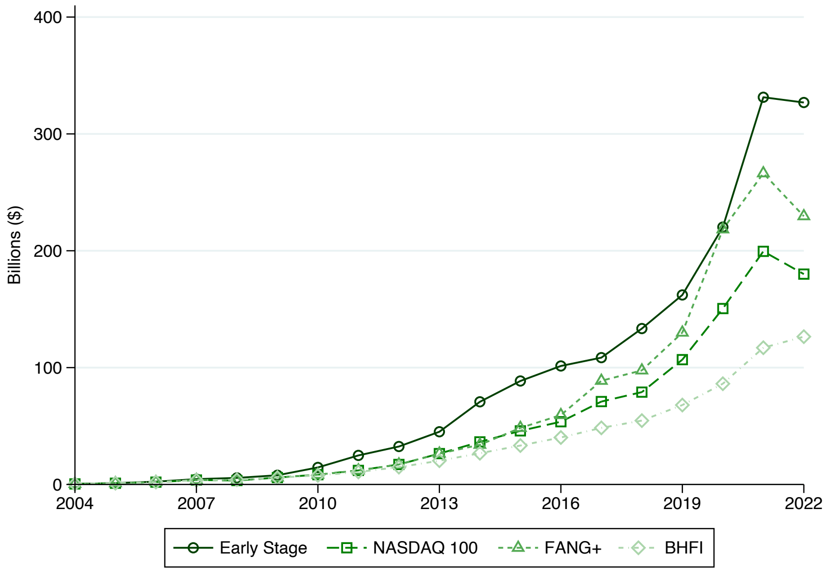


(c) Haircut Valuations



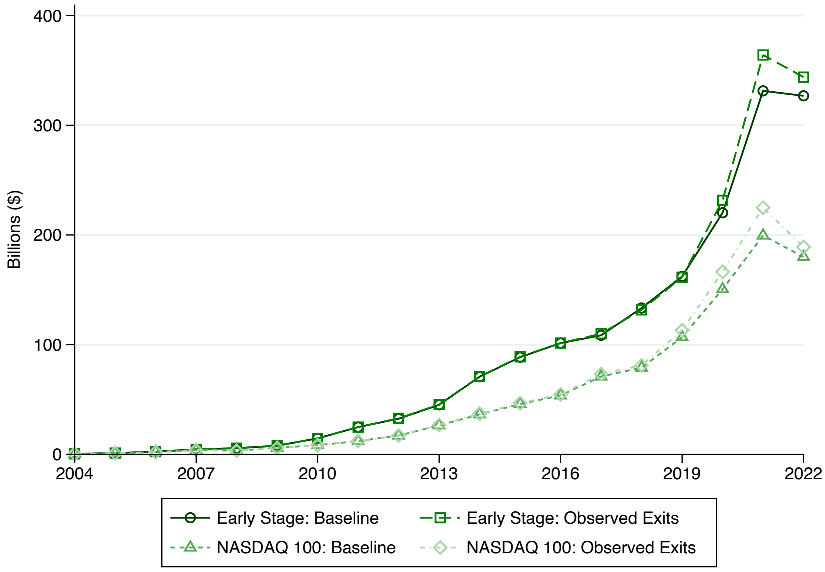
Sources: PitchBook, Capital IQ.
Notes: This figure describes the excess returns that U.S. high-net-worth individuals earned on their early-stage investments in U.S. companies from 2004 to 2022 relative to the total return version of the NASDAQ 100. Panel (a) plots the accumulated value of HNWIs' investments by the end of each year, comparing the series based on our baseline imputation procedure to alternative series based on optimistic and pessimistic variants of the procedure. Panel (b) compares the baseline series to an alternative series that also imputes bankruptcies for companies that last raised financing in or before 2022. Panel (c) compares the baseline series to an alternative series based on the application of haircuts to "unicorn" valuations that exceed $1 billion but are likely overvalued (Gornall and Strebulaev, 2020; Gahng, 2023). The values are expressed in nominal terms.

Figure B5: Accumulated Value of HNWIs' Early-Stage Investments: FANG+ and BHFI



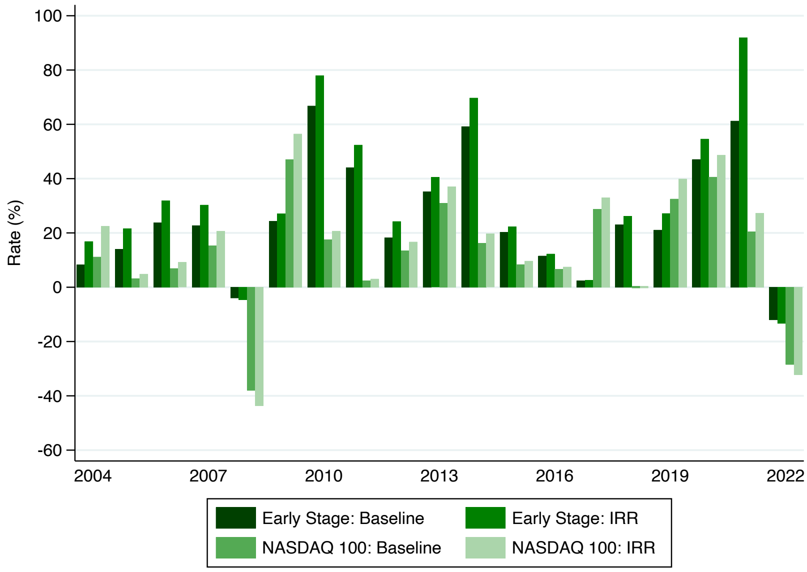
Sources: PitchBook, Capital IQ, Bloomberg.
Notes: This figure describes the excess returns that U.S. high-net-worth individuals earned on their early-stage investments in U.S. companies from 2004 to 2022 relative to the total return versions of the NASDAQ 100, FANG+, and Barclay Hedge Fund Index (BHFI). We set the daily rate of return on the FANG+ equal to that of the NASDAQ 100 on all dates on or before the index's inception date on September 19, 2014. The values are expressed in nominal terms.

Figure B6: Accumulated Value of HNWIs' Early-Stage Investments: Observed Exits



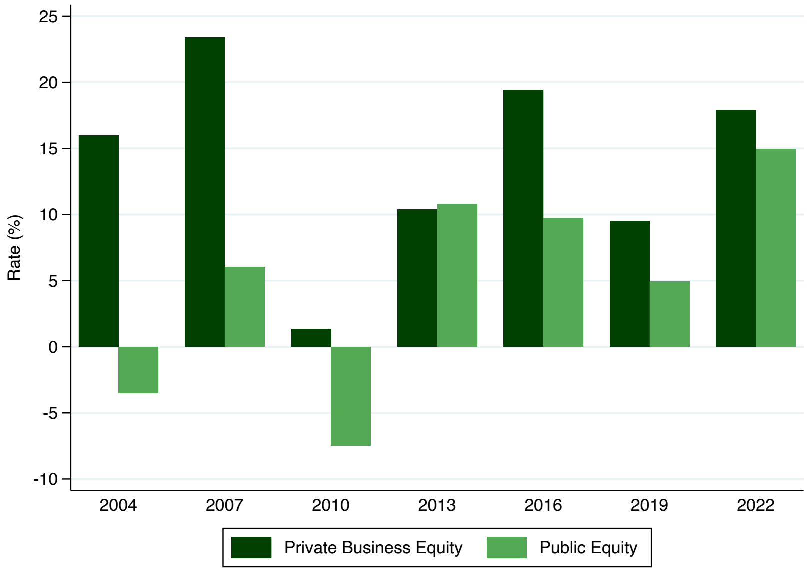
Sources: PitchBook, Capital IQ.
Notes: This figure describes the excess returns that U.S. high-net-worth individuals earned on their early-stage investments in U.S. companies from 2004 to 2022 relative to the total return version of the NASDAQ 100. It compares the baseline series based on both observed and imputed exits to an alternative series based on only observed exits. For each investment whose exit date we do not observe, we impute its exit date as the date five years after its entry date. The values are expressed in nominal terms.

Figure B7: Average Annual Rate of Return on HNWIs' Early-Stage Investments: IRR



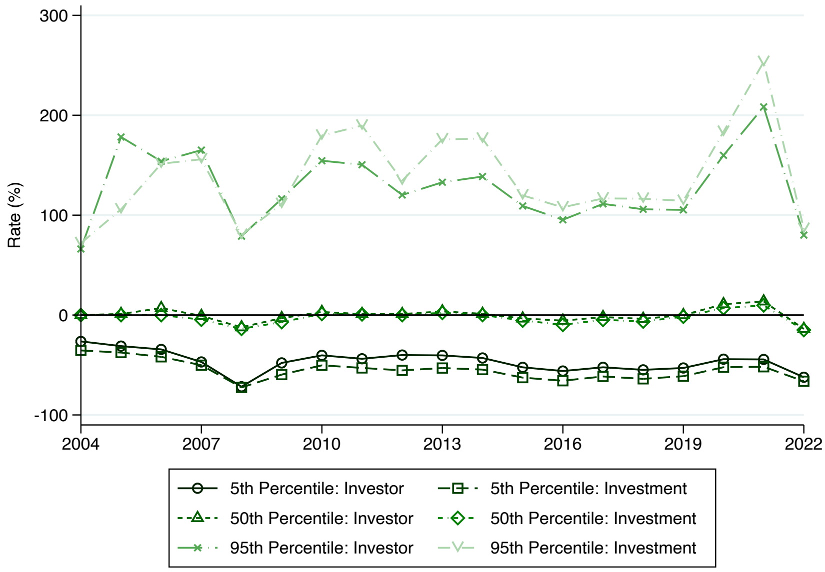
Sources: PitchBook, Capital IQ.
Notes: This figure describes the excess returns that U.S. high-net-worth individuals earned on their early-stage investments in U.S. companies from 2004 to 2022 relative to the total return version of the NASDAQ 100. It compares our baseline measure of their average annual rates of return to the alternative measure from Phalippou (2025). Our baseline measure (which is not annualized) is the ratio of the total returns on the investments during each year to their total accumulated value as of the start of the year. The alternative measure (which is annualized) is the 1-year NAV-to-NAV internal rate of return (IRR) across investments, which treats the entries into and exits from investments as cash flows and their accumulated values as of the start and by the end of each year as net asset values (NAV). The values are based on only the investments that have already been entered but that have not yet been exited.

Figure B8: Average Annualized 3-Year Rate of Return on Private Business Equity



Sources: Survey of Consumer Finances.
Notes: This figure plots the excess returns that U.S. households earned on their holdings of private business equity relative to their holdings of public equity from 2004 to 2022, using data from the Survey of Consumer Finances and the methodology developed by Moskowitz and Vissing-Jørgensen (2002) and Kartashova (2014).

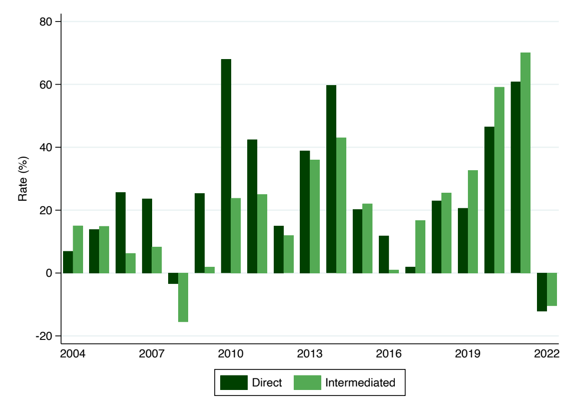
Figure B9: Distribution of Annual Rates of Return across HNWIs' Early-Stage Investments



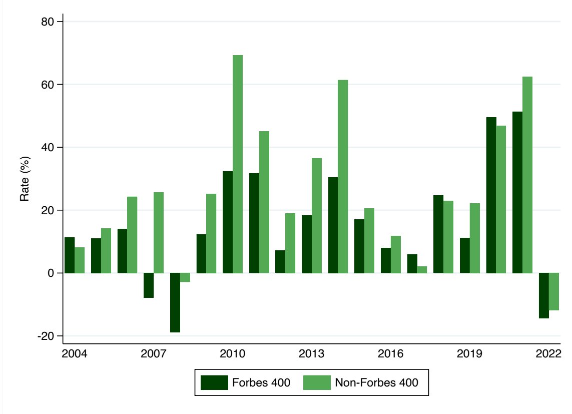
Sources: PitchBook.
Notes: This figure describes the distribution of annual rates of return across U.S. high-net-worth individuals' early-stage investments in U.S. companies. It compares the investor-level distribution to the investment-level distribution. The values are based on only the investments that, as of the start of each year, had already been entered but had not yet been exited.

Figure B10: Heterogeneous Annual Rates of Return on HNWIs' Early-Stage Investments

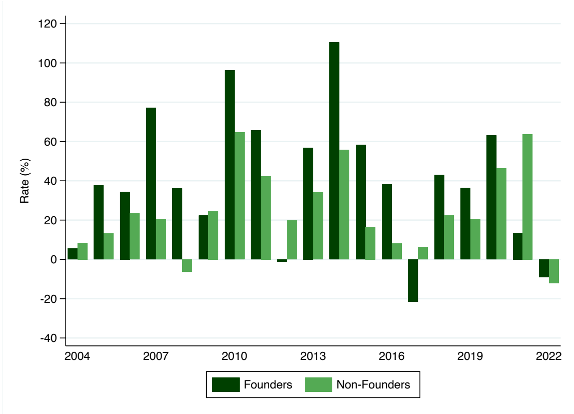
(a) Direct vs. Intermediated



(b) Forbes 400 vs. Non-Forbes 400



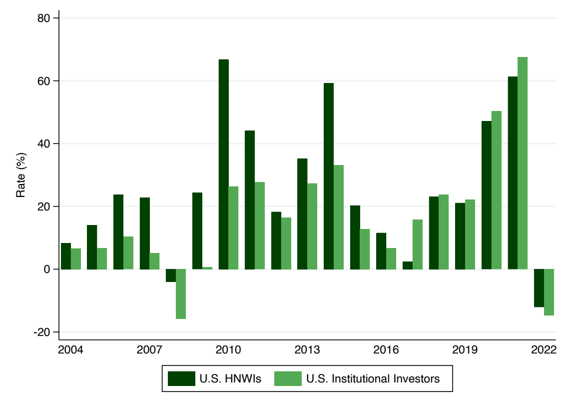
(c) Founders vs. Non-Founders



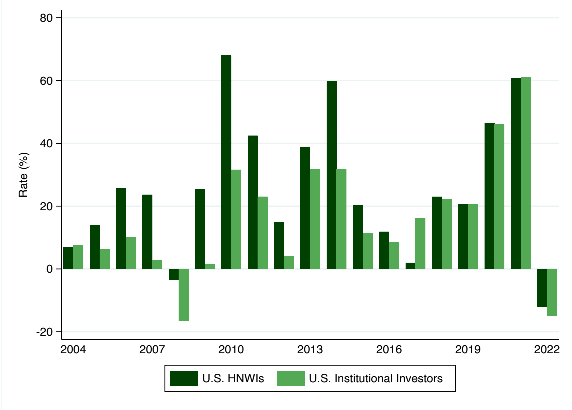
Sources: PitchBook.
Notes: This figure describes the average annual rate of return on U.S. high-net-worth individuals' early-stage investments in U.S. companies from 2004 to 2022. Panel (a) compares HNWIs' direct investments to their intermediated investments. Panel (b) compares investments by HNWIs ranked in the Forbes 400 to those by unranked ones. Panel (c) compares investments by founders to those by non-founders. The values are based on only the investments that, as of the start of each year, had already been entered but had not yet been exited. Panel (a) excludes 1 intermediated investment whose annual returns exceeded $300 million in 2011 and 2012, which we therefore consider an outlier.

Figure B11: Average Annual Rates of Return on U.S. Investors' Early-Stage Investments

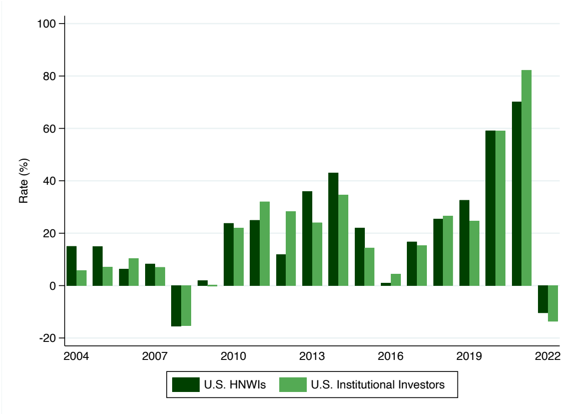
(a) All Investments



(b) Direct Investments



(c) Intermediated Investments



Sources: PitchBook.
Notes: This figure compares the average annual rates of return on U.S. high-net-worth individuals' and U.S. institutional investors' early-stage investments in U.S. companies from 2004 to 2022. Panel (a) compares all their investments. Panel (b) compares only their direct investments. Panel (c) compares only their intermediated investments. The values are based on only the investments that, as of the start of each year, had already been entered but had not yet been exited. Panel (c) excludes 1 intermediated investment (made by a U.S. HNWI) whose annual returns exceeded $300 million in 2011 and 2012, which we therefore consider an outlier.

Appendix C: Qualified Small Business Stock Reforms

In this appendix, we describe our procedure to identify eligible issuers of qualified small business stock (QSBS) using the data from PitchBook, and we elaborate upon our company-level and state-level analyses. Section C.1 describes how we identify the eligibility-relevant characteristics of each U.S. company: when it was an active private company, whether it was a C corporation, whether it was active primarily in a qualified trade or business, and when (if ever) its gross assets first exceeded \$50 million. Section C.2 examines how a single company-year panel regression that pools companies from different founding-year cohorts may generate spurious pre-trends due to the unbalancedness of the annual panel in the pre-period. Finally, Section C.3 reports additional results related to the history of the QSBS capital gains tax exclusion, the effects of its expansions on high-net-worth individuals' early-stage investments and companies, and the implications for state-level income inequality.

C.1 QSBS Eligibility

To evaluate the effects of the QSBS reforms on companies, we identify the companies that were active and privately held prior to the introduction of the reforms and determine which of them satisfied all three QSBS eligibility criteria.

C.1.1 Active Private Companies

We first identify the years during which each company was active and privately held, which we define as the period from its founding year 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, the founding year is observed directly. For an additional 58,777 companies (14.6%), we impute the missing founding year as the first year in which the company raised financing.

We also identify the first bankruptcy date for 45,399 companies (11.3%), the first public offering date for 18,524 (4.6%), and the first acquisition date for 200,277 (49.7%).

C.1.2 C Corporations

We then identify C corporations by parsing each company's legal name, which we observe for 316,779 (78.7%) U.S. companies. For the rest whose legal names are missing, we impute their legal name by setting it equal to their trade name—that is, the specific name under which a company operates and presents itself to the public, which may differ from its formal, legal, or registered corporate name.

From this directly observed or imputed legal name, we can then identify limited partnerships ("LP"), limited liability partnerships ("LLP"), and limited liability limited partnerships ("LLLP"), none of which could be taxed as C corporations. Although we can also identify limited companies ("LC" or "Ltd"), limited liability companies ("LLC"), professional limited liability companies ("PLLC"), and professional corporations ("PC"), these could be—but were not necessarily—taxed as C corporations.

We therefore classify only the remaining corporations ("Corp" or "Inc") as C corporations. To justify this classification, we emphasize that companies seeking financing in private capital markets are unlikely to be taxed as S corporations, since these can have at most 100 shareholders (Polsky and Yale, 2023). Based on our classification, 198,984 (49.4%) of the U.S. companies that we observe were C corporations. If we cannot classify the company into any of these types of companies, then it is classified as "Other".

C.1.3 Qualified Trades and Businesses

We next identify U.S. companies that were active primarily in a qualified trade or business. We do this based on each company's primary industry code, which is missing for only 146 (<0.1%) companies.[45] We classify a company as active primarily in a disqualified trade or business if its primary industry code was either missing or related to health services, legal services, engineering services, accounting services, consulting services, financial services, performing arts, hospitality, agriculture, or natural resources. Based on our classification, 278,168 (69.1%) U.S. companies were active primarily in qualified trades or businesses.

C.1.4 Gross Assets Exceeding $50 Million

To identify companies' QSBS eligibility as of the end of 2008 (i.e., the year immediately prior to the QSBS reforms), we require a measure of their gross assets. PitchBook's data contains information on the financial statements of 86,054 (21.4%) U.S. companies, whose gross assets we can calculate as the sum of the cash and cash equivalents and the net property, plant, and equipment on their balance sheets.[46] However, we observe both components of gross assets as of the end of 2008 for only 3,597 (4.2%) of these companies.

We therefore construct a proxy for each company's gross assets: the total financing that it raised up to each of its deals. For this calculation, we consider only the financing raised by the company as part of deals that we would expect to have increased the gross assets

[45] At least 80% of a QSBS-eligible company's assets must be used in the active conduct of qualified trades or businesses (Polsky and Yale, 2023). 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.

[46] 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.

on its balance sheet. For example, this excludes buyout and refinancing deals. In this way, we calculate the total financing raised as of the end of 2008 for 28,726 U.S. companies, 23,100 (80.4%) of which had not raised more than \$50 million.

We validate this proxy using PitchBook's data on the financial statements for the 3,491 companies for which we can calculate both measures. The two measures lie on the same side of the \$50 million threshold for 2,311 (66.2%) of these companies. The proxy's accuracy is even higher (82.0%) for the 172 companies founded in or after 2004 that were the most affected by the QSBS reforms (see Appendix Figure C2). For consistency, we use this proxy for all companies, even if we observe their relevant balance sheet information.

C.2 Spurious Pre-Trends in Regressions on Unbalanced Panels

We examine how a single company-year panel regression (see Equation (1) in Section 4.2.1) that pools companies from different founding-year cohorts may generate spurious pre-trends due to the unbalancedness of the annual panel 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 $\mathbf{Y_1} \coloneqq \{Y_{1,t}\}_{t=1}^{3} = \{0, 0, 1\}$; a treated company 2 founded in the second period with the history $\mathbf{Y_2} = \{\cdot, 0, 1\}$, 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 $\mathbf{Y_3} = \{0, 0, 0\}$; and a control company 4 founded in the second period with the history $\mathbf{Y_4} = \{\cdot, 0, 0\}$. After we partial out the company fixed effects $\{\bar{Y}_i\}_{i=1}^{4} \coloneqq \{\frac{1}{3}\sum_t Y_{i,t}\}_{i=1}^{4} = \{\frac{1}{3}, \frac{1}{2}, 0, 0\}$, the residuals are $\mathbf{Y_1} - \bar{Y}_1 = \{-\frac{1}{3}, -\frac{1}{3}, \frac{2}{3}\}$, $\mathbf{Y_2} - \bar{Y}_2 = \{\cdot, -\frac{1}{2}, \frac{1}{2}\}$, $\mathbf{Y_3} - \bar{Y}_3 = \{0, 0, 0\}$, and $\mathbf{Y_4} - \bar{Y}_4 = \{\cdot, 0, 0\}$.

First, consider a pooled regression. After we further partial out the year fixed effects $\{\bar{Y}_t\}_{t=1}^{3} \coloneqq \{\frac{1}{2}\sum_i (Y_{i,1} - \bar{Y}_i), \frac{1}{4}\sum_i (Y_{i,2} - \bar{Y}_i), \frac{1}{4}\sum_i (Y_{i,3} - \bar{Y}_i)\} = \{-\frac{4}{24}, -\frac{5}{24}, \frac{7}{24}\}$, the residuals become $\mathbf{Y_1} - \bar{Y}_1 - \bar{Y}_t = \{-\frac{4}{24}, -\frac{3}{24}, \frac{9}{24}\}$, $\mathbf{Y_2} - \bar{Y}_2 - \bar{Y}_t = \{\cdot, -\frac{7}{24}, \frac{5}{24}\}$, $\mathbf{Y_3} - \bar{Y}_3 - \bar{Y}_t = \{\frac{4}{24}, \frac{5}{24}, -\frac{7}{24}\}$, and $\mathbf{Y_4} - \bar{Y}_4 - \bar{Y}_t = \{\cdot, \frac{5}{24}, -\frac{7}{24}\}$. The mean differences between the residuals of the treated and control companies 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 period, the unbalancedness of the panel generates a spurious non-zero estimate.

Now, consider a separate regression for each founding-year cohort. After we partial out company fixed effects, the difference between the residuals for companies 1 and 3 in the first cohort is $(\mathbf{Y_1} - \bar{Y}_1) - (\mathbf{Y_3} - \bar{Y}_3) = \{-\frac{1}{3}, -\frac{1}{3}, \frac{2}{3}\}$, or $\{0, 0, 1\}$ if we set the second period as the base period. Similarly, the difference between the residuals for companies 2 and 4 in the second cohort is $(\mathbf{Y_2} - \bar{Y}_2) - (\mathbf{Y_4} - \bar{Y}_4) = \{\cdot, -\frac{1}{2}, \frac{1}{2}\}$, or $\{\cdot, 0, 1\}$. Thus, the weighted average of these cohort-specific differences—with each cohort's weight equal to its share of all the companies 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

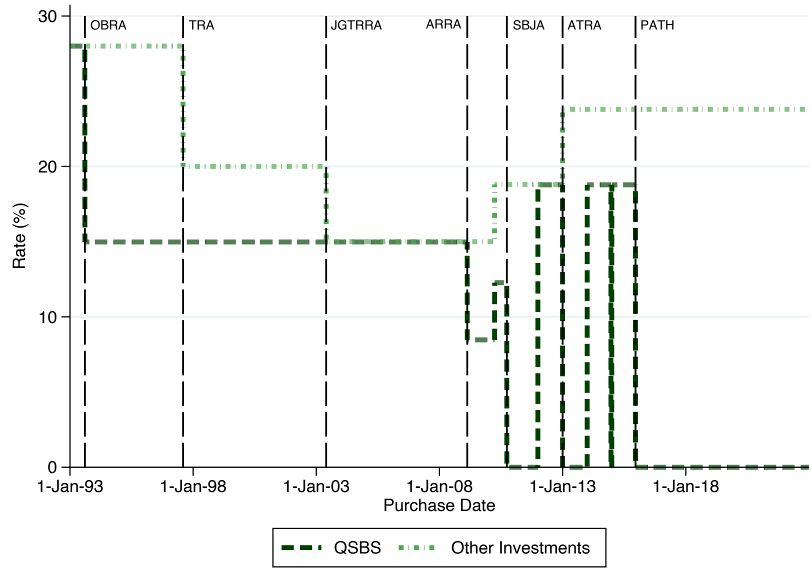
Table C1: U.S. Companies' Characteristics by QSBS Eligibility (2008)

	Share of Companies	
	Eligible	Ineligible
	(1)	(2)
Total	1.000	1.000
incl. founded between 2004 and 2008	0.448	0.388
incl. founded before 2004	0.552	0.612
incl. C corporations	1.000	0.332
incl. other companies	0.000	0.668
incl. technology companies	0.787	0.410
incl. professional services companies	0.000	0.201
incl. financial services companies	0.000	0.073
incl. agriculture/natural resources companies	0.000	0.074
incl. hospitality companies	0.000	0.044
incl. other non-technology companies	0.213	0.199
incl. $[0, 0.5) million total financing raised	0.209	0.227
incl. $[0.5, 1) million total financing raised	0.077	0.050
incl. $[1, 5) million total financing raised	0.268	0.171
incl. $[5, 10) million total financing raised	0.139	0.095
incl. $[10, 25) million total financing raised	0.187	0.121
incl. $[25, 50) million total financing raised	0.121	0.084
incl. $[50, 100) million total financing raised	0.000	0.128
incl. $[100, 500) million total financing raised	0.000	0.101
incl. $[500, 1000) million total financing raised	0.000	0.013
incl. >$1000 million financing raised	0.000	0.010
incl. raised early-stage financing from U.S. HNWI	0.061	0.049
incl. raised early-stage financing from non-U.S. HNWI	0.019	0.016
No. companies	7,832	6,392

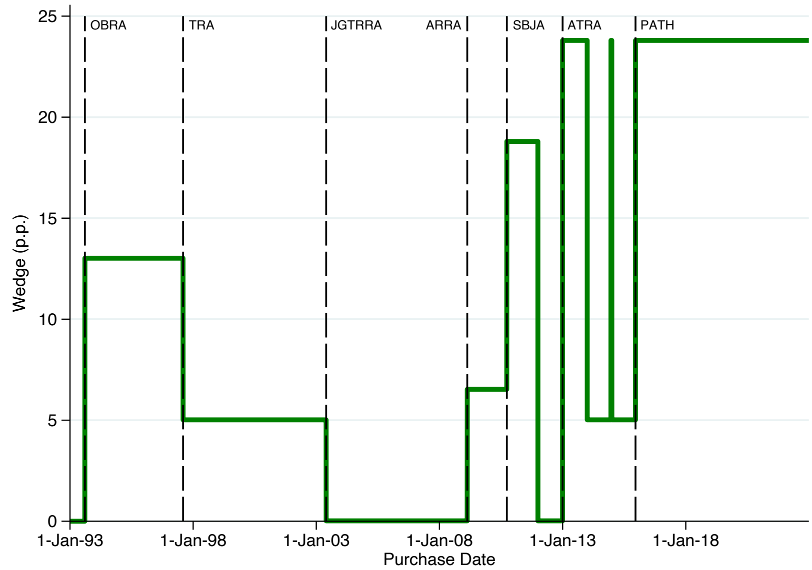
Sources: PitchBook.
Notes: This table summarizes the characteristics of the U.S. companies included in our company-level analyses in Section 4.2 as of 2008. QSBS-eligible issuers are C corporations active primarily in a qualified trade or business and that had raised no more than $50 million in total financing as of the end of 2008.

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

(a) Expected Tax Rates on QSBS vs. Other Investments

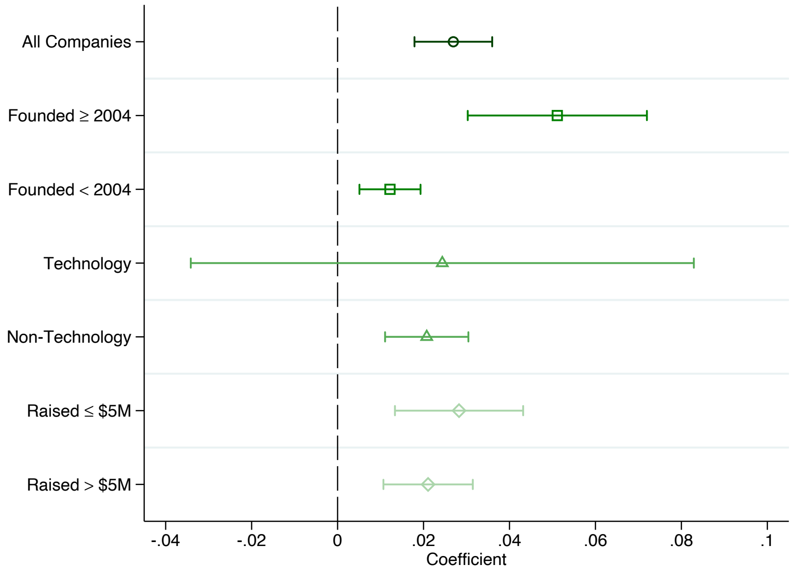


(b) Expected Tax Wedge on QSBS



Sources: Polsky and Yale (2023).
Notes: This figure compares the evolution of the expected federal tax rate on QSBS capital gains from 1993 to 2022 with that of the expected federal long-term capital gains tax rate on other investments. Panel (a) plots the evolution of the two expected rates (i.e., on QSBS and other investments) separately. Panel (b) plots the difference between them (i.e., the expected tax wedge, measured in percentage points). The values are the rates that individuals expected to be subject to as of their purchase date, even though the actual rates may have changed ex-post due to changes in the long-term capital gains tax rates on other investments. The vertical dashed lines 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 (JGTRRA), the American Recovery and Reinvestment Act (ARRA), the Small Business Jobs Act (SBJA), the American Taxpayer Relief Act (ATRA), and the Protecting Americans from Tax Hikes Act (PATH).

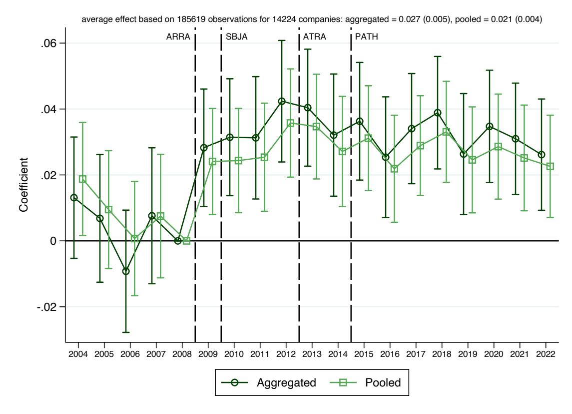
Figure C2: Effects of the QSBS Reforms on Companies: Heterogeneity



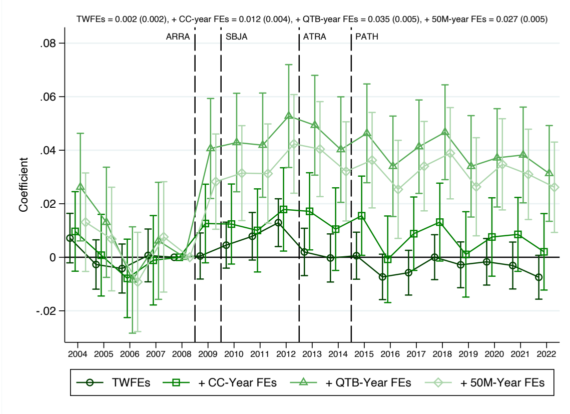
Sources: PitchBook.
Notes: This figure describes the heterogeneous effects of the QSBS reforms on QSBS-eligible companies' probability of raising early-stage financing from at least one U.S. high-net-worth individual, estimated using Equation (1). Specifically, it compares companies founded in or after 2004 to those founded before 2004, technology companies to non-technology companies, and companies that had raised no more than $5 million as of the end of 2008 to those that had raised more than $5 million. We cluster standard errors at the company level and report the 95% confidence interval for each estimate.

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

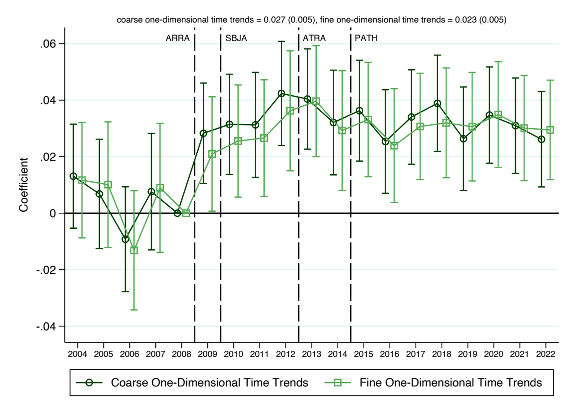
(a) Aggregated vs. Pooled Estimator



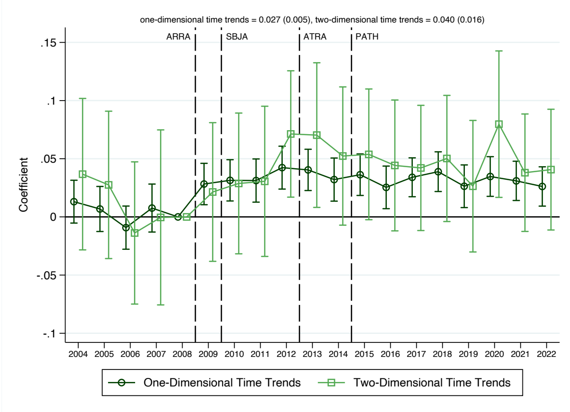
(b) Iteratively Added Time Trends



(c) Coarse vs. Fine Time Trends

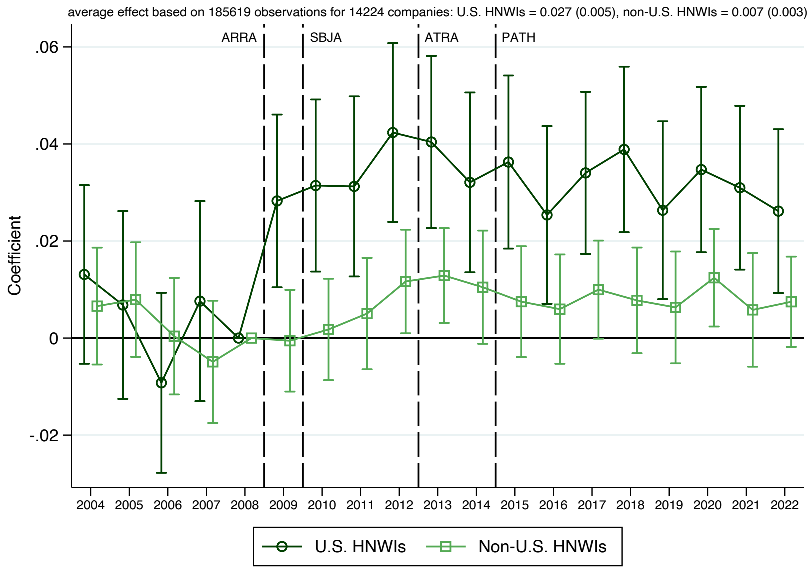


(d) One- vs. Two-Dimensional Time Trends



Sources: 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. high-net-worth individual, estimated using Equation (1). Panel (a) compares our baseline estimates, which are based on weighted averages of the estimates from separate regressions for different founding-year cohorts, with estimates from a single pooled regression. Panel (b) documents how, starting from a simple regression with only two-way fixed effects (TWFEs), we build up to our baseline estimates by iteratively adding separate time trends for each of the three dimensions of QSBS eligibility: being a C corporation (CC), being active primarily in a qualified trade or business (QTB), and having raised no more than $50 million as of the end of 2008 (50M). Panel (c) shows that our baseline estimates are robust to switching from coarse QTB-year fixed effects to fine year fixed effects for 157 different industries, and from coarse 50M-year fixed effects to fine year fixed effects for having raised no more than $5 million, $5-50 million, or more than $50 million as of the end of 2008. 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. We cluster standard errors at the company level and report the 95% confidence interval for each estimate.

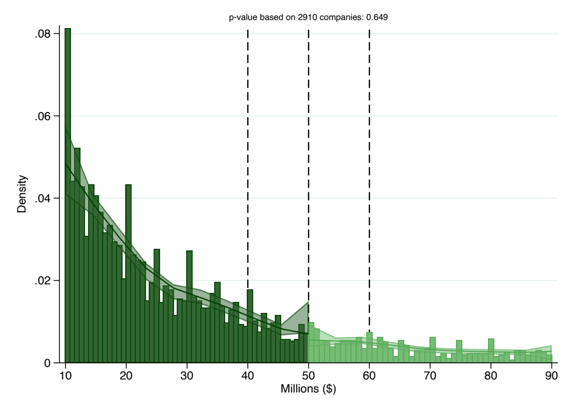
Figure C4: Effects of the QSBS Reforms on Companies: Placebo Test



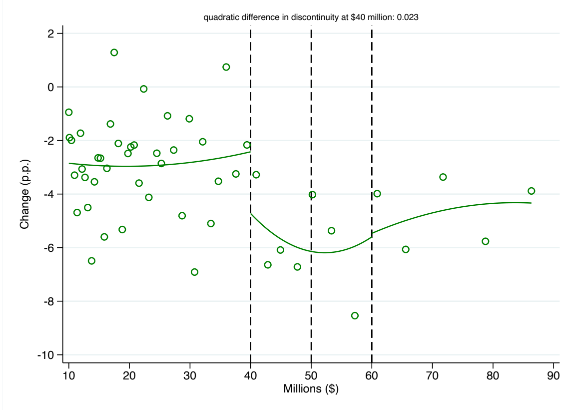
Sources: PitchBook.
Notes: This figure compares the effects of the QSBS reforms on QSBS-eligible companies' probability of raising early-stage financing from at least one U.S. high-net-worth individual to the effects on their probability of raising early-stage financing from at least one non-U.S. HNWI, estimated using Equation (1). Since non-U.S. HNWIs were not directly affected by the reforms, this comparison serves as a placebo test for our estimates of the effects on the probability of raising financing from U.S. HNWIs. The statistically significant estimates for the probability of raising financing from non-U.S. HNWIs may indicate the presence of spillovers via induced co-investment. We cluster standard errors at the company level and report the 95% confidence interval for each estimate.

Figure C5: Effects of the QSBS Reforms on Companies: Regression Discontinuity Design

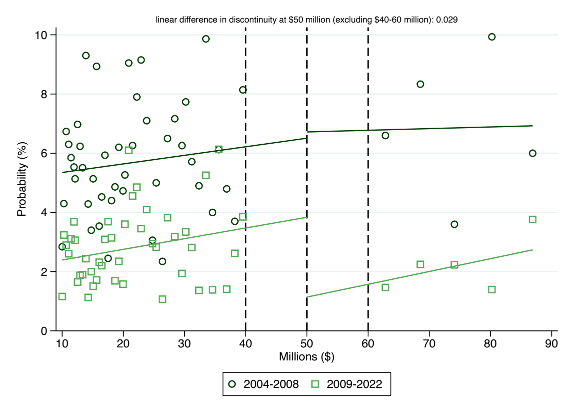
(a) Test for Manipulation in Running Variable



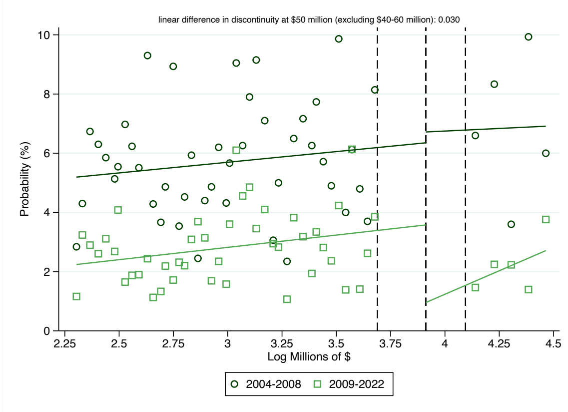
(b) Difference in Discontinuities



(c) Discontinuities in Financing Raised

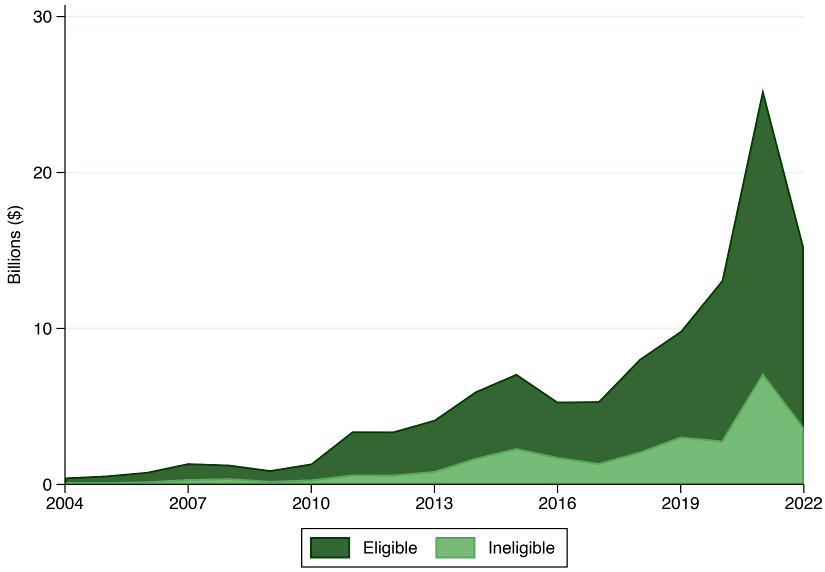


(d) Discontinuities in Log Financing Raised



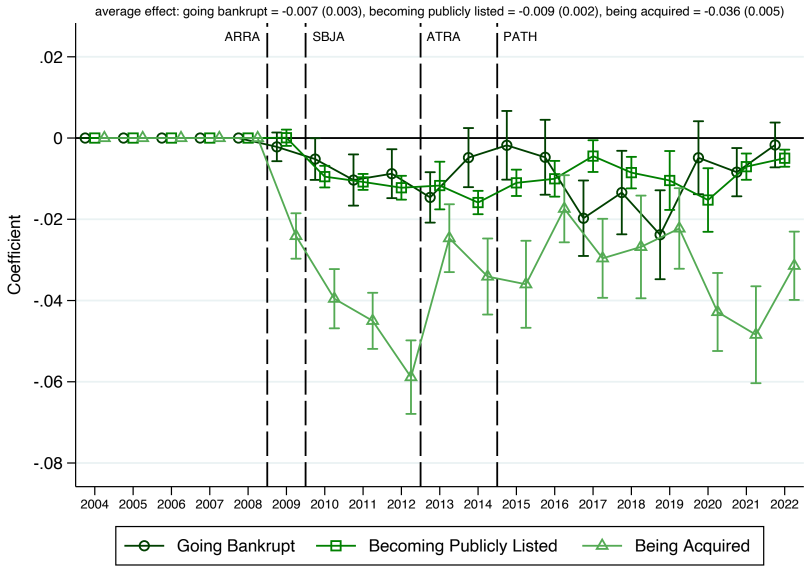
Sources: PitchBook.
Notes: This figure explores an auxiliary regression discontinuity design to evaluate the effects of the QSBS reforms on QSBS-eligible companies' probability of raising early-stage financing from at least one U.S. HNWI. We consider only U.S. C corporations that are active primarily in a qualified trade or business, investigating discontinuities around the $50 million gross assets threshold that corresponds to the third dimension of QSBS eligibility. In Panels (a)-(c), the horizontal axis plots each company's total financing raised as of the end of 2008. Panel (a) plots the test for manipulation in the running variable described in Cattaneo et al. (2015). Panel (b) plots the change between the pre-reform and post-reform periods in each company's probability of raising early-stage financing from U.S. HNWIs (i.e., the share of each period's years in which it did so), fitting separate quadratic functions for the ranges $10-40 million, $40-60 million, and $60-90 million. Panel (c) plots both the pre-reform and post-reform probabilities, fitting separate linear functions for the ranges $10-40 million and $40-60 million. Panel (d) replaces the horizontal axis in Panel (c) with its logged equivalent. Panels (b)-(d) plot binned scatter plots with 50 quantiles.

Figure C6: U.S. HNWIs' Early-Stage Investments in U.S. Companies by QSBS Eligibility



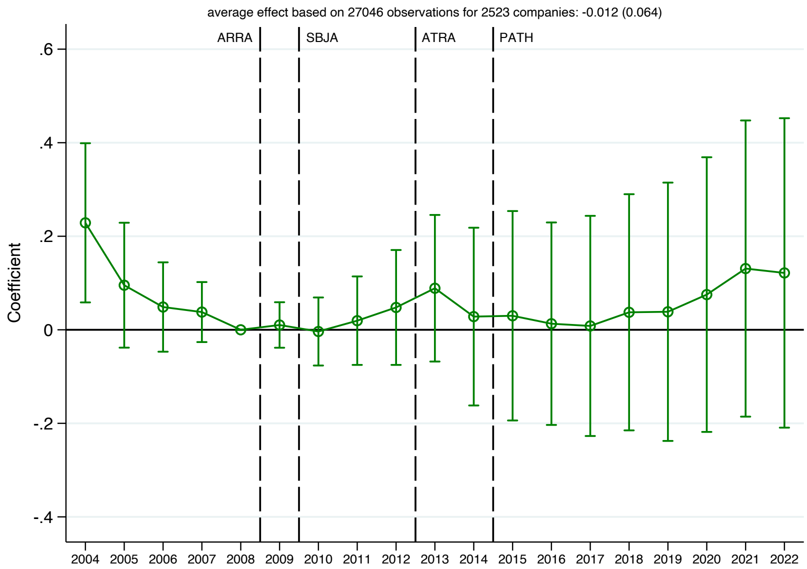
Sources: PitchBook.
Notes: This figure compares U.S. high-net-worth individuals' early-stage investments in U.S. C corporations active primarily in qualified trades and businesses (i.e., eligible issuers) to those in other U.S. companies (i.e., ineligible issuers). Unlike in the rest of our analyses, we do not consider the third dimension of QSBS eligibility (i.e., total financing raised) when categorizing companies in this plot. If we instead did so, then the originally QSBS-eligible companies that raised enough financing to eventually surpass the $50 million gross assets threshold would switch categories. This would complicate the interpretation of the plotted dynamic patterns, especially if the switchers—which would be positively selected—continued to raise early-stage financing from HNWIs. The values are expressed in nominal terms.

Figure C7: Effects of the QSBS Reforms on Companies' Real Outcomes: Decomposition



Sources: PitchBook.
Notes: This figure decomposes the effects of the QSBS reforms on QSBS-eligible companies' probability of remaining active private companies into their probabilities of going bankrupt, becoming publicly listed, and being acquired, estimated using Equation (1). We cluster standard errors at the company level and report the 95% confidence interval for each estimate.

Figure C8: Effects of the QSBS Reforms on Companies' Real Outcomes: Employment

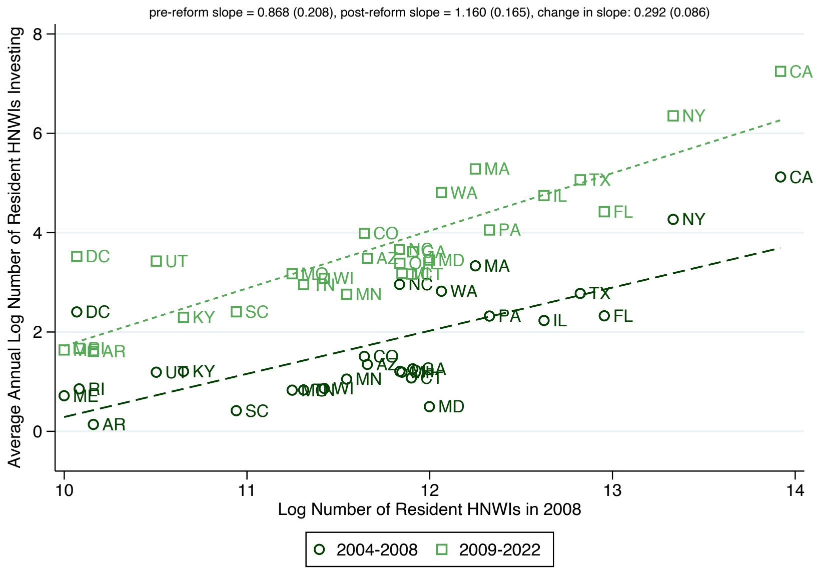


Sources: PitchBook.
Notes: This figure describes the effects of the QSBS reforms on QSBS-eligible companies' log number of employees, estimated using Equation (1). We first log-linearly interpolate the number of employees at each company and then, after the last year in which we observe this number, extrapolate it forward under the assumption that it remains constant. As in our other company-level analyses, we consider companies only from their founding until their first bankruptcy, public listing, or acquisition. We cluster standard errors at the company level and report the 95% confidence interval for each estimate.

Figure C9: Number of Resident HNWIs in 2008 as a State-Level Exposure to the Reforms

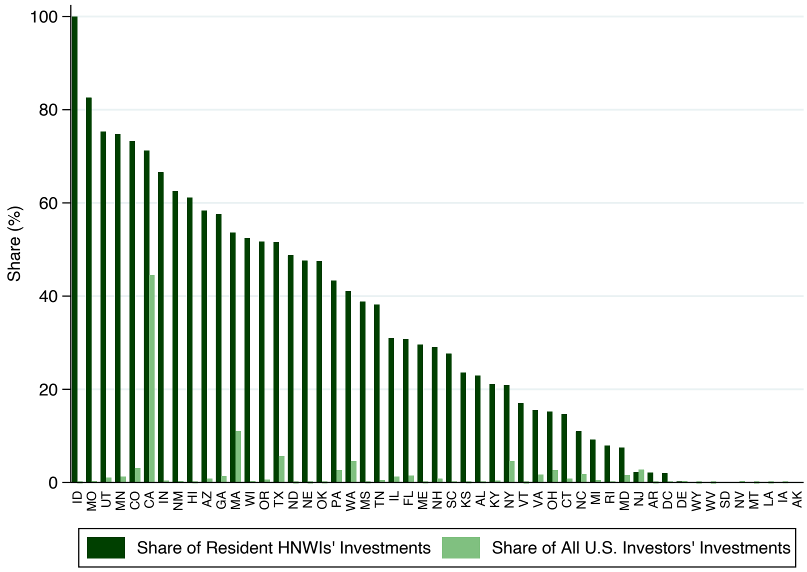
(a) Resident HNWIs' Early-Stage Investments

(b) Number of Resident HNWIs with Early-Stage Investments



Sources: PitchBook, GEOWEALTH-US.
Notes: This figure plots the relationships between various measures of resident high-net-worth individuals' early-stage investment activity and the number of resident HNWIs in 2008 across states. Specifically, it compares how the relationships change between the 2004-2008 period (before the QSBS reforms) and the 2009-2022 period (after the reforms). On the vertical axis, Panel (a) plots the average across years of the log total millions of U.S. dollars invested by resident HNWIs as part of their early-stage investments in U.S. companies. Panel (b) plots the average across years of the log number of resident HNWIs making such investments. On the horizontal axis, both panels plot the log number of resident HNWIs in 2008, as measured in the GEOWEALTH-US. 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.

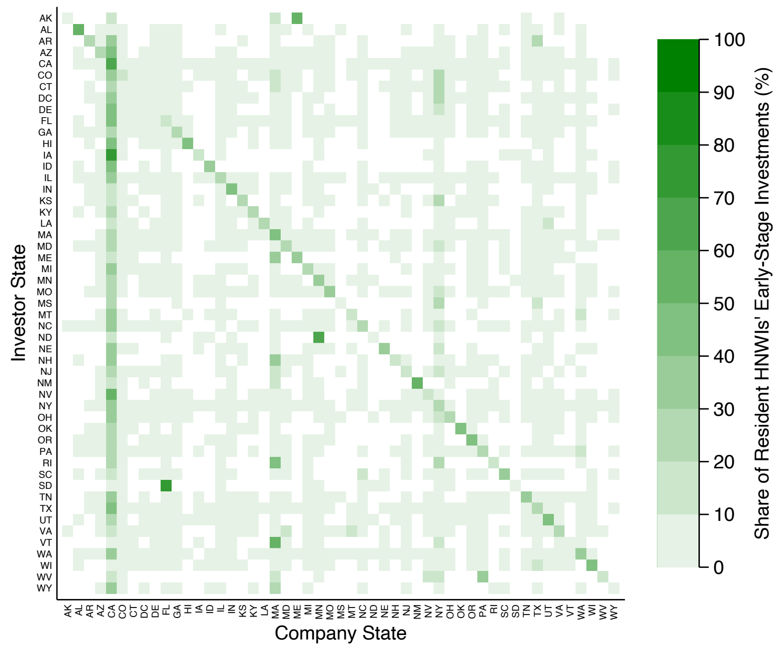
Figure C10: In-State Share of Resident HNWIs' Early-Stage Investments (2004-2008)



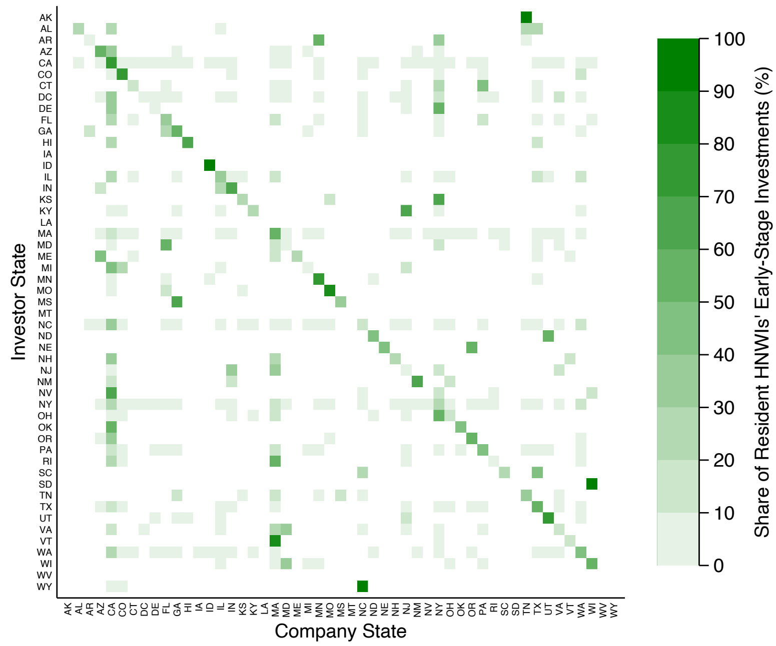
Sources: PitchBook.
Notes: This figure compares the share of each state's resident HNWIs' early-stage investments allocated to companies headquartered in their own state to the share of all U.S. investors' (i.e., both HNWIs' and institutional investors') early-stage investments allocated to those same companies from 2004 to 2008.

Figure C11: Distribution across States of Resident HNWIs' Early-Stage Investments

(a) 2004-2022

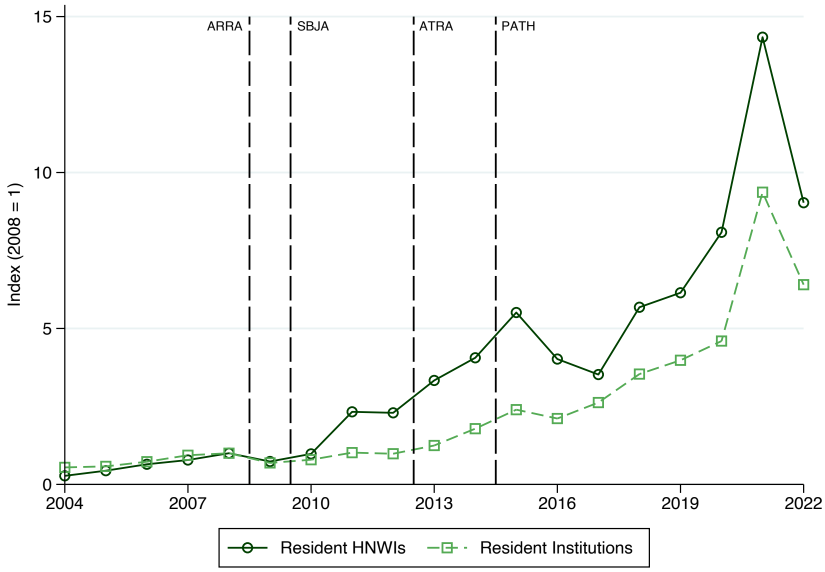


(b) 2004-2008



Sources: 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. Panel (b) plots it for the 2004-2008 period.

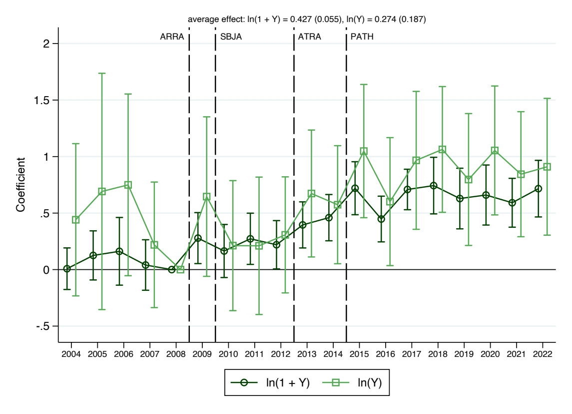
Figure C12: Resident HNWIs' vs. Institutions' In-State Early-Stage Investments



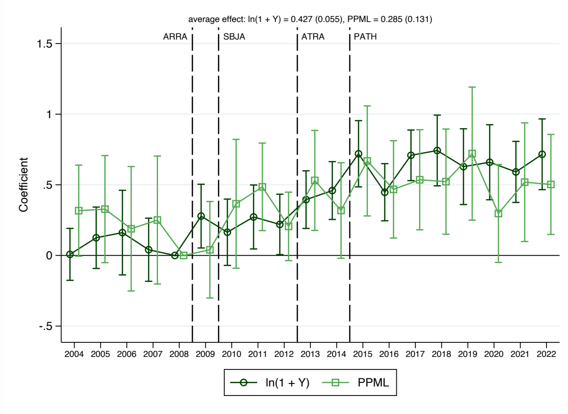
Sources: PitchBook.
Notes: This figure compares the evolution of resident HNWIs' and resident institutional investors' early-stage investments in companies headquartered in their own state from 2004 to 2022.

Figure C13: Effects of the QSBS Reforms on Early-Stage Investments: Estimators

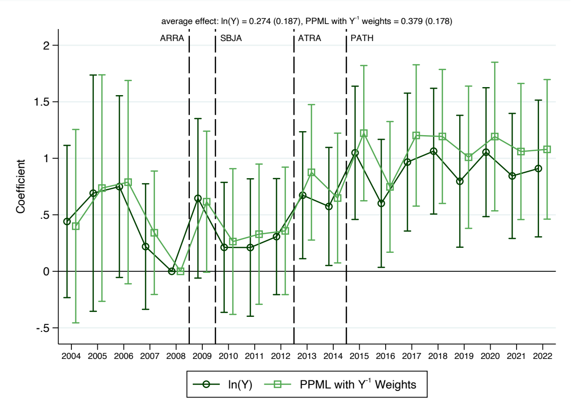
(a) $\ln(1 + Y_{s,g,t})$ vs. $\ln Y_{s,g,t}$



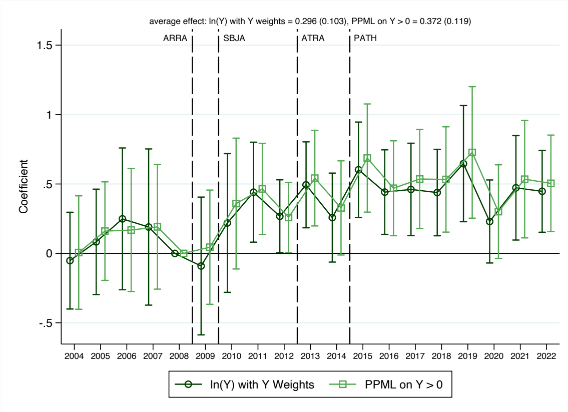
(b) $\ln(1 + Y_{s,g,t})$ vs. PPML



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



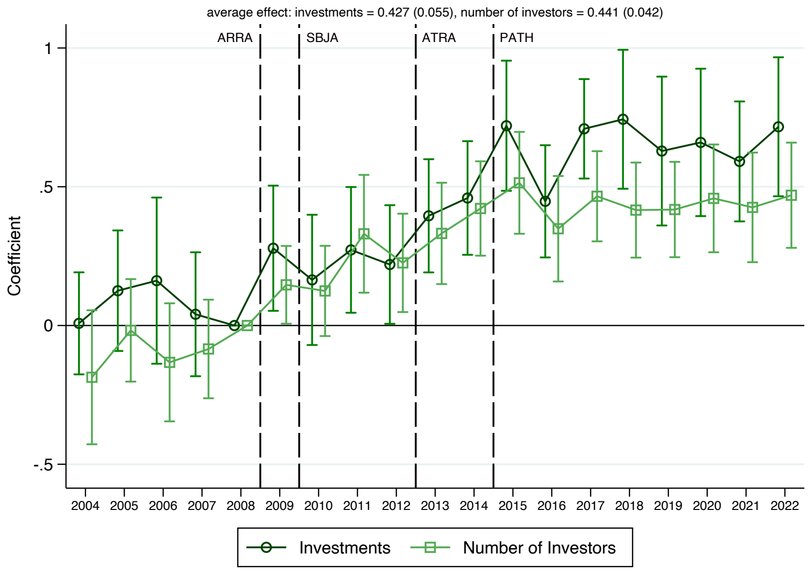
(d) $\ln Y_{s,g,t}$ with $Y_{s,g,t}$ Weights vs. PPML



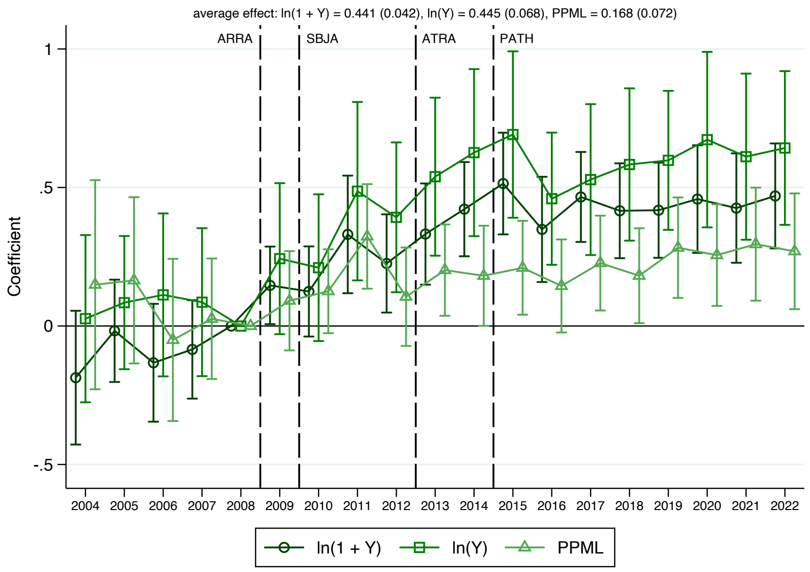
Sources: PitchBook, GEOWEALTH-US.
Notes: This figure evaluates the robustness of our baseline estimates of the effects of the QSBS reforms on resident high-net-worth individuals' early-stage investments to using alternative estimators, estimated using Equation (2). Panel (a) compares our baseline OLS estimates with $\ln(1 + Y_{s,g,t})$ as the outcome variable to OLS estimates with $\ln Y_{s,g,t}$ as the outcome variable. Panel (b) compares our baseline estimates to PPML estimates with $Y_{s,g,t}$ as the outcome variable. Panel (c) compares unweighted OLS estimates with $\ln Y_{s,g,t}$ as the outcome variable to weighted PPML estimates with $Y_{s,g,t}$ as the outcome variable and $Y_{s,g,t}^{-1}$ as the weights, based on the subsample of observations for which $Y_{s,g,t} > 0$. Panel (d) considers the same subsample, comparing weighted OLS estimates with $\ln Y_{s,g,t}$ as the outcome variable and $Y_{s,g,t}$ as the weights to unweighted PPML estimates with $Y_{s,g,t}$ as the outcome variable. We cluster standard errors at the state level and report the 95% confidence interval for each estimate.

Figure C14: Effects of the QSBS Reforms on the Number of Resident HNWIs Investing

(a) Investments vs. Number of Investors



(b) Alternative Estimators for the Number of Investors

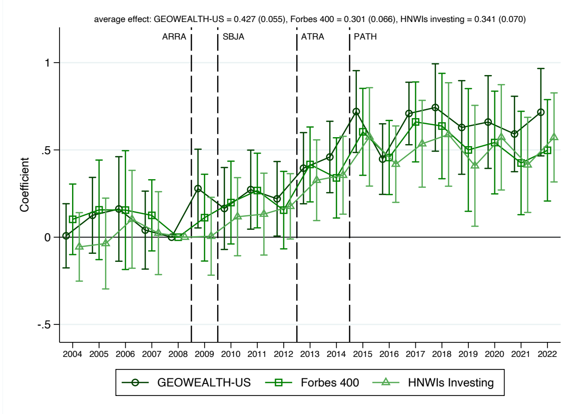


Sources: PitchBook, GEOWEALTH-US.
Notes: This figure describes the effects of the QSBS reforms on resident high-net-worth individuals' early-stage investments and on the number of resident HNWIs investing, estimated using Equation (2). Panel (a) compares the effects on investments to those on the number of investors, replacing $\ln Y_{s,g,t}$ with $\ln(1 + Y_{s,g,t})$ as the outcome variable to ensure a balanced panel. Panel (b) evaluates the robustness of the effects on the number of investors to the use of alternative estimators. We cluster standard errors at the state level and report the 95% confidence interval for each estimate.

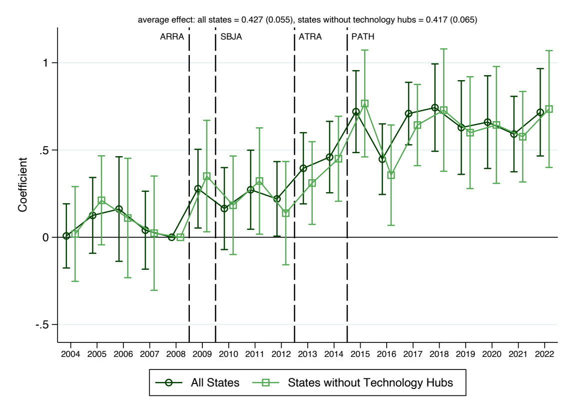
Figure C15: Effects of the QSBS Reforms on Early-Stage Investments: Robustness

(a) Vector of Control Variables

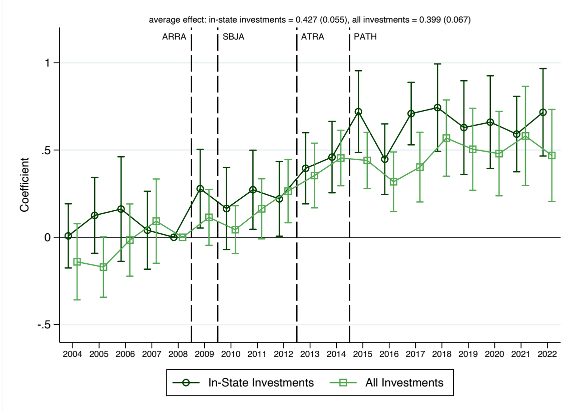
(b) Number of Resident HNWIs in 2008



(c) States without Technology Hubs



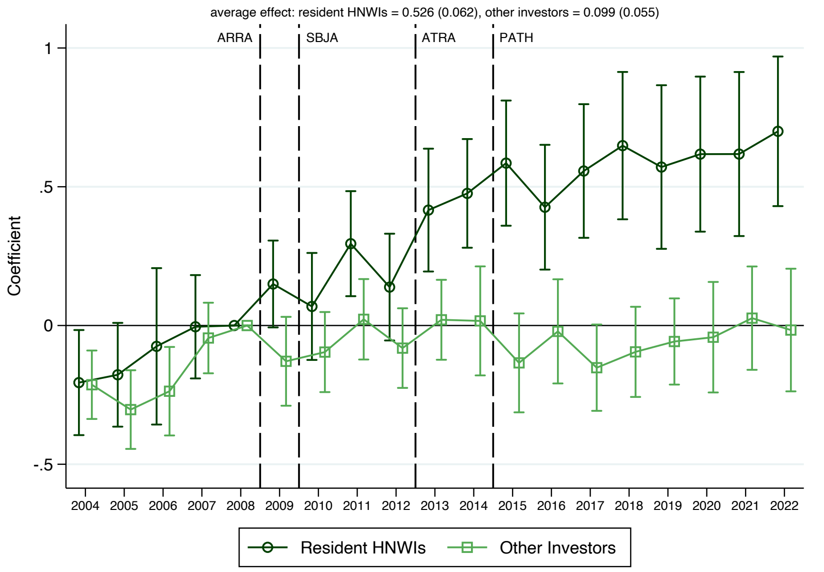
(d) All Investments



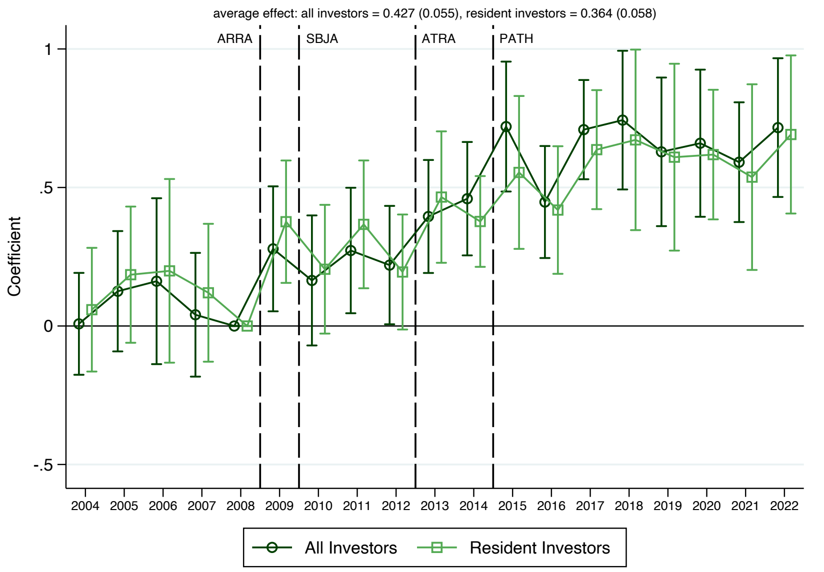
Sources: PitchBook, GEOWEALTH-US, Forbes.
Notes: This figure evaluates the robustness of our baseline estimates of the effects of the QSBS reforms on resident high-net-worth individuals' early-stage investments, estimated using Equation (2). Panel (a) compares our baseline estimates, which control for 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 control variables. Panel (b) compares our baseline estimates, which exploit state-level variation in the log number of resident HNWIs in 2008 according to the GEOWEALTH-US, to estimates that instead exploit variation in the log of one plus the number of residents ranked in the Forbes 400 rich list in 2008 or number of 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 that contain major technology hubs (see https://builtin.com/tech-hubs). 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 C11), we weight the observations for each investor state-company state pair by the average from 2004 to 2008 of the share of that investor state's resident HNWIs' early-stage investments allocated to the companies headquartered in that company state. We cluster standard errors at the state level and report the 95% confidence interval for each estimate.

Figure C16: Effects of the QSBS Reforms on Early-Stage Investments: Investor Groups

(a) Difference-in-Differences Estimates

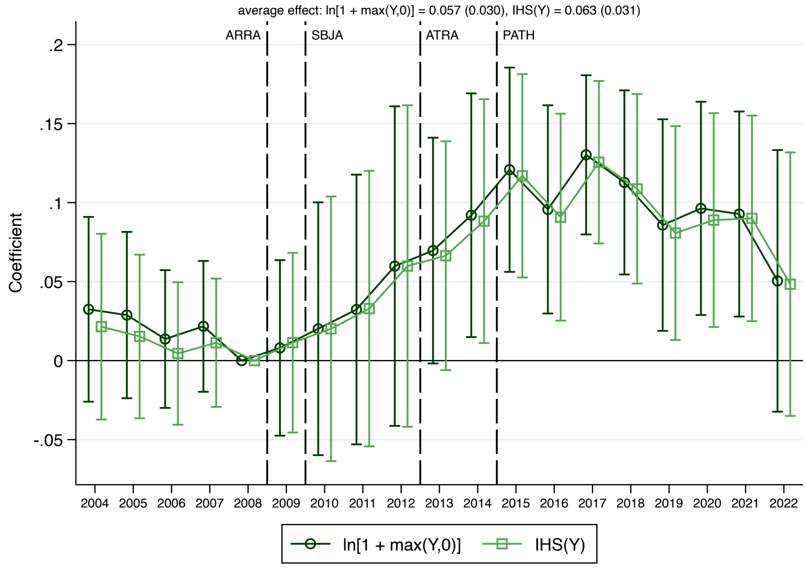


(b) Triple-Difference Estimates Considering Only Resident Investors



Sources: PitchBook, GEOWEALTH-US.
Notes: This figure evaluates the robustness of our baseline estimates of the effects of the QSBS reforms on resident high-net-worth individuals' early-stage investments to considering alternative investor groups, estimated using Equation (2). Panel (a) plots the difference-in-differences estimates underlying our baseline triple-difference estimates, comparing the difference-in-differences estimates for "treated" resident HNWIs to the average 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 institutional investors. We cluster standard errors at the state level and report the 95% confidence interval for each estimate.

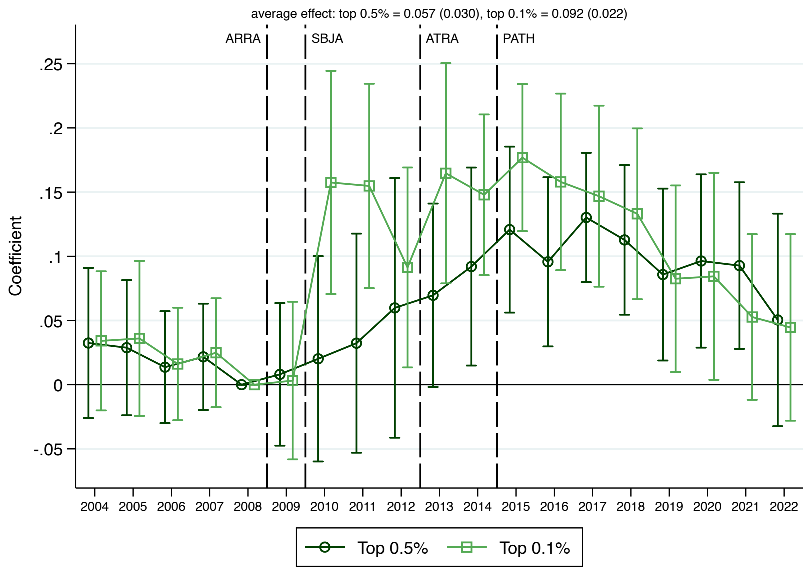
Figure C17: Implications for State-Level Income Inequality: Alternative Transformations



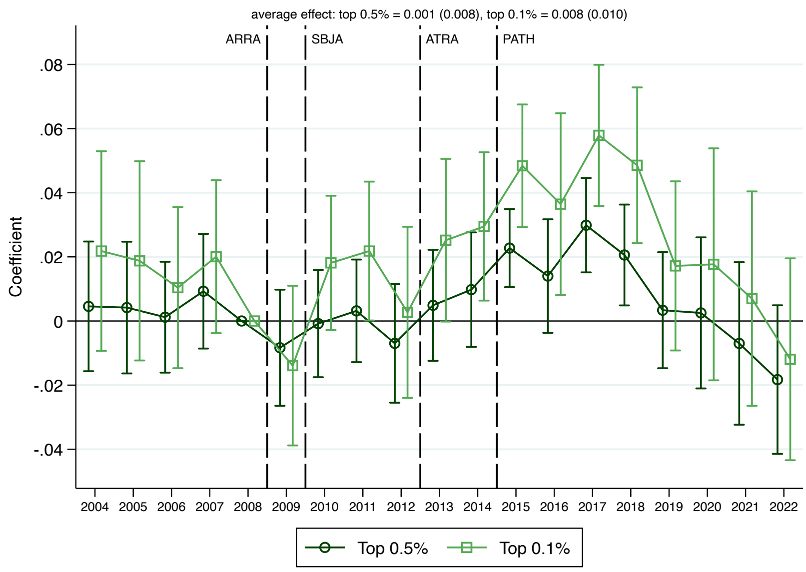
Sources: SOI Tax Stats, GEOWEALTH-US.
Notes: This figure describes the implications of the QSBS reforms on state-level income inequality based on alternative transformations of the outcome variable in Equation (3), namely, $\ln[1 + \max(Y_{s,g,t}, 0)]$ and the inverse hyperbolic sine (IHS) transformation. We focus on the estimates for capital gains income, cluster standard errors at the state level, and report the 95% confidence interval for each estimate.

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

(a) Capital Gains Income

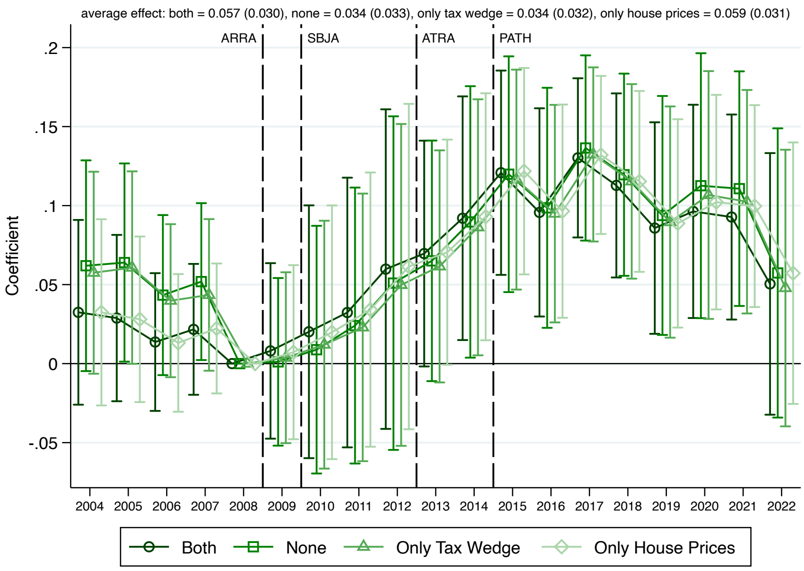


(b) Total Income



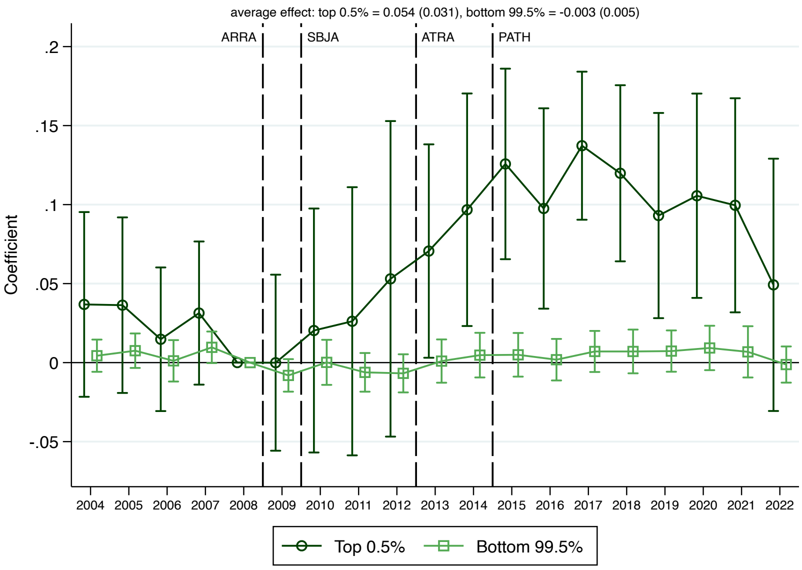
Sources: SOI Tax Stats, GEOWEALTH-US.
Notes: This figure compares the implications of the QSBS reforms on the average log income gap between the top 0.5% and bottom 99.5% to that between the top 0.1% and bottom 99.5%, estimated using Equation (3). To ensure a balanced panel, we replace $\ln Y_{s,g,t}$ with $\ln[1 + \max(Y_{s,g,t}, 0)]$ as the outcome variable. Panel (a) reports the estimates for capital gains income. Panel (b) reports those for total income. We cluster standard errors at the state level and report the 95% confidence interval for each estimate.

Figure C19: Implications for State-Level Income Inequality: Alternative Control Variables



Sources: SOI Tax Stats, GEOWEALTH-US.
Notes: This figure describes the implications of the QSBS reforms on state-level income inequality based on alternative vectors of control variables included in Equation (3), focusing on the estimates for capital gains income. We compare 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 control variables. To ensure a balanced panel, we replace $\ln Y_{s,g,t}$ with $\ln[1+\max(Y_{s,g,t},0)]$ as the outcome variable. We cluster standard errors at the state level and report the 95% confidence interval for each estimate.

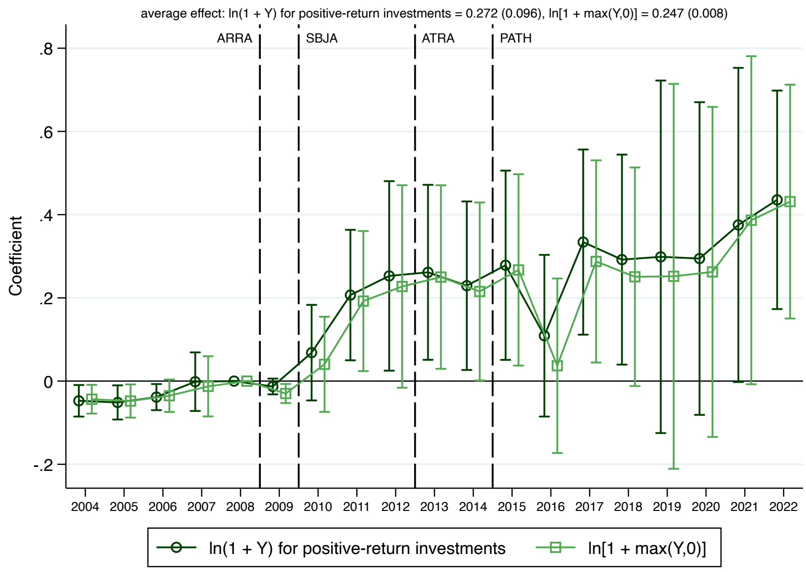
Figure C20: Implications for State-Level Income Inequality: Difference-in-Differences



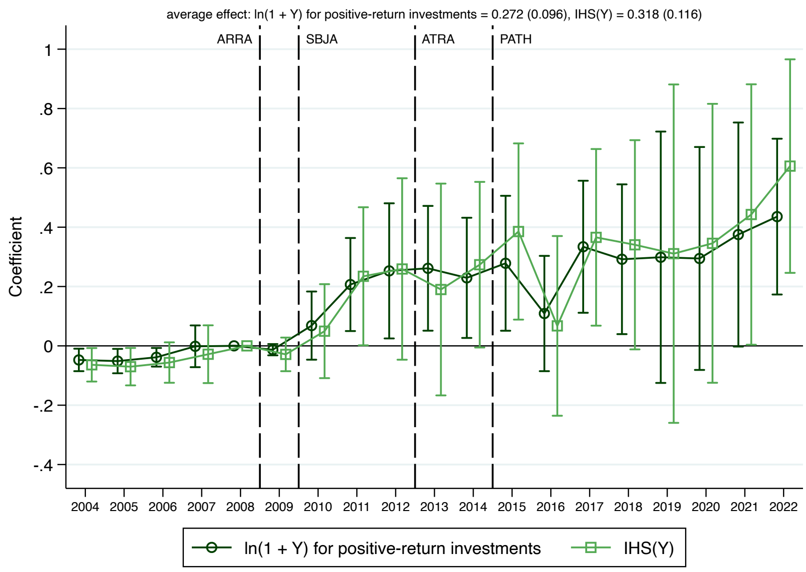
Sources: SOI Tax Stats, GEOWEALTH-US.
Notes: This figure compares the implications of the QSBS reforms on the average log capital gains income of the top 0.5% to that of the bottom 99.5%, representing the difference-in-differences parameters underlying the triple-difference parameters estimated using Equation (3). To ensure a balanced panel, we replace $\ln Y_{s,g,t}$ with $\ln[1 + \max(Y_{s,g,t}, 0)]$ as the outcome variable. We cluster standard errors at the state level and report the 95% confidence interval for each estimate.

Figure C21: Implications for Early-Stage Capital Gains Income: Robustness

(a) $\ln(1 + Y_{s,g,t})$ vs. $\ln[1 + \max(Y_{s,g,t}, 0)]$



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



Sources: PitchBook, SOI Tax Stats, GEOWEALTH-US.
Notes: This figure evaluates the robustness of the implications of the QSBS reforms on the average early-stage capital gains income of the top 0.5%, estimated using an equation similar to Equation (3). As the outcome variable, we calculate the log early-stage capital gains income of the HNWIs residing in each state in each year (based on PitchBook) per individual ranked in the top 0.5% of the state-level income distribution (based on the SOI Tax Stats). Panel (a) compares our baseline estimates, based on replacing $\ln Y_{s,g,t}$ with $\ln(1 + Y_{s,g,t})$ as the outcome variable and constructing $Y_{s,g,t}$ using only investments that yielded positive returns, to ones based on using $\ln[1 + \max(Y_{s,g,t}, 0)]$ on all investments. Note that, if $Y_{s,g,t} \geq 0$, then $\ln[1 + \max(Y_{s,g,t}, 0)] = \ln(1 + Y_{s,g,t})$. Panel (b) compares our baseline estimates to ones based on using the inverse hyperbolic sine (IHS) transformation on all investments. We cluster standard errors at the state level and report the 95% confidence interval for each estimate.

Appendix D: Inequality

In this appendix, we describe and validate our measures of state-level and nationwide economic inequalities, and we elaborate upon our counterfactual simulations. Section D.1 validates our baseline measure of the number of high-net-worth individuals residing in the U.S. Section D.2 details our procedures to construct income and wealth distribution series based on the Statistics of Income and the Survey of Consumer Finances, respectively. Section D.3 describes our methodology to counterfactually simulate income and wealth inequality in the U.S. and reports additional results on these simulations.

D.1 High-Net-Worth Individuals

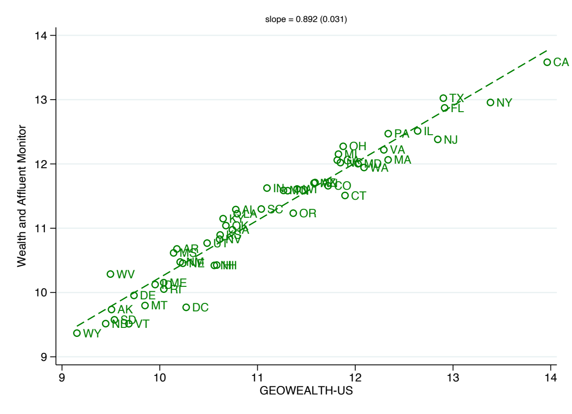
As described in Sections 2.2 and 4.3, our baseline measure of state-level exposure to the federal QSBS reforms is the number of high-net-worth individuals residing in each U.S. state in 2008, based on estimates from the GEOWEALTH-US built by Suss et al. (2024). The authors define HNWIs so as to resemble the Securities and Exchange Commission's definition of accredited investors, including in this definition all individuals whose estimated net worth (excluding the estimated value of their primary residence) exceeded \$1 million or whose observed household income exceeded \$300,000. We validate their estimates by comparing them with alternative estimates from the Phoenix Marketing International/MarketCast Wealth and Affluent Monitor, the Forbes 400 rich lists, the Credit Suisse/UBS Global Wealth Report, and the Survey of Consumer Finances.

Appendix Figure D1a plots the relationship between the average log number of HNWIs residing in each U.S. state from 2006 to 2019 based on the GEOWEALTH-US and the analogous measure from the Phoenix Marketing International/MarketCast Wealth and Affluent Monitor. The latter measures the number of individuals with \$1 million or more in investable assets residing in each U.S. state, which is estimated by combining information from the Survey of Consumer Finances with data from Nielsen/Claritas. Reassuringly, the slope of the relationship between the two measures is close to 1 (0.9). We find a similar—albeit noisier—slope (0.8) when we compare the state-level estimates based on the GEOWEALTH-US to the number of residents ranked in the Forbes 400 rich lists (see Appendix Figure D1b). For that, we rely on the digitized and harmonized data on the Forbes 400 rich lists described in Saez and Zucman (2020).

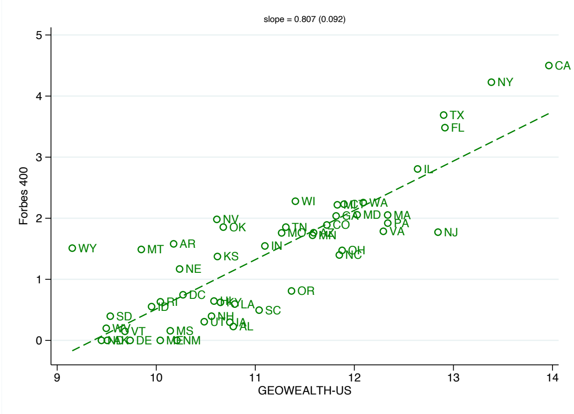
Appendix Figure D1c next compares the evolution of the total number of HNWIs residing in the U.S. based on the GEOWEALTH-US with the analogous measure from the Credit Suisse/UBS Global Wealth Report. The latter measures the number of millionaires residing in the U.S., which is estimated using information from the Flow of Funds Accounts of the Federal Reserve Board. The time series correlation between the two measures is 0.6. We

Figure D1: Number of HNWIs Residing in the U.S. (2006-2019)

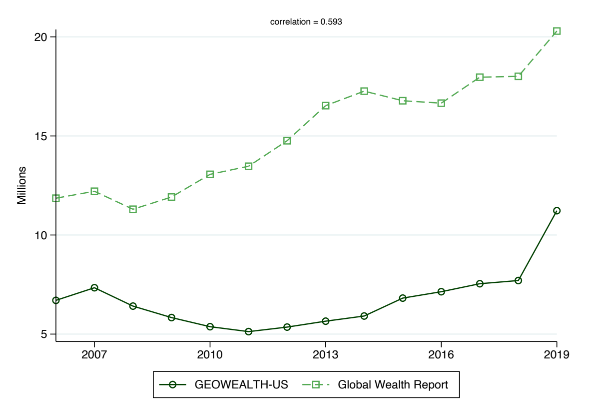
(a) Wealth and Affluent Monitor



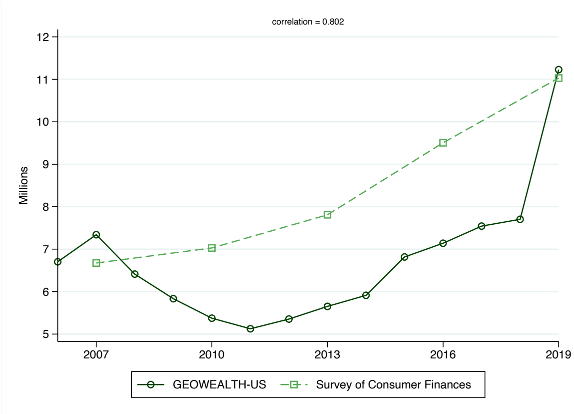
(b) Forbes 400



(c) Global Wealth Report



(d) Survey of Consumer Finances



Sources: GEOWEALTH-US, MarketCast, Forbes, UBS, Survey of Consumer Finances.
Notes: This figure validates our baseline measure of the number of high-net-worth individuals residing in the U.S. from 2006 to 2019, based on the GEOWEALTH-US built by Suss et al. (2024). Panel (a) compares the average log number of HNWIs residing in each U.S. state across years based on the GEOWEALTH-US to the average log number of residents with $1 million or more in investable assets based on the Phoenix Marketing International/MarketCast Wealth and Affluent Monitor. Panel (b) compares the GEOWEALTH-US estimates at the state level to the average log of one plus the number of residents ranked in the Forbes 400 rich lists. Panel (c) compares the total number of HNWIs residing in the U.S. based on the GEOWEALTH-US to the total number of millionaires based on the Credit Suisse/UBS Global Wealth Report. Panel (d) compares the GEOWEALTH-US estimates at the national level to the total number of accredited investors based on the Survey of Consumer Finances.

find an even higher correlation (0.8)—albeit based on fewer, triennial observations—when we similarly compare the nationwide estimates based on the GEOWEALTH-US to the total number of accredited investors based on the Survey of Consumer Finances (see Appendix Figure D1d). As in Appendix Figure A7 and Appendix Section D.3.2, we define accredited investors as households whose net wealth (excluding the value of their primary residence) exceeded $1 million, married households whose combined income exceeded $300,000, and single households whose individual income exceeded $200,000.

D.2 Construction of Distribution Series

D.2.1 Income Distribution

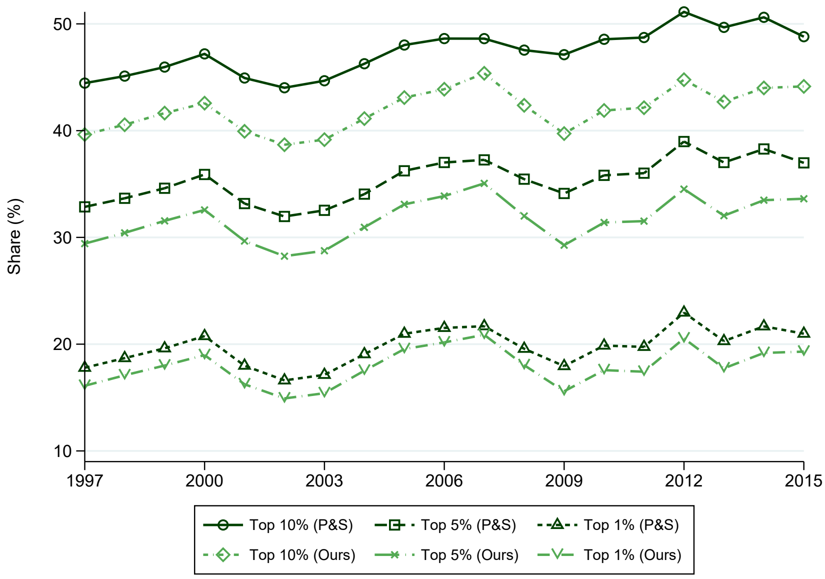
We build pre- and post-tax income distribution series for the U.S. by relying on the historical data tables from the Internal Revenue Service's Statistics of Income (SOI Tax Stats) available for the period 1996-2022. For each state in each year, these data tables provide personal income tax statistics disaggregated by bracket of adjusted gross income (i.e., gross income net of deductions and other adjustments)[47] and by income component (e.g., capital gains, wages, dividends). The data tables also contain information about personal income tax liabilities by adjusted gross income bracket, which we use to construct pre- and post-tax income distribution series at both the state level and federal level.

We build the pre-tax income distribution series by applying the generalized Pareto interpolation (GPI) method developed by Blanchet et al. (2022). This non-parametric approach avoids the assumptions of the Pareto approximation, which are often violated by empirical data. The unit of observation in the SOI Tax Stats is the tax unit. For each state in each year, we construct the state-level pre-tax income distribution across individuals, assuming that the reported household income of couples filing jointly was shared equally between spouses. We calculate the income shares of 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. Our series are consistent with those of Sommeiller and Price (2018), who also build state-level pre-tax income distribution series for the U.S. using the same personal income tax tabulations available between 1917 and 2015 (Appendix Figure D2). The differences between Sommeiller and Price (2018)'s series and ours arise from the fact that they use tax units as the unit of observation, whereas we use individuals. As there are more joint filers at the top of the income distribution, using tax units instead of equal-splitting them—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 adjusted gross income bracket, we further decompose the income distribution series into its different components, namely, realized capital gains and other income (i.e., labor, dividend, interest, and other investment income). We rely on these two aggregated components of income, as the specific subcomponents that are reported in the IRS's raw personal income tax tabulations for each year vary over time. Appendix Table D1 summarizes the different subcomponents that we attribute to each of the two aggregated components.

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

Figure D2: Top Income Shares in the U.S.: Sommeiller and Price (2018)'s Series vs. Ours



Sources: SOI Tax Stats; Sommeiller and Price (2018).
Notes: This figure compares the series for the top income shares in the U.S. that we construct using the SOI Tax Stats with those of Sommeiller and Price (2018) ("P&S"), who build their own series using the same personal income tax tabulations. We run the comparisons for the 1997-2015 period, as these are the years for which both series are available. The differences between Sommeiller and Price (2018)'s series and ours arise from the fact that they use tax units as the unit of observation, whereas we use individuals. As there are more joint filers at the top of the income distribution, using tax units instead of equal-splitting them—as we do in our methodology—increases the concentration of income at the top.

Table D1: Aggregated Components vs. IRS Subcomponents of Income

Component	Subcomponent
Capital gains	Net capital gains (i.e., gains minus losses)
Other income	Salaries and wages; Social security and unemployment payments; Dividends; Taxable interest; Business and professional net income

Sources: SOI Tax Stats.
Notes: This table reports the subcomponents in the IRS's raw personal income tax tabulations from the SOI Tax Stats that we aggregate into two components, namely, realized capital gains and other income.

From the personal income tax tabulations, we can calculate for every adjusted gross income bracket the share of each bracket's total income that is derived from capital gains and other income. We can thus directly apply these shares to the 127 different income groups by matching each income group to its corresponding adjusted gross income bracket. This allows for heterogeneity in the composition of income across the income distribution, with upper income groups obtaining more of their income from realized capital gains relative to lower income groups.

We use a similar procedure to calculate the absolute amounts of income—for both total income and its two components—that accrue to each income group. For that, we multiply each income group's income share by aggregate income to calculate the group's total income, and we further multiply the group's total income by each component's share of it to calculate the group's component-specific income. The procedure thus preserves the heterogeneous composition of income along the income distribution. Following this procedure, we obtain pre-tax income distribution series for total income and its two components for 127 income groups in each state in each year from 2004 to 2022.

To build the nationwide post-tax income distribution series, we start from the nationwide pre-tax income distribution series, which we build by aggregating our state-level income distribution series. Since the personal income tax tabulations from the SOI Tax Stats provide information about personal income tax liabilities by adjusted gross income bracket, we also rely on the GPI method to identify the total personal income tax liabilities of each income group by matching the group to its corresponding bracket. We then construct the post-tax income of each income group by applying to the group's total pre-tax income its average personal income tax rate. We calculate the average personal income tax rate for each income group as the ratio of the group's total personal income tax liabilities to its total taxable income. Based on this procedure, we obtain post-tax income distribution series for 127 income groups in each year from 2004 to 2022. Appendix Figure D3 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 D2 presents summary statistics for the top pre-tax income groups in the U.S. in 2010, 2019, and 2022. Specifically, it reports the pre-tax income shares, as well as the mean, median, and minimum absolute pre-tax income for each subgroup within the top 10%. Appendix Table D3 presents summary statistics for the top post-tax income groups in the U.S. in 2010, 2019, and 2022. Similarly, it reports the post-tax income shares, as well as the mean, median, and minimum absolute post-tax income for each subgroup within the top 10%. The top 0.5% pre-tax income group had an average income

Figure D3: Pre-Tax vs. Post-Tax Income Shares in the U.S.

Sources: SOI Tax Stats.
Notes: This figure compares the series for the top pre-tax and post-tax income shares in the U.S. from 2004 to 2022 that we construct using the SOI Tax Stats.

Table D2: Summary Statistics for the Top Pre-Tax Income Groups in the U.S. (2010, 2019, and 2022)

			Absolute Income (Millions of $)		
Year	Income Group	Income Share	Mean	Median	Minimum
2010	Top 10%	41.89%	1.71	1.02	0.74
	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 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 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: SOI Tax Stats.
Notes: This table presents summary statistics for the top pre-tax income groups in the U.S. in 2010, 2019, and 2022. We calculate both the pre-tax income shares and absolute pre-tax income by applying the generalized Pareto interpolation (GPI) method developed by Blanchet et al. (2022). Pre-tax income shares are reported in percent, while absolute pre-tax income is reported in 2022 U.S. dollars. The table does not report the maximum pre-tax absolute income, as we are using personal income tax tabulations that do not provide information on the maximum absolute income at the very top of the income distribution.

Table D3: Summary Statistics for the Top Post-Tax Income Groups in the U.S. (2010, 2019, and 2022)

			Absolute Income (Millions of $)		
Year	Income Group	Income Share	Mean	Median	Minimum
2010	Top 10%	39.00%	1.39	1.01	0.58
	Top 0.5%	12.02%	8.56	5.90	2.52
	Top 0.1%	6.97%	24.80	14.54	9.50
	Top 0.01%	2.90%	103.2	59.63	38.83
2019	Top 10%	40.46%	1.96	1.11	1.02
	Top 0.5%	12.56%	12.15	8.60	5.06
	Top 0.1%	6.51%	31.48	19.46	13.10
	Top 0.01%	2.53%	122.47	74.81	49.70
2022	Top 10%	41.57%	2.43	1.19	1.09
	Top 0.5%	14.10%	16.50	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: SOI Tax Stats.
Notes: This table presents summary statistics for the top post-tax income groups in the U.S. in 2010, 2019, and 2022. We calculate both post-tax income shares and absolute post-tax income by applying the generalized Pareto interpolation (GPI) method developed by Blanchet et al. (2022). Post-tax income shares are reported in percent, while absolute post-tax income is reported in 2022 U.S. dollars. The table does not report the maximum post-tax absolute income, as we are using personal income tax tabulations that do not provide information on the maximum absolute income at the very top of the income distribution.

of $11.51 million and concentrated 14.06% of total income in 2010. Its income share rose to 17.03% in 2022, with its average income more than doubling to $23.49 million. Due to the progressivity of the personal income tax system, 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 million ($16.50 million) and concentrated 12.02% (14.10%) of total income in 2010 (2022).

D.2.2 Wealth Distribution

We build a nationwide wealth distribution series for the U.S. following the Distributional Financial Accounts (DFA) methodology developed by Batty et al. (2022). The DFA combines two existing Federal Reserve Board statistical products—the triennial wealth distribution measures from the Survey of Consumer Finances, and the quarterly aggregate measures of household wealth from the Financial Accounts of the U.S.—to incorporate distributional information into a national accounting framework. The resulting wealth distribution series are thus consistent with macroeconomic aggregates.

The definitions of assets and liabilities differ between the SCF and the Financial Accounts. Thus, the first step in the harmonization process is to reconcile the definitions of the major asset and liability categories—that is, combining the subcategories of assets and liabilities in the SCF such that their total is analogous to the definition of an asset or liability in the Financial Accounts. Some components such as real estate, checkable deposits, and home mortgages require minimal combinations, while others such as consumer durables require considerable adjustments. Additionally, certain components such as life insurance are measured only indirectly in the SCF and must be imputed from the Financial Accounts. Finally, the SCF does not record defined benefit pension amounts 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 ages and employment status of the members of each household.

We also follow the DFA methodology developed by Batty et al. (2022) to further improve the SCF's ability to capture the top of the wealth distribution by adjusting the very top with the Forbes 400 rich lists. We use data on the Forbes 400 rich lists from Saez and Zucman (2020). We complement these authors' series with those of Moretti and Wilson (2023), who additionally gather the primary industry and source of wealth for each individual ranked in the Forbes 400 rich lists. We also use Moretti and Wilson (2023)'s series for 2004, as Saez and Zucman (2020) do not provide a series for this year.

The Forbes 400 rich lists provide an estimate of only the total net worth of each Forbes 400 household. We therefore need to impute the composition of the assets and liabilities of the Forbes 400 households. We do so by assuming that the Forbes 400 households have similar portfolios of assets and liabilities to those of the households in the top 0.1% of the SCF distribution. After combining the Forbes 400 households with the SCF households in this way, we finally rescale each component of households' assets and liabilities proportionally so as to match its value with that in the Financial Accounts. Thus, the final wealth distribution series are consistent with macroeconomic aggregates and appropriately capture the top of the wealth distribution for every wave of the survey from 2004 to 2022.

Appendix Table D4 presents summary statistics for the top wealth groups in the U.S. in 2010 and 2022. In particular, it reports the wealth share, as well as the mean, median, minimum, and maximum absolute wealth for each subgroup within the top 10%. The top 0.5% wealth group had an average wealth of $31.33 million and concentrated 21.74% of total wealth in 2010. Its wealth share rose to 23.22% in 2022, with its average wealth increasing to $48.47 million.

Table D4: Summary Statistics for the Top Wealth Groups in the U.S. (2010 and 2022)

			Absolute Wealth (Millions of $)			
Year	Wealth Group	Wealth Share	Mean	Median	Min	Max
2010	Top 10%	69.92%	5.04	2.80	1.63	98,766.86
	Top 0.5%	21.74%	31.33	19.72	13.23	98,766.86
	Top 0.1%	11.31%	81.31	40.66	29.77	98,766.86
	Forbes 400	2.95%	6,259.35	3,658.03	1,829.02	98,766.86
2022	Top 10%	66.03%	6.89	3.67	2.15	262,393.10
	Top 0.5%	23.22%	48.47	26.78	16.19	262,393.10
	Top 0.1%	13.42%	139.72	70.35	45.41	262,393.10
	Forbes 400	3.05%	10,466.71	5,331.49	2,822.56	262,393.10

Sources: Survey of Consumer Finances, Financial Accounts, Forbes.
Notes: This table presents summary statistics for the top wealth groups in the U.S. in 2010 and 2022. We calculate both wealth shares and absolute wealth based on the Distributional Financial Accounts (DFA) methodology developed by Batty et al. (2022), which combines the SCF with the Financial Accounts of the U.S. and the Forbes 400 rich lists. Wealth shares are reported in percent, while absolute wealth is reported in 2022 U.S. dollars.

D.3 Counterfactual Simulations

D.3.1 Income Inequality Counterfactual Simulations

Methodology. Our baseline counterfactual analyses focus on the top 0.5% income group, consistent with our state-level income inequality analyses in Section 4.3.2. We run five different counterfactual simulations for 2010-2022 (i.e., the years after the first QSBS reform), using as a baseline the U.S. taxable income distribution series that we construct from the SOI Tax Stats (see Appendix Section D.2.1). To ensure that baseline pre-tax income does not fluctuate because of changes in the QSBS capital gains tax exclusion, we use as the baseline in our simulations a modified pre-tax income series that accounts for total (i.e., not only taxable) private capital gains by adding back the total claimable tax exclusions on QSBS capital gains as derived from PitchBook. We start the simulations in 2010, since it is the first post-reform year for which there is a wave of the SCF available.

The income concept that we use to build the baseline taxable income distribution series consists of the sum of realized capital gains and all other income (see Appendix D.2.1). Hence, we cannot simply rely on the accumulated value of high-net-worth individuals' early-stage investments as of the end of each year (see Figure 2a) to run our income inequality counterfactual simulations. The reason is that this measure contains both unrealized and realized gains. Instead, we construct a measure of the capital gains realized in each year by HNWIs exiting their early-stage investments. These realized gains are based on the accumulated returns on each early-stage investment until its observed exit date. Whenever an investment's exit date is not observed, we assume that the investment was exited five years after it was made. This assumption is based on the fact that HNWIs typically hold early-stage investments for about five years before realizing their accumulated returns on them as capital gains income (see Appendix Section B.2.2). We thus obtain a time series of the capital gains realized in each year by U.S. HNWIs exiting their early-stage investments in U.S. companies from 2004 to 2022, with which we can run the income inequality counterfactual simulations.

The first counterfactual assesses how much HNWIs' excess returns on their early-stage investments relative to public stock markets have contributed to the rise in pre-tax income inequality. For that, we reconstruct the pre-tax income distribution series by replacing the pre-tax private capital gains with the pre-tax counterfactual capital gains had HNWIs instead invested in the total return version of the NASDAQ 100. We use the NASDAQ 100 as our baseline counterfactual, since among major U.S. public stock market indices, it puts the most weight on high-growth technology companies and, therefore, most closely resembles the sectoral composition of HNWIs' early-stage investments (see Appendix Figure A8). We also rerun our counterfactual simulations using other major U.S. public stock market indices as well as the Barclay Hedge Fund Index as robustness checks. The second counterfactual quantifies how much the post-tax excess returns have contributed to the rise in pre-tax income inequality. For that, we reconstruct the pre-tax income distribution series by replacing the post-tax private capital gains with the post-tax counterfactual capital gains had HNWIs instead invested in the NASDAQ 100.

The third simulation assesses how much the difference in taxes paid—resulting from the QSBS capital gains tax exclusion—has contributed to the rise in pre-tax income inequality. For that, we reconstruct the pre-tax income distribution series by replacing the taxes paid on private capital gains and adding the taxes paid had HNWIs instead invested in the NASDAQ 100. The difference in taxes paid is driven by both the difference in tax rates on QSBS capital gains and public (or other non-QSBS) capital gains—a tax wedge effect—and the additional amount of taxes paid by HNWIs because of excess returns—a tax base effect. The fourth and fifth simulations finally examine how each of the tax wedge and tax base effects have respectively shaped pre-tax income inequality. For the tax wedge effect, we reconstruct the pre-tax income distribution series by replacing the post-tax private capital gains with the post-tax private capital gains had these gains instead been taxed under the non-QSBS tax schedule. For the additional tax effect, we construct the income distribution series by replacing the taxes paid had private capital gains been taxed under the non-QSBS tax schedule with the taxes paid had HNWIs invested in the NASDAQ 100.

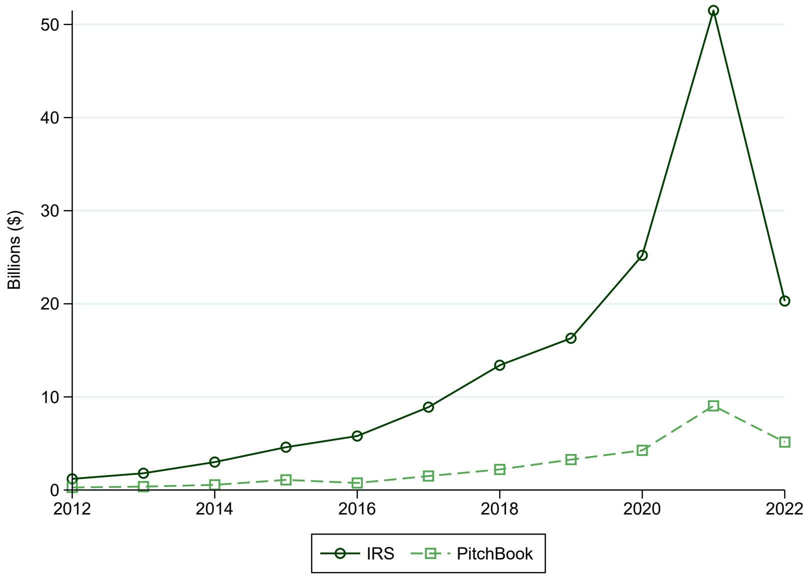
The main challenge that we face when implementing the counterfactual simulations is how to distribute the private and counterfactual realized capital gains along the income distribution in the SCF. This is not an issue for the HNWIs ranked in the Forbes 400 rich lists, since we observe HNWIs' names in PitchBook (see Appendix Section A.3.3). We thus directly attribute to the HNWIs ranked in the Forbes 400 rich lists their realized capital gains on their own early-stage and counterfactual investments. For the remaining HNWIs in PitchBook, we rely on an imputation procedure to identify households in the SCF to which to assign those HNWIs' realized capital gains. Specifically, we consider the accredited investors in the SCF—that is, households whose net wealth (excluding the value of their primary residence) exceeded $1 million, married households whose combined income exceeded $300,000, and single households whose individual income exceeded $200,000—who are full or partial owners of a C corporation or partnership.

When distributing realized capital gains across non-Forbes 400 HNWIs in our income inequality counterfactual simulations, we do not account for the possibility that HNWIs with different levels of private business wealth earned different rates of return, unlike in our wealth inequality counterfactual simulations. The reason is that it is not clear whether there exists a strong correlation between the distribution of realized private capital gains and the distribution of private business wealth. Hence, in our income inequality counterfactual simulations, we instead assume homogeneous realized rates of return for all but the HNWIs ranked in the Forbes 400 rich lists. Specifically, we distribute non-Forbes 400 HNWIs' total realized private capital gains from PitchBook to the accredited investors in the SCF in each year in proportion to private business wealth, but we directly assign to the Forbes 400 HNWIs their own realized capital gains. This methodology ensures heterogeneity in returns coming from the direct assignment of realized capital gains to the Forbes 400 HNWIs and homogeneity from the proportional allocation of realized capital gains across the distribution of non-Forbes 400 HNWIs who own private business wealth. We then rank households into income groups in the SCF, imputing the assigned realized private capital gains to those same income groups in the SOI Tax Stats.

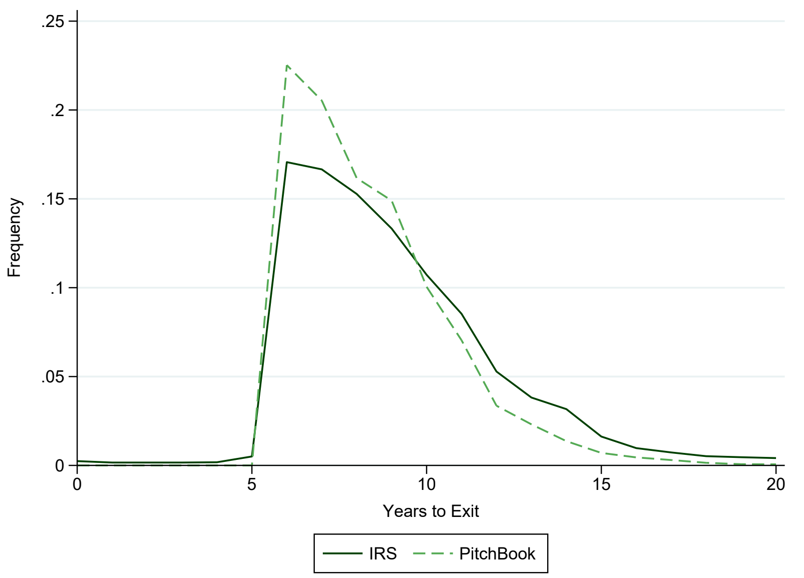
Finally, although PitchBook provides a rich snapshot of HNWIs' early-stage investments and their returns on them, it does not necessarily capture the universe of such investments. We may therefore be missing part of HNWIs' realized private capital gains. To overcome this challenge, we rescale both the realized private and counterfactual public capital gains so that HNWIs' total claimable tax exclusions on QSBS capital gains as derived from PitchBook match the total claimed exclusions as reported in the IRS's aggregated personal income tax data (Abdulrauf et al., 2025). Specifically, we use as a multiplier the ratio of the total claimed exclusions according to the IRS ($152 billion) to the total claimable exclusions as derived from PitchBook ($28 billion) from 2012 to 2022 (see Appendix Figure D4), yielding a rescaling factor of about 5.4. To calculate this $28 billion of HNWIs' total claimable tax exclusions on QSBS capital gains, we rely on our baseline return

Figure D4: HNWIs' Capital Gains from the Sale of QSBS: IRS vs. PitchBook

(a) Total Excluded/Excludable Capital Gains



(b) Distribution of Years to Exit (2012-2022)

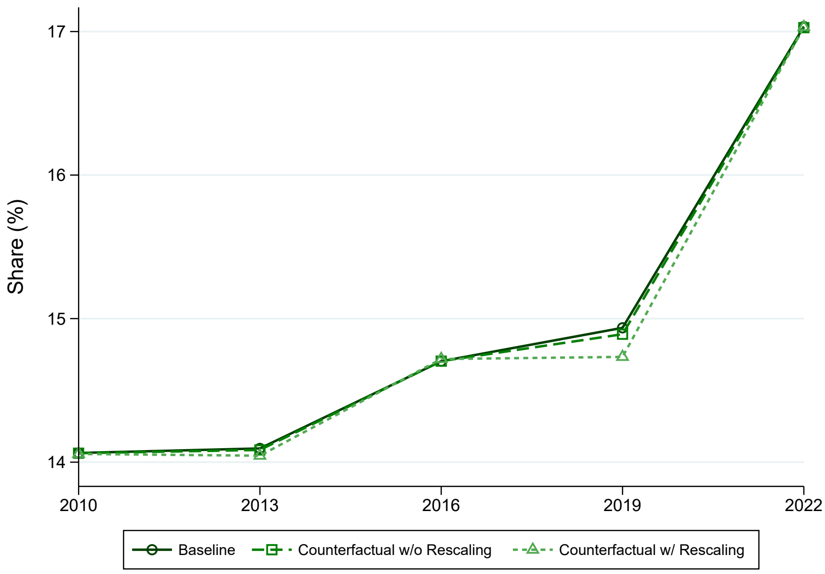


Sources: PitchBook; Abdulrauf et al. (2025).
Notes: This figure compares U.S. HNWIs' capital gains from the sale of QSBS according to the IRS's aggregated personal income tax data to the same capital gains according to PitchBook from 2012 to 2022. Panel (a) compares the total claimed exclusions on QSBS capital gains from Panel A of Figure 1 in Abdulrauf et al. (2025) to the total claimable exclusions that we calculate using PitchBook's data. Panel (b) compares the distribution of years to exit across exited QSBS from Figure 3 in Abdulrauf et al. (2025) to the same distribution that we calculate using PitchBook's data. We identify the early-stage investments held by U.S. HNWIs for at least five 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 total financing by the date of each investment. In Panel (a), we calculate the maximum excludable amount of QSBS capital gains as the greater of $10 million and 10 times the amount invested, and we calculate the excludable capital gains differentiating between the 50%, 75%, and 100% exclusion rates based on the date of the investment. The values in Panel (a) are expressed in nominal terms.

methodology (see Appendix B) and proceed with the following three steps. First, we identify the early-stage investments held by HNWIs for at least five 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 total financing by the date of each investment (see Appendix Section C.1). Second, we calculate the maximum excludable amount of QSBS capital gains as the greater of $10 million and 10 times the amount invested (see footnote 23). Third, we calculate the excludable capital gains, differentiating between the 50%, 75%, and 100% exclusion rates based on the date of the investment (Polsky and Yale, 2023). We run counterfactuals with and without rescaling realized capital gains and focus on the top 10%, top 0.5%, top 0.1%, and top 0.01% income groups.

Results. Appendix Figure D5 plots the results of our first counterfactual simulation. Given that income shares are slow-moving variables, the difference between the baseline top 0.5% pre-tax income share and the counterfactual share excluding excess returns is not very large in absolute terms—with less than 1 percentage point difference in both 2019 and 2022—even for the version using rescaled realized capital gains. We focus on the 2010-2019 period in the remainder of the section, because realizations of private capital gains were particularly low in 2022 (see Appendix Figure D4a). This is consistent with a downturn for startups that year (see Figures 1b and 2b) due to tighter monetary policy (e.g., Ma and Zimmermann, 2023; Abreu et al., 2025).

Figure D5: Top 0.5% Pre-Tax Income Share: Baseline vs. Counterfactuals



Sources: SOI Tax Stats, SCF, Financial Accounts, Forbes, PitchBook, Capital IQ.
Notes: This figure compares the baseline top 0.5% pre-tax income share to the counterfactual top 0.5% pre-tax income share from 2010 to 2022 constructed by replacing realized private capital gains with realized counterfactual capital gains based on the total return version of the NASDAQ 100. We present the counterfactuals both with and without rescaling the realized capital gains.

Table D5: Growth of Top 0.5% Pre-Tax Income Share:
Baseline vs. Counterfactuals (2010-2019)

		Counterfactual: NASDAQ 100				
	Baseline (1)	Pre-Tax Excess Returns (2)	Post-Tax Excess Returns (3)	Tax Savings (4)	Tax Wedge (5)	Tax Base (6)
Panel (a): Without Rescaling						
Absolute growth rate	6.20%	5.89%	5.95%	6.14%	6.21%	6.13%
Share of growth rate	/	4.94%	3.97%	0.96%	-0.23%	1.19%
Panel (b): With Rescaling						
Absolute growth rate	6.48%	4.81%	5.14%	6.16%	6.55%	6.08%
Share of growth rate	/	25.77%	20.71%	4.99%	-1.17%	6.17%

Sources: SOI Tax Stats, SCF, Financial Accounts, Forbes, PitchBook, Capital IQ.
Notes: This table compares the baseline growth rate of the top 0.5% pre-tax income share to the counterfactual growth rate of the top 0.5% pre-tax income share from 2010 to 2019 under five different counterfactual scenarios with respect to the total return version of the NASDAQ 100: (1) pre-tax excess returns, (2) post-tax excess returns, (3) overall tax savings, (4) tax savings arising from the tax wedge between the long-term capital gains tax rate on QSBS and other investments, and (5) tax savings arising from the differences in taxes paid due to excess returns. The table also reports the share of the overall growth in the top 0.5% pre-tax income share explained by each counterfactual. This share is derived as one minus the ratio between the counterfactual growth rate of the income share and the baseline growth rate of the income share. The shares are negative in Column (5) because the counterfactual growth rate is higher than the baseline growth rate. Panel (a) presents the counterfactual growth rates without rescaling realized capital gains. Panel (b) presents the counterfactual growth rates with rescaling realized capital gains. The baseline absolute growth rate is larger in Panel (b) because we are adding back the rescaled total claimable tax exclusions on QSBS capital gains from PitchBook.

The effects are, however, larger when quantifying the contribution of the excess return channel to the overall growth of the top 0.5% pre-tax income share from 2010 to 2019. Appendix Table D5 compares the baseline growth rates of the top 0.5% pre-tax income share from 2010 to 2019 to the growth rates under the five different counterfactual scenarios that we consider. Column (2) shows that excess returns account for approximately 5% (26%) of the growth of the top 0.5% pre-tax income share between 2010-2019 without (with) rescaled capital gains. Column (3) indicates that post-tax excess returns account for 4% (21%) of the growth of the top 0.5% pre-tax income share from 2010 to 2019 without (with) rescaled capital gains. This similarity between the pre-tax excess return and post-tax excess return counterfactuals suggests that HNWIs' tax savings from the QSBS capital gains tax exclusion were not as important a driver of rising pre-tax income inequality as the excess returns themselves. Interestingly, Column (4) reveals that tax savings account for a positive 1% (5%) share of the growth of the top 0.5% pre-tax income share from 2010 to 2019 without (with) rescaled capital gains, indicating that HNWIs would have actually paid less taxes if they had instead invested in the NASDAQ

100. Columns (5) and (6) show that the positive tax savings effect is coming from the positive tax base effect incurred because of higher returns, dominating the negative tax wedge effect. Our results are robust both in terms of direction and magnitude to the use of other public stock market indices and the Barclay Hedge Fund Index as alternative counterfactuals (Appendix Tables D6 and D7). Taken together, these counterfactual simulations suggest that most of the contribution to income inequality is coming from HNWIs' excess returns on their early-stage investments relative to public stock markets, rather than from their tax savings from the QSBS capital gains tax exclusion.

Table D6: Growth of Top 0.5% Pre-Tax Income Share:
Baseline vs. Alternative Counterfactuals without Rescaling (2010-2019)

		Alternative Counterfactuals				
	Baseline (1)	Pre-Tax Excess Returns (2)	Post-Tax Excess Returns (3)	Tax Savings (4)	Tax Wedge (5)	Tax Base (6)
Panel (a): S&P 500						
Absolute growth rate	6.20%	5.84%	5.92%	6.13%	6.21%	6.11%
Share of growth rate	/	5.72%	4.56%	1.15%	-0.23%	1.38%
Panel (b): Russell 2000						
Absolute growth rate	6.20%	5.82%	5.90%	6.12%	6.21%	6.11%
Share of growth rate	/	6.14%	4.88%	1.25%	-0.23%	1.48%
Panel (c): FANG+						
Absolute growth rate	6.20%	5.98%	6.02%	6.16%	6.21%	6.15%
Share of growth rate	/	3.58%	2.94%	0.64%	-0.23%	0.87%
Panel (d): BHFI						
Absolute growth rate	6.20%	5.78%	5.87%	6.11%	6.21%	6.10%
Share of growth rate	/	6.72%	5.33%	1.39%	-0.23%	1.61%

Sources: SOI Tax Stats, SCF, Financial Accounts, Forbes, PitchBook, Capital IQ, Bloomberg.
Notes: This table compares the baseline growth rate of the top 0.5% pre-tax income share to the counterfactual growth rate of the top 0.5% pre-tax income share from 2010 to 2019 under five different counterfactual scenarios with respect to the total return versions of alternative public stock market indices and the Barclay Hedge Fund Index (BHFI): (1) pre-tax excess returns, (2) post-tax excess returns, (3) overall tax savings, (4) tax savings arising from the tax wedge between the long-term capital gains tax rate on QSBS and other investments, and (5) tax savings arising from the differences in taxes paid due to excess returns. Panel (a) considers the S&P 500 index. Panel (b) considers the Russell 2000 index. Panel (c) considers the FANG+ index. Panel (d) considers the BHFI. The table also reports the share of the overall growth in the top 0.5% pre-tax income share explained by each counterfactual. This share is derived as one minus the ratio between the counterfactual growth rate of the income share and the baseline growth rate of the income share. The shares are negative in Column (5) because the counterfactual growth rate is higher than the baseline growth rate. The table presents the counterfactual growth rates without rescaling realized capital gains.

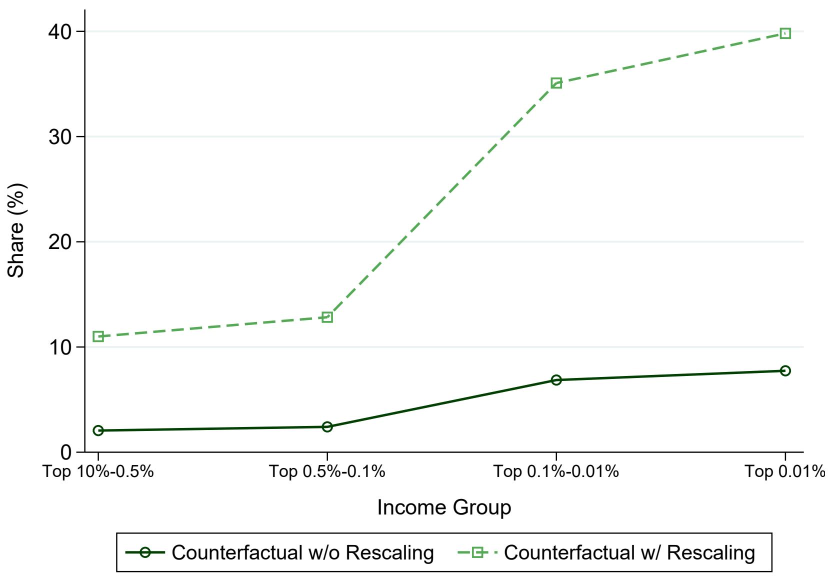
Table D7: Growth of Top 0.5% Pre-Tax Income Share:
Baseline vs. Alternative Counterfactuals with Rescaling (2010-2019)

		Alternative Counterfactuals				
	Baseline (1)	Pre-Tax Excess Returns (2)	Post-Tax Excess Returns (3)	Tax Savings (4)	Tax Wedge (5)	Tax Base (6)
Panel (a): S&P 500						
Absolute growth rate	6.48%	4.54%	4.94%	6.09%	6.55%	6.01%
Share of growth rate	/	29.87%	23.78%	5.99%	-1.17%	7.16%
Panel (b): Russell 2000						
Absolute growth rate	6.48%	4.40%	4.83%	6.06%	6.55%	5.98%
Share of growth rate	/	32.08%	25.47%	6.49%	-1.17%	7.67%
Panel (c): FANG+						
Absolute growth rate	6.48%	5.27%	5.49%	6.26%	6.55%	6.19%
Share of growth rate	/	18.63%	15.28%	3.31%	-1.17%	4.49%
Panel (d): BHFI						
Absolute growth rate	6.48%	4.20%	4.68%	6.01%	6.55%	5.94%
Share of growth rate	/	35.15%	27.82%	7.20%	-1.17%	8.38%

Sources: SOI Tax Stats, SCF, Financial Accounts, Forbes, PitchBook, Capital IQ, Bloomberg.
Notes: This table compares the baseline growth rate of the top 0.5% pre-tax income share to the counterfactual growth rate of the top 0.5% pre-tax income share from 2010 to 2019 under five different counterfactual scenarios with respect to the total return versions of alternative public stock market indices and the Barclay Hedge Fund Index (BHFI): (1) pre-tax excess returns, (2) post-tax excess returns, (3) overall tax savings, (4) tax savings arising from the tax wedge between the long-term capital gains tax rate on QSBS and other investments, and (5) tax savings arising from the differences in taxes paid due to excess returns. Panel (a) considers the S&P 500 index. Panel (b) considers the Russell 2000 index. Panel (c) considers the FANG+ index. Panel (d) considers the BHFI. The table also reports the share of the overall growth in the top 0.5% pre-tax income share explained by each counterfactual. This share is derived as one minus the ratio between the counterfactual growth rate of the income share and the baseline growth rate of the income share. The shares are negative in Column (5) because the counterfactual growth rate is higher than the baseline growth rate. The table presents the counterfactual growth rates with rescaling realized capital gains.

We further analyze whether the contribution of the excess return channel to the overall growth of top pre-tax income shares over the 2010-2019 period was heterogeneous across the income distribution. Figure D6 shows that the excess return channel was more important in explaining the growth in the pre-tax income share of billionaires than that of millionaires. In particular, for the top 10-0.5% and top 0.5%-0.1% pre-tax income groups, excess returns account for only 2% (11%) and 3% (13%) of the overall growth of their pre-tax income shares, respectively, over the 2010-2019 period without (with) rescaled realized capital gains. In contrast, for the top 0.1%-0.01% and for the top 0.01%, realized private capital gains account for 7% and 8% (35% and 40%) of the overall growth of their pre-tax income shares, respectively, over the 2010-2019 period without (with) rescaled realized capital gains. Our results are robust in terms of both direction and magnitude to the use of other public stock market indices and the Barclay Hedge Fund Index as alternative counterfactuals (Appendix Tables D8 and D9).

Figure D6: Heterogeneity in the Share of Growth Explained by Excess Returns across Top Pre-Tax Income Groups (2010-2019)



Sources: SOI Tax Stats, SCF, Financial Accounts, Forbes, PitchBook, Capital IQ.
Notes: This figure depicts how much of the 2010-2019 growth rate in the pre-tax income share of the income groups within the top 10% was accounted for by excess returns (i.e., by replacing realized private capital gains with realized counterfactual public gains based on the total return version of the NASDAQ 100 public stock market index). The share of growth for every income group is derived as one minus the ratio between the counterfactual growth rate of the income share without excess returns and the baseline growth rate of the income share. We present the counterfactual growth rates with and without rescaled capital gains.

Table D8: Heterogeneity in the Share of Growth Explained by Excess Returns across Non-Cumulative Top Pre-Tax Income Groups (2010-2019)

		Counterfactual				
	Baseline (1)	NASDAQ 100 (2)	S&P 500 (3)	Russell 2000 (4)	FANG+ (5)	BHFI (6)
Panel (a): Without Rescaling						
Top 10%-0.5%	4.64%	4.55%	4.53%	4.53%	4.57%	4.52%
	/	2.05%	2.32%	2.46%	1.61%	2.65%
Top 0.5%-0.1%	7.60%	7.41%	7.39%	7.37%	7.46%	7.35%
	/	2.45%	2.77%	2.98%	1.87%	3.25%
Top 0.1%-0.01%	6.58%	6.12%	6.06%	6.03%	6.22%	5.99%
	/	6.92%	7.88%	8.35%	5.40%	9.01%
Top 0.01%	4.11%	3.79%	3.73%	3.70%	3.91%	3.65%
	/	7.71%	9.25%	10.07%	4.81%	11.26%
Panel (b): With Rescaling						
Top 10%-0.5%	4.74%	4.22%	4.15%	4.11%	4.33%	4.06%
	/	11.07%	12.53%	13.30%	8.75%	14.34%
Top 0.5%-0.1%	7.78%	6.79%	6.65%	6.57%	7.03%	6.45%
	/	12.78%	14.51%	15.61%	9.65%	17.08%
Top 0.1%-0.01%	7.01%	4.55%	4.20%	4.03%	5.10%	3.79%
	/	35.09%	40.05%	42.50%	27.27%	45.89%
Top 0.01%	4.36%	2.62%	2.27%	2.09%	3.27%	1.82%
	/	39.86%	47.86%	52.11%	24.93%	58.22%

Sources: SOI Tax Stats, SCF, Financial Accounts, Forbes, PitchBook, Capital IQ, Bloomberg.
Notes: This table compares the baseline growth rates of the non-cumulative top pre-tax income shares to their counterfactual growth rates by replacing realized private capital gains with realized counterfactual capital gains based on total return versions of five different indices: 1) the NASDAQ 100 index, 2) the S&P 500 index, 3) the Russell 2000 index, 4) the FANG+ index, and 5) the Barclay Hedge Fund Index (BHFI). In the pair of rows for each income group in each panel, the first row reports the absolute growth rate, while the second row reports the share of the baseline growth rate explained. The share of growth for every income group is derived as one minus the ratio between the counterfactual growth rate of the income share without excess returns and the baseline growth rate of the income share. Panel (a) presents the counterfactual growth rates without rescaling realized capital gains. Panel (b) presents the counterfactual growth rates with rescaling realized capital gains. The baseline absolute growth rates are larger in Panel (b) because we are adding back the rescaled total claimable tax exclusions on QSBS capital gains from PitchBook.

Table D9: Heterogeneity in the Share of Growth Explained by Excess Returns across Cumulative Top Pre-Tax Income Groups (2010-2019)

		Counterfactual				
	Baseline (1)	NASDAQ 100 (2)	S&P 500 (3)	Russell 2000 (4)	FANG+ (5)	BHFI (6)
Panel A: Without Rescaling						
Top 10%	5.16%	5.00%	4.97%	4.96%	5.04%	4.94%
	/	3.22%	3.70%	3.95%	2.41%	4.30%
Top 0.5%	6.20%	5.89%	5.84%	5.82%	5.98%	5.78%
	/	4.94%	5.72%	6.14%	3.58%	6.72%
Top 0.1%	5.32%	4.94%	4.87%	4.84%	5.05%	4.79%
	/	7.21%	8.40%	9.01%	5.15%	9.88%
Top 0.01%	4.11%	3.79%	3.73%	3.70%	3.91%	3.65%
	/	7.74%	9.29%	10.11%	4.85%	11.29%
Panel B: With Rescaling						
Top 10%	5.32%	4.41%	4.28%	4.21%	4.64%	4.11%
	/	17.04%	19.58%	20.93%	12.74%	22.81%
Top 0.5%	6.48%	4.81%	4.54%	4.40%	5.27%	4.20%
	/	25.77%	29.87%	32.08%	18.63%	35.15%
Top 0.1%	5.66%	3.57%	3.22%	3.04%	4.17%	2.79%
	/	36.96%	43.11%	46.27%	26.34%	50.74%
Top 0.01%	4.36%	2.62%	2.27%	2.09%	3.27%	1.82%
	/	39.81%	47.82%	52.07%	24.87%	58.19%

Sources: SOI Tax Stats, SCF, Financial Accounts, Forbes, PitchBook, Capital IQ, Bloomberg.
Notes: This table compares the baseline growth rates of the cumulative top pre-tax income shares to their counterfactual growth rates by replacing realized private capital gains with realized counterfactual capital gains based on total return versions of five different indices: 1) the NASDAQ 100 index, 2) the S&P 500 index, 3) the Russell 2000 index, 4) the FANG+ index, and 5) the Barclay Hedge Fund Index (BHFI). In the pair of rows for each income group in each panel, the first row reports the absolute growth rate, while the second row reports the share of the baseline growth rate explained. The share of growth for every income group is derived as one minus the ratio between the counterfactual growth rate of the income share without excess returns and the baseline growth rate of the income share. Panel (a) presents the counterfactual growth rates without rescaling realized capital gains. Panel (b) presents the counterfactual growth rates with rescaling realized capital gains. The baseline absolute growth rates are larger in Panel (b) because we are adding back the rescaled total claimable tax exclusions on QSBS capital gains from PitchBook.

D.3.2 Wealth Inequality Counterfactual Simulations

Methodology. To assess the implications of the high-net-worth individuals' increasing participation in private capital markets for the dynamics of U.S. wealth inequality, we carry out counterfactual simulations that compare counterfactual wealth distribution series to the baseline series that we construct (see Appendix Section D.2.2). In particular, we construct the wealth distribution under counterfactual scenarios where U.S. HNWIs had invested in public stock markets instead of in U.S. early-stage companies. We use the NASDAQ 100 as our baseline counterfactual, since among major U.S. public stock market indices, it puts the most weight on high-growth technology companies and, therefore, most closely resembles the sectoral composition of HNWIs' early-stage investments (see Appendix Figure A8). We also rerun our counterfactual simulations using other public stock market indices as well as the Barclay Hedge Fund Index as robustness checks.

To run the counterfactuals, we first subtract from the baseline wealth distribution series the accumulated value of U.S. HNWIs' early-stage investments in U.S. companies by the end of each year, as depicted in Figure 2a and calculated following the return methodology detailed in Appendix B. We then add the accumulated value of their counterfactual investments in the total return version of the NASDAQ 100, as also depicted in Figure 2a.

The main challenge that we face when implementing the counterfactual simulations is how to distribute the accumulated private and counterfactual public capital gains along the wealth distribution in the SCF. This is not an issue for the Forbes 400 HNWIs, since we observe HNWIs' names in PitchBook (see Appendix Section A.3.3). We thus directly attribute to the Forbes 400 HNWIs their accumulated capital gains on their own early-stage and counterfactual investments. For the remaining HNWIs in PitchBook, we rely on an imputation procedure to identify households in the SCF to which to assign those HNWIs' accumulated capital gains. For that, we first need to identify the population of households in the SCF that corresponds to the non-Forbes 400 HNWIs in PitchBook. We thus consider the accredited investors—that is, households whose net wealth (excluding the value of their primary residence) exceeded $1 million, married households whose combined income exceeded $300,000, and single households whose individual income exceeded $200,000—who are full or partial owners of a C corporation or partnership.

When distributing the accumulated capital gains from PitchBook to the accredited investors with private business wealth in the SCF, we account for the possibility that those with different levels of private business wealth earned different rates of return. For that, we rank these accredited investors into 100 percentiles based on their private business wealth. We similarly rank the non-Forbes 400 HNWIs in PitchBook into 100 percentiles based on the accumulated value of their early-stage investments. To ensure that the SCF distribution and the PitchBook distribution are comparable, we drop accredited investors from the bottom of the SCF distribution until the median private business wealth among our selected sample of accredited investors in the SCF matches the median accumulated value of early-stage investments among the non-Forbes 400 HNWIs in PitchBook. We then match percentiles across the two populations and, within each percentile, distribute HNWIs' accumulated capital gains in each year in proportion to private business wealth. Thus, this methodology allows for return heterogeneity both from the direct assignment of capital gains to the HNWIs ranked in the Forbes 400 rich lists and from the differences in returns along the distribution of private business wealth across non-Forbes 400 HNWIs.

Although PitchBook provides a rich snapshot of HNWIs' early-stage investments and their returns on them, it does not necessarily capture the universe of such investments. We may therefore be missing part of HNWIs' accumulated private capital gains. To overcome this challenge, we rescale both the private and counterfactual public capital gains so that HNWIs' total claimable tax exclusions on QSBS capital gains as derived from PitchBook match the total claimed exclusions as reported in the IRS's aggregated personal income tax data (Abdulrauf et al., 2025). Specifically, we use as a multiplier the ratio of the total claimed exclusions according to the IRS ($152 billion) to the total claimable exclusions as derived from PitchBook ($28 billion) from 2012 to 2022 (see Appendix Figure D4), yielding a rescaling factor of about 5.4. To calculate this $28 billion of HNWIs' total claimable tax exclusions on QSBS capital gains, we rely on our baseline return methodology (see Appendix B) and proceed with the following three steps. First, we identify the early-stage investments held by HNWIs for at least five 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 total financing by the date of each investment (see Appendix Section C.1). Second, we calculate the maximum excludable amount of QSBS capital gains as the greater of $10 million and 10 times the amount invested (see footnote 23). Third, we calculate the excludable capital gains, differentiating between the 50%, 75%, and 100% exclusion rates based on the date of the investment (Polsky and Yale, 2023). We run counterfactuals with and without rescaled capital gains for different top wealth groups, in particular, the top 10%, top 0.5%, top 0.1%, and Forbes 400. We focus on the top 0.5% wealth group, consistent with the state-level income inequality analyses in Section 4.3.2.

Results. Figures 8-9 and Tables 3-4 in Section 5.2 report the main results of our wealth inequality counterfactual simulations. Appendix Table D10 further compares the baseline growth rate in the top 0.5% wealth share to the growth rate under alternative counterfactual scenarios based on the S&P 500, Russell 2000, and FANG+ public stock market indices, as well as the Barclay Hedge Fund Index. Appendix Table D11 additionally compares the baseline counterfactual simulation for the top 0.5% wealth share with heterogeneous returns to the same simulation but with homogeneous returns.

Table D10: Growth of Top 0.5% Wealth Share:
Baseline vs. Alternative Counterfactuals (2010-2022)

	Baseline	Alternative Counterfactuals				
		NASDAQ 100	W/ S&P 500	Russell 2000	FANG+	BHFI
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Without Rescaling						
Absolute growth rate	6.80%	6.47%	6.44%	6.42%	6.52%	6.41%
Share of growth rate	/	4.85%	5.29%	5.59%	4.12%	5.74%
Panel B: With Rescaling						
Absolute growth rate	6.80%	5.01%	4.92%	4.78%	5.31%	4.66%
Share of growth rate	/	26.32%	27.65%	29.71%	21.91%	31.47%

Sources: Survey of Consumer Finances, Financial Accounts, Forbes, PitchBook, Capital IQ.
Notes: This table compares the baseline growth rate of the top 0.5% wealth share to the counterfactual growth rate of the top 0.5% wealth share from 2010 to 2022 by replacing private capital gains with counterfactual capital gains based on the total return versions of five different indices: 1) the NASDAQ 100 index, 2) the S&P 500 index, 3) the Russell 2000 index, 4) the FANG+ index, and 5) the Barclay Hedge Fund Index (BHFI). The table also reports the share of the overall growth in the top 0.5% wealth share explained by the excess returns under each scenario. This share is derived as one minus the ratio between the counterfactual growth rate of the wealth share without excess returns and the baseline growth rate of the wealth share. Panel (a) presents the counterfactual growth rates without rescaling realized capital gains. Panel (b) presents the counterfactual growth rates with rescaling realized capital gains.

Table D11: Growth of Top 0.5% Wealth Share:
Counterfactuals under Heterogeneous vs. Homogeneous Returns (2010-2022)

	Baseline	Counterfactual: NASDAQ 100	
		Heterogeneous Returns	Homogeneous Returns
	(1)	(2)	(3)
Panel A: Without Rescaling			
Absolute growth rate	6.80%	6.47%	6.65%
Share of growth rate	/	4.85%	2.21%
Panel B: With Rescaling			
Absolute growth rate	6.80%	5.01%	5.95%
Share of growth rate	/	26.32%	12.50%

Sources: Survey of Consumer Finances, Financial Accounts, Forbes, PitchBook, Capital IQ.
Notes: This table compares the baseline growth rate of the top 0.5% wealth share to the counterfactual growth rate of the top 0.5% wealth share from 2010 to 2022 by replacing private capital gains with counterfactual capital gains based on the total return versions of the NASDAQ 100 index. We consider two different scenarios: one in which we assume that HNWIs' returns on their early-stage investments were heterogeneous across the wealth distribution, and another in which we assume that their returns were homogeneous. The table also reports the share of the overall growth in the top 0.5% wealth share explained by the excess returns under each scenario. This share is derived as one minus the ratio between the counterfactual growth rate of the wealth share without excess returns and the baseline growth rate of the wealth share. Panel (a) presents the counterfactual growth rates without rescaling realized capital gains. Panel (b) presents the counterfactual growth rates with rescaling realized capital gains.

Our baseline counterfactuals after rescaling capital gains assume that the missing investments from PitchBook yielded the same average rate of return as those that we directly observe in PitchBook. To further assess the sensitivity of the excess return channel for wealth inequality, Appendix Figure D7 illustrates the contribution of the channel to the overall growth of the top 0.5% wealth share under alternative assumptions about missing investments and their corresponding rates of return using rescaled capital gains and homogeneous returns. For that, we calculate the missing amounts invested and their corresponding returns for different values of the rescaled counterfactual gap—that is, the difference between total accumulated rescaled private and counterfactual public gains on HNWIs' early-stage investments from 2004 to 2022. We rely on the following equation:

$$\text{rescaling factor} \times R = R + R' = r_{\text{nas}} \times (I + I') + x, \tag{D1}$$

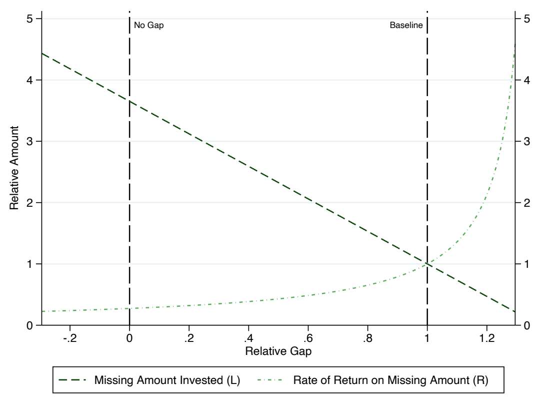
where the rescaling factor is the ratio of the total claimed exclusions according to the IRS to the total claimable exclusions as derived from PitchBook; R is the total accumulated returns on the investments that we observe in PitchBook; R' is the total accumulated returns on the investments missing from PitchBook; r_{nas} is the accumulated rate of return on counterfactual investments in the NASDAQ 100; I is the total amount invested as part of the observed investments; I' is the total amount invested as part of the missing investments; and x is the counterfactual gap. Given the accumulated rate of return on the observed investments $r = R \,/\, I$ and the accumulated rate of return on the missing investments $r' = R' \,/\, I'$, we can insert these expressions into Equation (D1) as follows:

$$\text{rescaling factor} \times R = r_{\text{nas}} \times \left(\frac{R}{r} + \frac{R'}{r'}\right) + x \tag{D2}$$

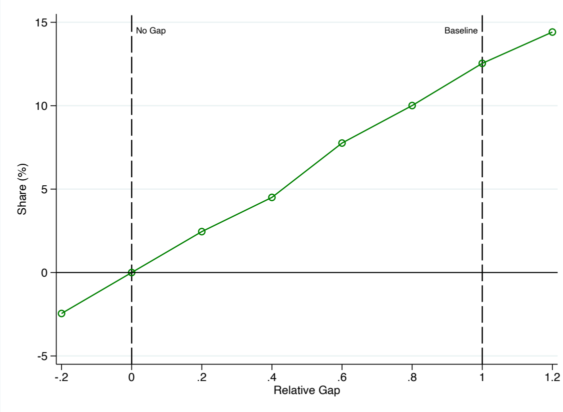
Since we know the values of the rescaling factor, R, r_{nas}, r, and I, we can visualize Equation (D1) in (I', x) space and Equation (D2) in (r', x) space. Appendix Figure D7a plots the missing amounts invested I' on the left axis, their corresponding rates of return r' on the right axis, and the counterfactual gap x on the horizontal axis. All three variables are represented relative to the values we use in our baseline counterfactual simulations with homogeneous returns. Appendix Figure D7b further documents the changing contribution of the excess return channel to the overall growth of the top 0.5% wealth share under different counterfactual gaps—that is, under alternative assumptions about the missing amounts invested and their corresponding rates of return. Appendix Figure D7c plots the correlation between the share of the growth of the top 0.5% wealth share from 2010 to 2022 explained by the excess return channel and the log rate of return on the missing investments relative to that under our baseline rescaling, in which we assume that the missing investments yielded the same rate of return as the observed investments.

Figure D7: Sensitivity of the Excess Return Channel for Wealth Inequality to Alternative Assumptions about the Rescaling

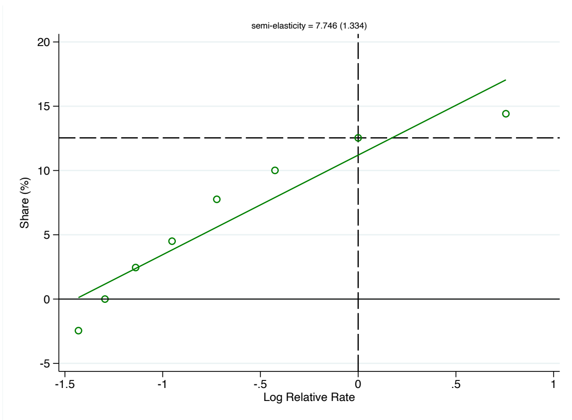
(a) Missing Amounts Invested and their Corresponding Rates of Return



(b) Explained Share of Growth Rate of Top 0.5% Wealth Share vs. Relative Gap



(c) Explained Share of Growth Rate of Top 0.5% Wealth Share vs. Log Relative Rate



Sources: Survey of Consumer Finances, Financial Accounts, Forbes, PitchBook, Capital IQ.
Notes: This figure assesses the sensitivity of the excess return channel for wealth inequality to alternative assumptions about the rescaling, namely, about the missing amounts invested and their corresponding rates of return. Panel (a) visualizes Equation (D1) in (I', x) space and Equation (D2) in (r', x) space, plotting the missing amounts invested I' on the left vertical axis, their corresponding rates of return r' on the right vertical axis, and the counterfactual gap x on the horizontal axis. All three variables are represented relative to their values under our baseline rescaling, which assumes $r' = r$ such that $I' = (\text{rescaling factor} - 1) \times I$. Panel (b) documents how the contribution of the excess return channel to the overall growth of the top 0.5% wealth share changes under different values of the rescaled counterfactual gap, plotting the share of the growth rate of the top 0.5% from 2010 to 2022 explained by the counterfactual on the vertical axis and the rescaled counterfactual gap on the horizontal axis. Panel (c) plots the correlation between the share of the growth of the top 0.5% wealth share from 2010 to 2022 explained by the excess return channel and the log rate of return on the missing investments relative to that under our baseline rescaling.

Finally, we also rerun our wealth inequality counterfactual simulations for different subgroups within the top 10% of the wealth distribution. Specifically, Appendix Tables D12 and D13 compare the baseline growth rates in the wealth share for different subgroups within the top 10% to the growth rates under different counterfactual scenarios based on the NASDAQ 100, S&P 500, Russell 2000, and FANG+ public stock market indices, as well as the Barclay Hedge Fund Index.

Table D12: Heterogeneity in the Share of Growth Explained by Excess Returns across Non-Cumulative Top Wealth Groups (2010-2022)

		Alternative Counterfactuals				
	Baseline (1)	NASDAQ 100 (2)	S&P 500 (3)	Russell 2000 (4)	FANG+ (5)	BHFI (6)
Panel (a): Without Rescaling						
Top 10%-0.5%	-11.13%	-11.05%	-11.05%	-11.05%	-11.07%	-11.04%
	/	0.69%	0.71%	0.72%	0.52%	0.73%
Top 0.5%-0.1%	-6.14%	-6.17%	-6.19%	-6.21%	-6.15%	-6.21%
	/	0.62%	0.96%	1.20%	0.16%	1.17%
Top 0.1%-Forbes 400	24.14%	23.37%	23.34%	23.32%	23.45%	23.29%
	/	3.19%	3.30%	3.39%	2.87%	3.54%
Forbes 400	3.45%	3.36%	3.32%	3.29%	3.45%	3.26%
	/	2.79%	3.91%	4.84%	0.05%	5.62%
Panel (b): With Rescaling						
Top 10%-0.5%	-11.13%	-10.72%	-10.72%	-10.70%	-10.73%	-10.70%
	/	3.62%	3.69%	3.88%	3.54%	3.83%
Top 0.5%-0.1%	-6.14%	-6.69%	-6.70%	-6.86%	-6.55%	-6.99%
	/	8.96%	9.14%	11.73%	6.83%	13.93%
Top 0.1%-Forbes 400	24.14%	20.42%	20.28%	20.16%	20.86%	20.07%
	/	15.42%	15.99%	16.47%	13.60%	16.84%
Forbes 400	3.45%	2.93%	2.71%	2.54%	3.44%	2.39%
	/	15.25%	21.40%	26.50%	0.30%	30.79%

Sources: Survey of Consumer Finances, Financial Accounts, Forbes, PitchBook, Capital IQ, Bloomberg.
Notes: This table compares the baseline growth rates of the non-cumulative top wealth shares to their counterfactual growth rates by replacing private capital gains with counterfactual capital gains based on total return versions of five different indices: 1) the NASDAQ 100 index, 2) the S&P 500 index, 3) the Russell 2000 index, 4) the FANG+ index, and 5) the Barclay Hedge Fund Index (BHFI). In the pair of rows for each wealth group in each panel, the first row reports the absolute growth rate, while the second row reports the share of the baseline growth rate explained. The share of growth for every wealth group is derived as one minus the ratio between the counterfactual growth rate of the wealth share without excess returns and the baseline growth rate of the wealth share. Whenever the growth rate is negative, we report the absolute value of one minus the ratio to ensure consistency. Panel (a) presents the counterfactual growth rates without rescaling realized capital gains. Panel (b) presents the counterfactual growth rates with rescaling realized capital gains.

Table D13: Heterogeneity in the Share of Growth Explained by Excess Returns across Cumulative Top Wealth Groups (2010-2022)

		Alternative Counterfactuals				
	Baseline	NASDAQ 100	S&P 500	Russell 2000	FANG+	BHFI
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): Without Rescaling						
Top 10%	-5.55%	-5.60%	-5.61%	-5.62%	-5.60%	-5.62%
	/	0.90%	1.08%	1.26%	0.90%	1.26%
Top 0.5%	6.80%	6.47%	6.44%	6.42%	6.52%	6.41%
	/	4.85%	5.29%	5.59%	4.12%	5.74%
Top 0.1%	18.74%	18.14%	18.11%	18.09%	18.22%	18.05%
	/	3.20%	3.36%	3.47%	2.77%	3.68%
Forbes 400	3.45%	3.36%	3.32%	3.29%	3.45%	3.26%
	/	2.61%	3.77%	4.64%	0.00%	5.51%
Panel (b): With Rescaling						
Top 10%	-5.55%	-5.84%	-5.86%	-5.89%	-5.75%	-5.93%
	/	5.23%	5.59%	6.13%	3.60%	6.85%
Top 0.5%	6.80%	5.01%	4.92%	4.78%	5.31%	4.66%
	/	26.32%	27.65%	29.71%	21.91%	31.47%
Top 0.1%	18.74%	15.84%	15.68%	15.55%	16.30%	15.45%
	/	15.47%	16.33%	17.02%	13.02%	17.56%
Forbes 400	3.45%	2.93%	2.71%	2.54%	3.44%	2.39%
	/	15.07%	21.45%	26.38%	0.29%	30.72%

Sources: Survey of Consumer Finances, Financial Accounts, Forbes, PitchBook, Capital IQ, Bloomberg. *Notes*: This table compares the baseline growth rates of the cumulative top wealth shares to their counterfactual growth rates by replacing private capital gains with counterfactual capital gains based on total return versions of five different indices: 1) the NASDAQ 100 index, 2) the S&P 500 index, 3) the Russell 2000 index, 4) the FANG+ index, and 5) the Barclay Hedge Fund Index (BHFI). In the pair of rows for each wealth group in each panel, the first row reports the absolute growth rate, while the second row reports the share of the baseline growth rate explained. The share of growth for every wealth group is derived as one minus the ratio between the counterfactual growth rate of the wealth share without excess returns and the baseline growth rate of the wealth share. Whenever the growth rate is negative, we report the absolute value of one minus the ratio to ensure consistency. Panel (a) presents the counterfactual growth rates without rescaling realized capital gains. Panel (b) presents the counterfactual growth rates with rescaling realized capital gains.