Private Capital Markets and Inequality*

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Abstract

This paper studies the relationship between the growth in private capital markets and the rise in economic inequalities over the last two decades in the U.S. First, we document that the share of financing raised by early-stage companies from U.S. high-net-worth individuals (HNWIs) tripled from 2004 to 2022. Second, exploiting both company- and state-level variation in exposure to the expanded federal capital gains tax exclusion on qualified small business stock (QSBS), we find that QSBS-eligible companies' probability of staying private increased by 3.5 percentage points, and that the average income gap between HNWIs and other income earners increased by 7.2%. Third, we show that this rise in income concentration appears to have been driven by HNWIs' excess returns on their early-stage investments relative to public stock market returns. Finally, using counterfactual simulations, we find that HNWIs' excess returns on these investments accounted for 1% and 9% of the growth in the top 0.5% share of post-tax income and wealth, respectively, from 2010 to 2022.

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1 Introduction

Two important stylized facts have marked the last four decades in the U.S. On the one hand, the concentration of income and wealth has steadily risen (Saez and Zucman, 2016; Piketty et al., 2018; Smith et al., 2023; Auten and Splinter, 2024). On the other hand, private capital markets have expanded, while public stock market listings have fallen (Stulz, 2020; Ewens and Farre-Mensa, 2022). The aim of this paper is to examine whether high-net-worth individuals (HNWIs) have increased their participation in private capital markets and, if so, whether their growing presence in these markets could be behind these two key macroeconomic trends.[1]

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

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

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

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

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

We provide four sets of results. First, we show that U.S. HNWIs' participation in U.S. private capital markets has grown considerably in recent decades. This growth has been driven mainly by their investments in early-stage companies, which have increased from about $0.4 billion in 2004 to almost $15 billion in 2022, rather than by their investments in more mature private companies.[3] HNWIs' early-stage investments have increased not only in absolute but also in relative terms. In particular, the share of financing raised by U.S. startups from U.S. HNWIs tripled from about 2% to 6% over the 2004-2022 period.[4] We further document that the accumulated value of HNWIs' early-stage investments since 2004 reached almost $490 billion by 2022, which was more than double their counterfactual value had HNWIs instead invested in either the NASDAQ 100, S&P 500, or Russell 2000 public stock market index or in the Barclay Hedge Fund Index. HNWIs have thus, on average, earned excess returns on their early-stage investments relative to public stock markets or other alternative investments.[5]

Second, we study the relationship between the increase in HNWIs' early-stage investments and the probability that companies stay private. To do so, we exploit the expansion of an existing federal tax exclusion on long-term capital gains from the sale of qualified small business stock (QSBS). This tax exclusion is meant to incentivize HNWIs to invest in early-stage companies, since it applies only to QSBS purchased from companies that satisfy the following three conditions. First, the issuing company must be a U.S. C corporation; second, at least 80% of the company's assets must be actively used in qualified—mainly non-professional and non-extractive—trades and businesses; and, third, the company's gross assets must have never exceeded $50 million, inclusive of the financing raised from the investor purchasing the QSBS. At the exclusion's first introduction in 1993, investors who held QSBS for at least five years could exclude 50% of the first $10 million of the

[3] In the remaining analyses of the paper, we restrict the focus to early-stage financing, as this is the private asset class that has mainly driven the increasing participation of HNWIs in private capital markets.

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

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

associated capital gains. However, in response to the 2008 financial crisis, the federal government temporarily expanded the exclusion rate to 75% in 2009, and to 100% in 2010. The 100% exclusion was made permanent eventually in 2015. Exploiting company-level variation in QSBS eligibility as part of a difference-in-differences design, we find that the QSBS reforms made QSBS-eligible companies 2.4 percentage points more likely to raise financing from U.S. HNWIs and 3.5 percentage points more likely to stay private.

Third, we further exploit the QSBS reforms to establish a link between the increase in HNWIs' early-stage investments and the rise in U.S. income inequality over the last two decades. We follow a two-step approach, estimating the effects of the QSBS reforms on HNWIs' early-stage investments in the first step, and estimating the effects of these investments on income inequality in the second step. In both steps, we exploit state-level variation in exposure to the federal reforms, based on the number of HNWIs who resided in each state in 2008 (i.e., the year immediately prior to these reforms). To avoid additional regulatory burdens, the only individuals from which startups generally raise money are HNWIs. Thus, the reforms should increase HNWIs' investments more in states where the ex-ante number of resident HNWIs is higher. This setting has two main threats for identification. On the one hand, HNWIs may choose to settle in certain states specifically to get access to exclusive new investment opportunities (e.g., aspiring venture capitalists moving to California). On the other hand, resident HNWIs' early-stage investments may be biased toward local startups, which happens to be true in our setting. In this latter case, if the startups in states with more resident HNWIs are exposed to different economic shocks than those in states with fewer resident HNWIs, then startup investments by HNWIs residing in one state may grow faster or slower relative to those by HNWIs residing in another state, and for reasons entirely unrelated to the reforms.

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

Fourth, we document that HNWIs' excess returns on their early-stage investments (relative to public stock market returns) can be associated with the rise in U.S. income concentration over the last two decades. To that end, we first decompose our state-level income measures into three components: realized capital gains, other capital income, and labor income. We then show, using the same state-level design, that the rise in the average income gap between the top 0.5% and bottom 99.5% in the post-reform period was mainly due to a disproportionate rise in the capital gains of the top 0.5%. We further show that this rise in the average capital gains gap between the top 0.5% and the bottom 99.5% is strongly associated with an increase in HNWIs' excess returns from their early-stage investments. We also document, by means of counterfactual simulations, that these excess returns account for 1% and 9% of the overall nationwide growth in the top 0.5% shares of post-tax income and wealth, respectively, in the post-reform period. The effects are stronger for billionaires than millionaires. Finally, we show that the rise in economic inequalities can lead to a further increase in HNWIs' participation in private capital markets, suggesting the existence of a feedback loop between the two phenomena.

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

In this way, we further contribute to the separate literature that focuses on measuring the returns to different asset classes and providing explanations for the heterogeneity in those returns across investors. A number of theoretical papers have suggested that such heterogeneity can be driven by differences in entrepreneurial ability (Lucas, 1978), information (Peress, 2004), or sophistication (Kacperczyk et al., 2019). Several recent empirical studies have instead explored the return heterogeneity within a particular asset class, including household wealth (Bach et al., 2020; Fagereng et al., 2020; Xavier, 2021; Balloch and Richers, 2023), bank deposits (Deuflhard et al., 2019), and stocks (Calvet and Fisher, 2007; Campbell et al., 2019). Our paper is most closely related to prior and contemporaneous studies examining differences in returns between public and private companies (Moskowitz and Vissing-Jørgensen, 2002; Kartashova, 2014; Brown and Kaplan, 2019; Brown et al., 2021; Balloch et al., 2025), most of which have focused on documenting either the under- or outperformance of buyout funds. Our findings that early-stage private companies have outperformed public stock markets in the U.S. over the last two decades are in contrast to those of Moskowitz and Vissing-Jørgensen (2002) for the 1990s, but they are consistent with those of Kartashova (2014) and Balloch et al. (2025) for more recent periods.

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

The rest of this paper is organized as follows. Section 2 first describes the data that we use, while Section 3 documents key stylized facts. Section 4 then analyzes the effects of the QSBS reforms on HNWIs' early-stage investments and companies' decisions to stay private, after which Section 5 discusses the implications of these investments for inequality. Section 6 finally concludes.

2 Data

2.1 Private Capital Market Activity

Pitchbook. Our main data source for private capital market activity is Pitchbook, which is a commercial data provider that collects data on financing deals, the investors and funds participating in these deals, and the companies that they invest in. From 2004 to 2022, it contains information on 448,797 deals for 204,791 US companies, corresponding to 3,114,226 investments by 128,243 distinct investors. Pitchbook collects this data from various sources, including press releases and regulatory filings by companies, Freedom of Information Act requests to public pension funds, and correspondence with the general and limited partners of private investment funds (Cumming and Monteiro, 2023).[6]

First, we use Pitchbook to identify the investors participating in each financing deal and measure the amount that they invested in the deal. We classify investors into HNWIs (i.e., individuals, angel groups, and family offices) and institutional investors (e.g., pension funds, endowment plans, foundations, funds of funds). We also classify private capital market deals as either early-stage (i.e., equity investments in startup companies), private equity (i.e., equity investments in more mature companies), private debt (i.e., debt investments by non-bank entities in the form of non-bond loans), or real assets (i.e., equity investments in real estate, infrastructure, or natural resource assets).[7] Investments in these deals include direct investments in companies by their ultimate investors, as well as intermediated investments by private investment funds. For the intermediated investments, we attribute the investments by each fund to its limited partners, based on the amount committed to the fund by each limited partner. Appendix A contains a detailed description of our data-cleaning procedure for Pitchbook's investment data, including information on companies, deals, investments, and investors.[8]

Second, we also rely on data from Pitchbook to calculate the returns on HNWIs' early-stage investments. Although returns are often calculated at the fund level, we instead emphasize investment-level returns, given that about 82% of the early-stage investments by U.S. HNWIs in U.S companies that we observe between 2004 and 2022 are direct rather than intermediated (see Appendix Table A1). First, we take a similar approach to that of Korteweg and Sorensen (2010), using information on the changing valuation of each company across deals to calculate each company's rate of return. In particular, between any consecutive pair of deals, we compare the company's pre-money valuation from the later deal (i.e., its valuation before accounting for the equity financing that it raised as part of this deal) to its post-money valuation from the earlier deal (i.e., its valuation after accounting for the equity financing that it raised as part of that deal). This comparison accounts for any shareholder dilution between deals, differentiating the growth in the value of the investments by the earlier deal's investors from the value of the new investments by the later deal's investors. Based on this approach, we calculate the historical rate of return on each company. We then calculate the return yielded by each investment in each year by letting the value of each investment in each company

[6] For further details about Pitchbook's data collection process see Pitchbook's website: https://www.pitchbook.com/research-process.

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

[8] We prefer Pitchbook's data to data from other providers (e.g., Preqin, Burgiss) because Pitchbook has more extensive data coverage, especially of early-stage investments by HNWIs. For further details, see Pitchbook's website: https://www.pitchbook.com/compare/pitchbook-vs-preqin.

evolve over time according to the company's historical rate of return. There are instances where the valuation of a company is missing as part of a particular deal, or where the company did not raise financing in a particular year. Appendix B discusses in detail the methodology that we use to impute these missing valuations, as well other aspects of our data-cleaning procedure for Pitchbook's valuation data and our return methodology including robustness checks.

Finally, we use our investment-level dataset to construct a company-level panel dataset for our company-level analyses. In particular, we distinguish between QSBS-eligible and QSBS-ineligible companies by relying on the legal type, primary industry, and financing history of each company. We also use each company's financing history to identify the year in which it was founded, as well as when it first became bankrupt, went public, or was acquired by another company. We use this information for two distinct purposes. On the one hand, since our company-level analysis focuses on companies that were active at the time of the QSBS reforms, the information makes it possible to distinguish these companies from those that had already become inactive or were founded only after the reforms. On the other hand, the same information also enables us to study the effects of the reforms on bankruptcies, initial public offerings, and acquisitions. We provide further details about how we identify QSBS-eligible companies in Appendix C.

2.2 High-Net-Worth Individuals and Inequality

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

Based on observable income and estimated wealth, Suss et al. (2024) define HNWIs to resemble the U.S. Securities and Exchanges Commission's (SEC) definition of accredited investors.[9] This is because, in the view of the SEC, only accredited investors are sophis-

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

ticated enough to invest in unregistered securities like the QSBS issued by early-stage private companies. Private investment funds and companies that raise financing from non-accredited investors are therefore required to register their securities with the SEC.[10]

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

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

We apply the method of generalized Pareto interpolation (GPI) developed by Blanchet et al. (2022). GPI is a non-parametric approach that avoids the assumptions of a Pareto approximation, which are often violated by empirical data. For every state in each year, we construct the state-level income distribution across individuals using data on IRS tax filing units, assuming that the reported household income of couples filing jointly is shared equally between spouses. Our series are consistent with those of Sommeiller and Price (2018), who build state-level income inequality series for the U.S. using personal income tax tabulations from 1917 to 2015 (see Appendix Figure D3). Using the information available on the composition of income for each tax bracket, we further decompose the aggregate income in each income group into labor income, realized capital gains, and capital income (i.e., dividends, interest income, and other income from investments).

We also aggregate our state-level income inequality series at the national level to implement the counterfactual simulations of U.S. income inequality in Section 5.2. Finally, we build nationwide post-tax income distribution series, starting from the federal pre-tax income distribution series. Since the raw IRS Statistics of Income files provide information about the total personal income tax liabilities corresponding to each income bracket, we can

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

subtract the tax liabilities to the total pre-tax income in each bracket and similarly apply the GPI method to build post-tax income distribution series each year from 2004 to 2022.[11]

Survey of Consumer Finances. We also construct a series for the nationwide wealth distribution to implement the counterfactual simulations of U.S. wealth inequality in Section 5.2. We rely on the SCF, which provides a representative picture of the structure of the incomes, assets, and debts of U.S. households. The SCF oversamples individuals at the top of the wealth distribution, enabling a more accurate measurement of the wealth of the wealthiest individuals. The survey is updated every three years and is available between 1989 and 2022. We build the wealth distribution for every wave of the survey between 2004 and 2022 using a measure of net wealth—that is, the sum of private business wealth, public equity, real estate, interest-earning assets, and other financial and non-financial assets, minus all liabilities.[12] To complement our analysis of HNWIs' returns on their early-stage investments based on the data from Pitchbook, we also rely on the SCF to compute their returns on both private and public equity using the methodology used by Moskowitz and Vissing-Jørgensen (2002) and Kartashova (2014).

3 Descriptive Evidence

This section presents descriptive evidence on the evolution of private capital market activity in the U.S., focusing on the growing participation of HNWIs in these markets. We also provide evidence on the returns earned by HNWIs on their early-stage investments, benchmarking them with respect to public stock market returns.

3.1 HNWIs' Increasing Participation in Private Capital Markets

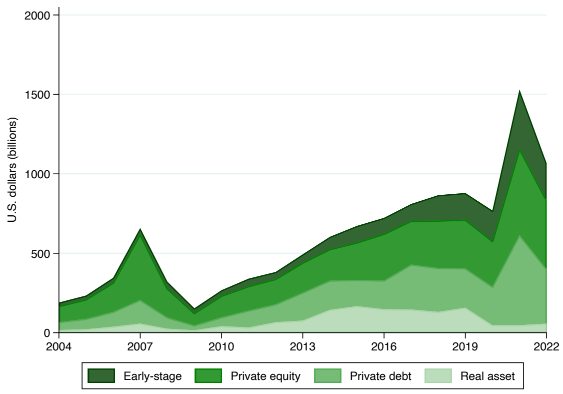
We first describe the evolution of private capital market activity in the U.S. Figure 1a shows that the total financing raised by U.S. private companies grew from about $190 billion in 2004 to about $660 billion in 2007. This growth was mainly driven by private equity, private debt, and real asset financing, rather than by early-stage financing. With the onset of the 2008 global financial crisis, U.S. private capital market activity collapsed to below its initial level. However, it quickly recovered starting in 2010, alongside the broader economic recovery from the crisis. The post-crisis period was marked by the

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

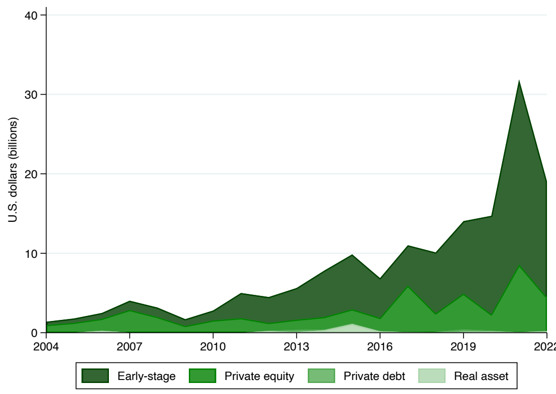
[12] Appendix D.3.1 explains in more detail the methodology used to build the wealth distribution series.

Figure 1: Private Investments in U.S. Companies

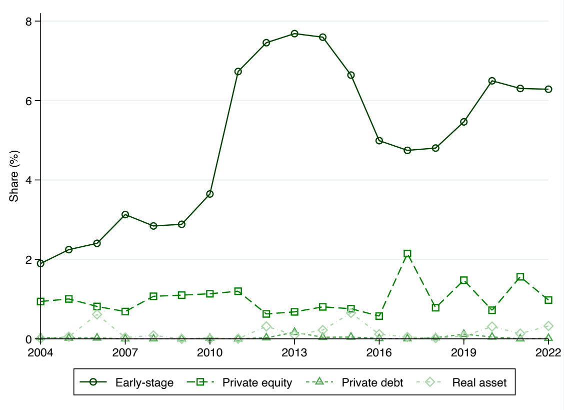
(a) All Investments



(b) U.S. HNWIs' Investments



(c) U.S. HNWIs' Shares of All Investments



Source: Pitchbook.
Notes: This figure shows the evolution of private investments in U.S. companies over the period 2004-2022. Panel (a) depicts the evolution of overall private investments, while panel (b) shows the evolution of U.S. HNWIs investments. Panel (c) depicts the share of U.S. HNIWs investments out of all private investments. Private investments refer to investments by individual investors in early-stage, private equity, private debt, and real asset deals. U.S. companies refer to companies headquartered in the U.S. U.S. high-net-worth individuals (HNWIs) refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S. The values in panels (a) and (b) are expressed in nominal terms (U.S. billion dollars). Panel (b) excludes 9 private equity investments for which the amount invested in the deal by the HNWI exceeded $1 billion and that are therefore outliers.

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 $100-170 billion annually, compared to only $20-45 billion during the 2000s. This trend accelerated during the COVID-19 pandemic, with the total financing raised by early-stage companies peaking in 2021, before the tightening of U.S. monetary policy in 2022 and the subsequent decline in the demand for risky assets.[13]

The rise in early-stage financing has been partly driven by the increasing participation of HNWIs in these markets. Figure 1b shows that the total amount invested annually in U.S. startups by U.S.-based HNWIs was relatively stable between 2004 and 2008. However, it grew from about $1 billion to over $20 billion between 2008 and 2021. In line with the patterns documented in Figure 1a for U.S. private capital markets as a whole, the total amount invested by HNWIs decreased to about $15 billion in 2022. However, HNWIs barely increased their investments in private equity, private debt, or real asset deals over the 2008-2022 period. Early-stage investments have thus become the most important private asset class in HNWIs portfolios. The increase in HNWIs' early-stage investments has been primarily driven by the entry of first-time investors (see Appendix Figure A3a), who account for the vast majority of investments post-2010 (see Appendix Figure A3b). Despite the entry of new investors and the sharp increase in the total amount invested by HNWIs in early-stage companies, the sector composition of these investments has remained remarkably stable (see Appendix Figure A4).

The growth in HNWIs' early-stage investments has outpaced the growth of overall early-stage financing. Figure 1c shows that U.S.-based HNWIs only accounted for approximately 2% of the total financing raised by U.S. early-stage companies in 2004. However, this share spiked to 8% during the mid-2010s, eventually settling above 6% during the early 2020s. HNWIs have therefore emerged as a new and important source of financing for U.S. startups. Even though HNWIs account for a relatively small share of early-stage financing compared to institutional investors, private business wealth is very unequally distributed across households and accounts for a large share of the portfolio of the wealthy (see Appendix Figure A5). Thus, a drastic increase in the private business wealth of the wealthy could significantly contribute to a rise in economic inequalities in the U.S. In Sections 4 and 5, we formally explore the link between the increasing participation of HNWIs in early-stage markets and the rise in U.S. economic inequalities between 2004 and 2022. Throughout the rest of the paper, we focus on investments in early-stage companies,

[13] U.S. companies account for about half of the total financing raised in private capital markets globally (see Appendix Figure A1). U.S. private capital market activity has grown not only in absolute terms but also relative to the overall size of the U.S. economy. Appendix Figure A2 shows that private capital market investments as a share of gross domestic product have increased from about 1.5% in 2004 to 4% in 2022.

as this is the private asset class that has mainly driven the increasing participation of HNWIs in private capital markets.

3.2 Excess Returns on Early-Stage Investments

Next, we present descriptive evidence on HNWIs' returns on their early-stage investments, as well as the counterfactual returns HNWIs would have earned had they instead invested in public stock markets. For that, we follow a similar methodology to Korteweg and Sorensen (2010)—as briefly described in Section 2.1 and detailed in Appendix B—and compare the pre-money valuation of a company as part of a given deal to its pre-money valuation as part of its previous deal. The main threat in the returns estimation stems from missing valuations and survivorship bias. Hence, in our baseline return estimation we adopt a conservative approach and account for both directly observed but also imputed valuations based on observable company characteristics, as well as assume a -100% valuation change for observed companies becoming permanently bankrupt. We consider alternative return estimations in Appendix B.1.2.

Figure 2a plots the total accumulated value of U.S.-based HNWIs' investments in U.S. early-stage companies from 2004 to 2022, which comprises the accumulated value of their initial investments plus their accumulated returns on these investments. It also plots the total value of HNWIs' counterfactual investments in either the NASDAQ 100, the S&P 500, the Russell 2000, or the Barclay Hedge Fund Index. The accumulated value of HNWIs' early-stage investments by 2022 was more than double that of their counterfactual investments in any of the public indices. HNWIs thus earned excess returns on their early-stage investments, relative to the returns that were available in public stock markets.

To explain the divergence between the total value of HNWIs' early-stage investments and that of their counterfactual investments, Figure 2b plots the average rate of return in every year on both sets of investments. Following Phalippou (2024), we calculate the internal rate of return (IRR) on the pooled investments at each 1-year horizon, comparing each investment's start-of-year net asset value (NAV) to its end-of-year NAV. This calculation accounts for the fact that HNWIs enter and exit investments at different points during a year, whereas a mere weighted average of investment-specific rates of return would not. In 13 out of the 19 years from 2004 to 2022, HNWIs' average rate of return on their early-stage investments exceeded the rate that they would have earned on their counterfactual investments in any of the public indices considered. Appendix Figures B1a, B2a, and B3a show that this excess average rate of return on HNWIs' early-stage investments is robust to three alterative return estimations discussed in Appendix B.1.2. In particular, instead of considering observed and imputed valuations and observed bankruptcies (baseline return estimation), we consider observed valuations and observed bankruptcies (alternative return estimation 1), observed valuations, and observed and imputed bankruptcies (alternative return estimation 2), and observed and imputed valuations and observed and imputed bankruptcies (alternative return estimation 3). Furthermore, Appendix Figure B4 shows that the returns on private business equity have also outperformed those of listed equity from 2004 to 2022 using the SCF and the methodology developed by Moskowitz and Vissing-Jørgensen (2002) and Kartashova (2014).

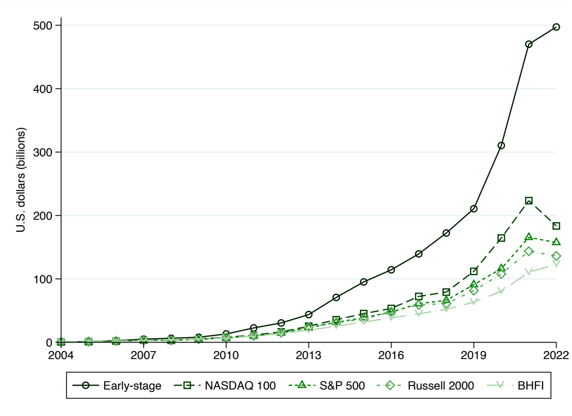
These excess average returns mask substantial heterogeneity across HNWIs' early-stage investments. Figure 2c plots the 5th, 20th, 50th, 80th, and 95th percentiles of the distribution of annualized rates of return across HNWIs investments from 2004 to 2022. Since an investor can exit or enter an investment during the middle of a year, we annualize the rate of return on each investment before calculating the distribution. Whereas the investment at the 5th percentile consistently lost value, and the median investment earned close to a zero return, the investment at the 95th percentile more than doubled—if not tripled or even quadrupled—in value. Our finding that the median early-stage investment by HNWIs earned close to a zero return is consistent with existing evidence on the distribution of early-stage returns in general (Karlsen et al., 2023; Stanley and Øvrum, 2023). Furthermore, Appendix Figures B1c to B3c show a similar pattern of heterogeneity across alternative return estimations. All together, the distribution of returns is therefore highly right-skewed: though half of these investments did not yield positive returns, the minority of investments that did so resulted in outsized capital gains.

Finally, we also calculate risk-adjusted returns to understand if the investors are taking on riskier bets. In section B.2.5, we risk adjust returns in a one-factor dynamic selection model estimated monthly, to account for concerns such as self-selection, survivorship bias, and stale prices, following Korteweg and Sorensen (2010). We present OLS and GLS estimates for comparison. Table B1 shows β in the one-factor model to be 1.0 or 1.1 in the pre- and post-policy periods, respectively. The α ranges from 3.7% (pre-policy) to 3.57% (post-policy). Thus, although α did not change significantly over time, it was positive and statistically significant in both periods. Moreover, the estimate of α yielded by the dynamic selection model is less positive than our OLS and GLS estimates of it, confirming the important role of selection.

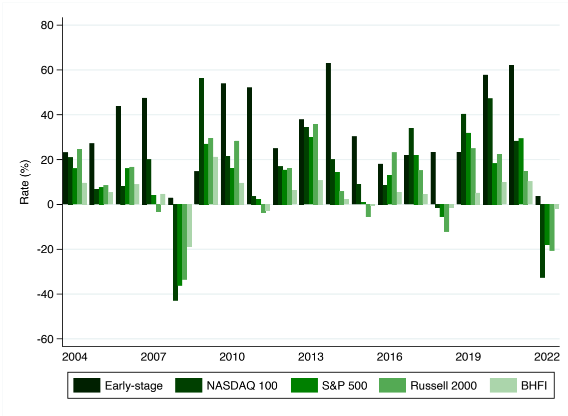
In Section 5, we formally explore whether these excess returns could be behind the recent rise of economic inequalities in the U.S. and quantify its importance.

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

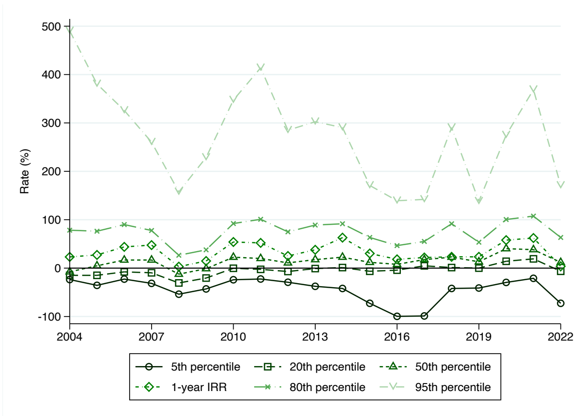
(a) Accumulated Value by End of Year



(b) 1-Year NAV-to-NAV IRR



(c) Distribution of Annualized Rates of Return



Source: Pitchbook, S&P, Bloomberg.
Notes: This figure depicts U.S. high-net-worth individuals' (HNWIs') returns on early-stage investments in U.S. companies from 2004 to 2022. Panel (a) compares the total accumulated value of U.S. HNWIs' early-stage investments in U.S. companies since 2004 with counterfactual scenarios where HNWIs had instead invested the same initial amounts in the NASDAQ 100, the S&P 500, the Russell 2000, or the Barclay Hedge Fund Index (BHFI). Panel (b) shows the 1-year NAV-to-NAV internal rate of return (IRR) of these early-stage and counterfactual investments, calculated following Phalippou (2024). Panel (c) shows various percentiles of the distribution of the annualized rates of return across U.S. HNWIs' early-stage investments in U.S. companies. U.S. companies refer to companies headquartered in the U.S. U.S. HNWIs refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S. BHFI refers to counterfactual returns based on the simple average of the daily rates of return across five Barclay Hedge Fund Indices: the Equity Market Neutral Index, the Distressed Securities Index, the Currency Traders Index, the Convertible Arbitrage Index, and the Equity Long Bias Index. The values in panel (a) are expressed in nominal terms (U.S. billion dollars).

4 Effects of Qualified Small Business Stock Reforms on Private Capital Markets and Inequality

This section exploits the expansions of the U.S. federal capital gains tax exclusion on qualified small business stock (QSBS)— which incentivized HNWIs to invest in early-stage companies in the aftermath of the 2008 financial crisis—as a quasi-exogenous shock to HNWIs' participation in private capital markets. This is the first step in our empirical analysis, before turning in Section 4.2.1 and 5 to our ultimate goal, which is to study how HNWIs' early-stage investments shaped companies' decisions to stay private, as well as the dynamics of inequality in the U.S, respectively. We start this section by providing institutional details about the QSBS reforms and then carry out both company-level and state-level analyses to evaluate the reforms' role in increasing the participation of HNWIs in early-stage markets. We also carry out company-level analyses to study the reforms' effects on U.S. companies, in particular, on their decisions to stay private.

4.1 The QSBS Capital Gains Tax Exclusion

The QSBS capital gains tax exclusion was first introduced by the U.S. federal government in 1993. Set forth in Section 1202 of the U.S. Internal Revenue Code, it is a personal income tax exclusion on the capital gains realized from the sale of QSBS. To qualify for the exclusion, an investor needs to hold the QSBS for at least five years, and the amount of gain eligible for exclusion is limited to the larger of \$10 million or 10 times the acquisition value of the stock. For a company to be categorized as a qualified small business, it needs to meet the following three requirements: (1) it must be an active business that is incorporated as a U.S. C corporation (a type of legal entity that is taxed separately from its owners); (2) it must have had gross assets of \$50 million or less at all times before and immediately after the QSBS was issued; and (3) at least 80% of the company's assets must be actively used in a qualified trade or business.[14] Companies satisfying these requirements tend to be startups in high-growth sectors (e.g., information technology) that are attractive to early-stage investors. The exemption was thus explicitly

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

designed to incentivize investments in such startups (Polsky and Yale, 2023).[15]

Although the QSBS capital gains tax exclusion has been in place since 1993, it was only in the aftermath of the 2008 financial crisis that it become attractive from a tax savings perspective.[16] Figure 3a shows that, from 2004 to 2008, 50% of the first $10 million in capital gains realized from the sale of QSBS were *expected* to be excludable from the federal long-term capital gains tax, while the remaining 50% of gains were expected to be taxable at a fixed 28% rate (i.e., the federal long-term capital gains tax rate at the time of the exclusion's original introduction in 1993).[17] Nevertheless, since the federal tax rate on capital gains from other long-term investments was itself 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 to no incentive to favor QSBS investments over other investments.

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

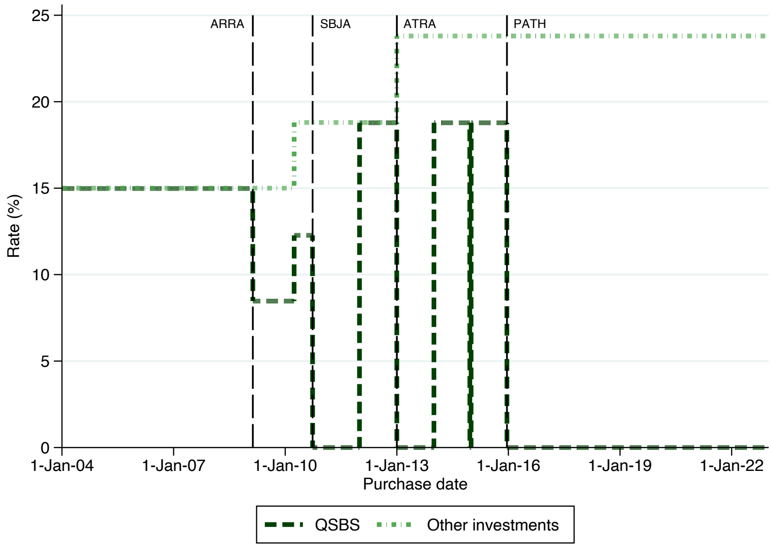
[15] The QSBS exclusion also varies across states due to differences in their state-level long-term capital tax rates or in their decisions to adopt the federal tax rules about the QSBS exclusion for their own state-level tax rules. States either fully conform with federal tax law and apply the same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

[16] Appendix Figure C2 plots the history of the federal tax wedge on QSBS investments all the way back to its original introduction in 1993. The figure shows that cuts to the federal long-term capital gains tax rate in 1997 and 2003 eroded any incentives to favor QSBS investments over other investments.

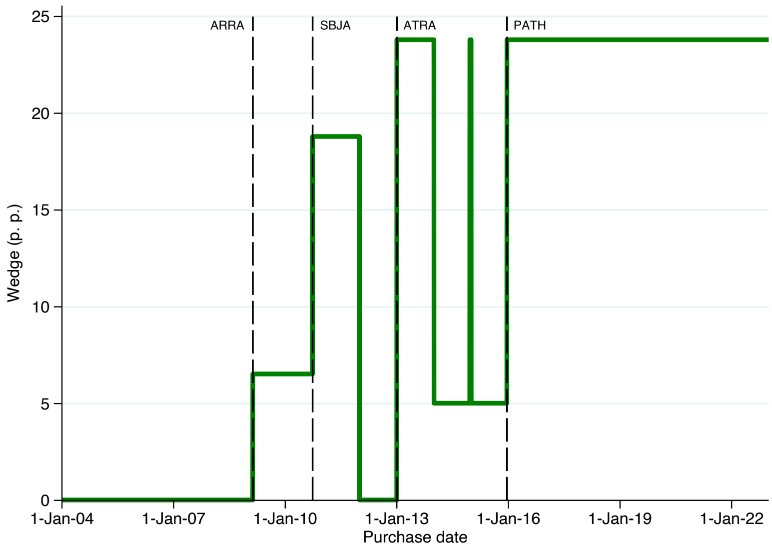
[17] We refer to the tax rates as *expected* rates, since benefiting from the tax exclusion requires at least a five-year gap between the investment date and the selling date and thus individuals face uncertainty about the future evolution of the long-term federal capital gains tax rate from the time of their investment decision. As a result, the tax wedge can either shrink or expand after the investment decision has been made. In contrast, the exclusion rate is determined at the time of the investment.

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

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



(b) Expected Tax Wedge on QSBS



Source: Polsky and Yale (2023).
Notes: This figure compares the evolution of the federal tax exemption on QSBS capital gains between 2004 until 2022, with that of the long-term capital gains tax rate on other alternative investments. Panel (a) depicts the evolution of the two expected rates—QSBS and other investments—separately, while panel (b) plots the difference between the two lines in panel (a), that is, the expected tax wedge. Note that the rates shown are expected and not actual rates, as those are the rates that individuals are expected to be subject to as of the purchase date, but the rates may change (and indeed, they changed) ex-post due to changed in the long-term capital gains tax rate on non-QSBS investments. The dashed vertical black bars indicate the different Acts that changed QSBS legislation, in particular, the American Recovery and Reinvestment Act (ARRA), the Small Business Jobs Act (SBJA), the American Tax Payer Relief Act (ATRA), and the Protecting Americans from Tax Hikes Act (PATH).

4.2 Effects of HNWIs' Early-Stage Investments on Private Capital Markets

4.2.1 Effects on investments

We carry out company-level analyses to first study the extent to which the expanded federal tax exclusion on QSBS capital gains increased HNWIs' investments in QSBS-eligible companies. To that end, we consider the sample of U.S. companies that, as of 2008, were already in existence and that had never before been bankrupt, publicly listed, or acquired by another company. We distinguish between treated and control companies based on their QSBS eligibility. Specifically, treated companies are those that satisfy the three necessary conditions to be a qualified small business: (1) they are legally structured as tax-paying C corporations; (2) they operate primarily in a QSBS-eligible industry; and (3) they have raised no more than $50 million in financing as of or before 2008.[18] In contrast, the controls are other U.S. companies that were active, private, and independent as of 2008 but that failed to satisfy at least one of these three conditions.

We identify the effects of the QSBS reforms on treated companies, relative to control companies, using the following regression:

$$Y_{i,t} = \beta_t \mathrm{QSBS}_i + \alpha_i + \gamma_{\mathrm{corp}(i),t} + \delta_{\mathrm{ind}(i),t} + \zeta_{\mathrm{size}(i),t} + u_{i,t} \tag{1}$$

where $Y_{i,t}$ is the probability of company i raising financing in year t from at least one U.S.-based HNWI, and QSBS_i is a dummy variable equal to 1 for treated companies and 0 for control companies. We also include company fixed effects α_i, corporate structure-year fixed effects $\gamma_{\mathrm{corp}(i),t}$, industry-year fixed effects $\delta_{\mathrm{ind}(i),t}$, and size-year fixed effects $\zeta_{\mathrm{size}(i),t}$, where corp($i$) is a dummy variable for whether company i is a C corporation, ind(i) is a dummy variable for whether i is active primarily in a qualified trade or business, and size(i) is a dummy variable for whether the company had raised no more than $50 million in financing as of or before 2008. If we exclude the coefficient β_{2008} when estimating Equation (1), then we can interpret the parameter of interest β_t as the percentage-point change since 2008 in the probability that QSBS-eligible U.S. companies raised money from at least one U.S. HNWI, relative to QSBS-ineligible U.S. companies.[19]

[18] We use capital raised as a proxy for gross assets—which is the measure with which the tax code determines QSBS-eligible firms—since Pitchbook only has information on gross assets for a very small sample of companies.

[19] We first separately estimate Equation (1) on each founding year cohort of companies, before then taking a weighted average of the cohort-specific effects in each year, with each cohort's weight equal to its share of the companies included in the sample in that year. We do this to address the issue that a single regression pooling different founding year cohorts could risk generating spurious pre-trends, due to the unbalancedness of the sample in the pre-period.

Figure 4a plots our estimates of Equation (1). The estimated coefficients for the years 2004-2007 are statistically insignificant, suggesting the absence of pre-trends. Immediately after the first reform was introduced in 2009, the probability that QSBS-eligible companies raised financing from HNWIs jumped by approximately 3 percentage points, with this effect remaining quite stable until 2022. The average effect over the entire post-reform period was 2.4 percentage points and it is statistically significant at the 1% level. Appendix Table C1 shows the results are robust to the use of different estimators and fixed effects.

Figure 4b depicts instead our estimates of Equation (1) using instead the log accumulated amount of financing raised from U.S. HNWIs as outcome variable. The estimated coefficients for the years 2004-2007 are statistically insignificant, suggesting the absence of pre-trends. The post-reform estimates are only statistically significant starting in 2013, indicating that although QSBS-eligible companies did have a higher probability of raising financing from at least one U.S. HNWI immediately after the reform, the total amount of financing raised from U.S. HNWIs was barely different between QSBS and non-QSBS eligible firms until 2013. The average effect over the entire post-reform period was a 23.7% increase, and it is statistically significant at the 1% level.

4.2.2 Effects on U.S. companies

We then carry out similar company-level analyses to assess the reforms' effects on U.S. companies. Figure 4c reports the reforms' effects on other company-level outcomes using Equation (1). We show that over the entire post-reform period treated companies became, on average, 3.5 percentage points more likely to remain private, 3.6 percentage points less likely to go bankrupt, and 10.5 percentage points less likely of being acquired. This evidence indicates that there appears to be indeed a link between the increasing participation of HNWIs in private capital markets and growth in private capital markets, as companies that receive disproportionately more financing from HNWIs are more likely to stay private and less likely to go bankrupt or be acquired.

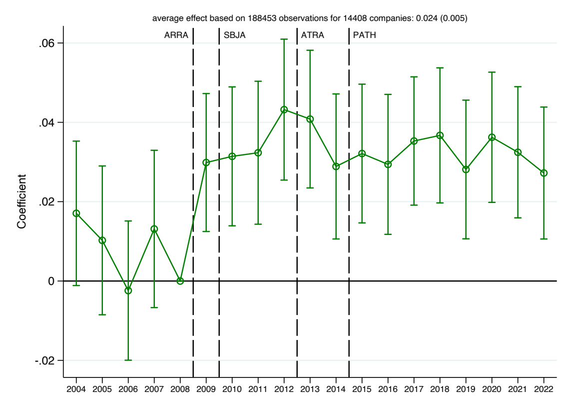
4.3 Effects of HNWIs' Early-Stage Investments on State-level Income Inequality

4.3.1 Effects on Investments

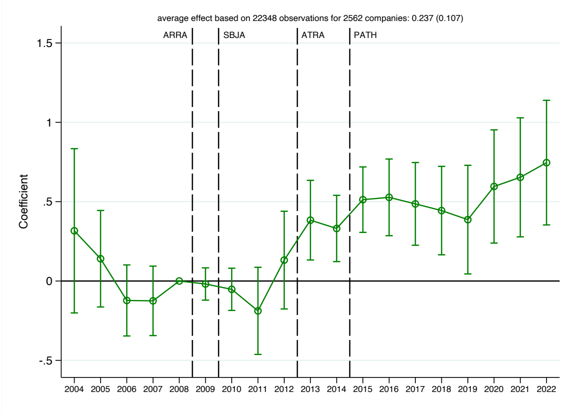
To further assess the effects of the QSBS reforms on the participation of HNWIs in early-stage markets, we next carry out a state-level analysis. The reforms constitute common shocks to HNWIs residing in all U.S. states, since startups generally do not raise

Figure 4: Effects of the QSBS Reforms on QSBS-Eligible Companies

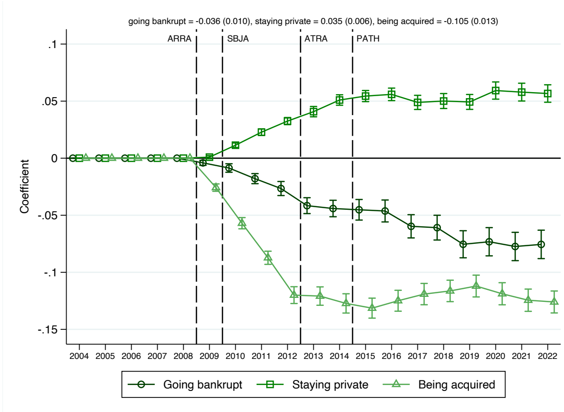
(a) Probability of Raising Financing from At Least One U.S. HNWI



(b) Log Accumulated Amount of Financing Raised from U.S. HNWIs



(c) Probability of Going Bankrupt, Staying Private, and Being Acquired



Source: Pitchbook.
Notes: This figure depicts the effects of the QSBS reforms on various outcomes for QSBS-eligible companies using Equation (1). Panel (a) plots the reforms' effects on a QSBS-eligible company's probability of raising financing from at least one U.S. HNWI in each year. Panel (b) plots their effects on the log accumulated amount of financing (in millions of U.S. dollars) that it raised from U.S. HNWIs. Panel (c) plots their effects on its probability of going bankrupt, staying private, and being acquired. In Panels (a) and (b), companies are considered only from the year in which they were founded until the year in which they first went bankrupt, became publicly listed, or were acquired. In contrast, for each outcome in Panel (c), companies are still considered even after the value of that outcome changes; for example, when estimating the effects on the probability of a QSBS-eligible company going bankrupt, the company is still considered even in the years after the year in which it went bankrupt. After we first estimate a separate regression for each founding year cohort of companies, we then take a weighted average of the estimates across cohorts, setting each cohort's weight equal to its share of all the companies considered in each year. QSBS-eligible companies refer to U.S. companies structured as C corporations, active primarily in a qualified trade or business, and with less than $50 million in total financing raised by the end of 2008. U.S. HNWIs refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S.

financing from less wealthy non-accredited investors to avoid the additional regulatory burdens of doing so (see Section 2.2). We thus rely on the number of resident accredited investors as a proxy for the number of resident HNWIs who could potentially invest in early-stage companies and on the amount invested by resident HNWIs as their actual participation. Following this intuition, the reforms should increase HNWIs' investments more in states where the ex-ante number of resident HNWIs is higher.

Figure 5a visualizes this intuition, which we later formalize in our regression analyses. For both the pre-reform (2004-2008) and post-reform (2009-2022) periods, it plots the relationship between the average annual log millions of dollars invested by resident HNWIs in U.S. startups (on the vertical axis) and the log number of resident HNWIs in 2008 (on the horizontal axis), with each pair of points representing a different U.S. state (including the District of Columbia). We find a relatively large and statistically significant increase in the slope of this relationship after the QSBS reforms, suggesting that HNWIs' participation in early-stage markets increased more in states where there were ex-ante more resident HNWIs who could have entered these markets.

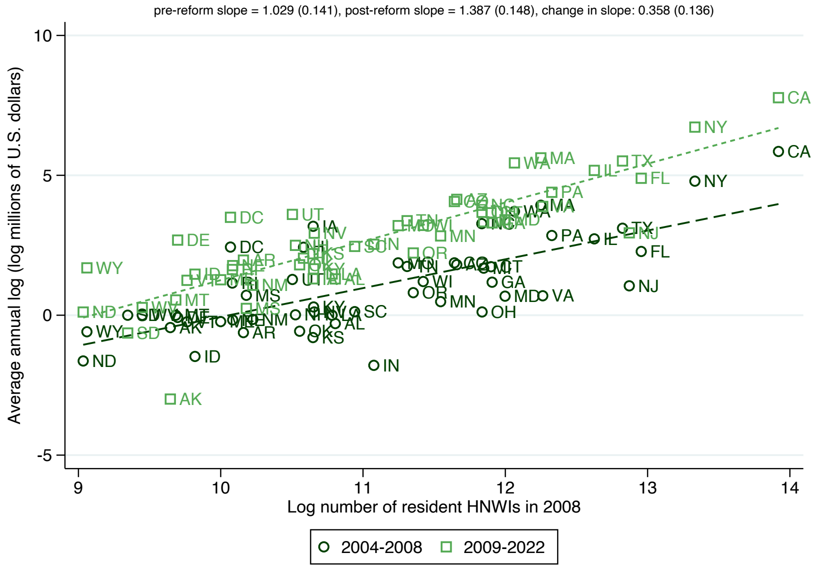
There are two main confounding factors that could also explain this increase in the slope beyond the QSBS reforms. On the one hand, HNWIs may have chosen to settle in certain states to get access to exclusive local investment opportunities (e.g., aspiring angel investors moving to California). This would especially be a threat if HNWIs exhibited home bias and thus had the tendency to invest primarily in local startups. Figure 5b shows that this was indeed the case in the U.S. from 2004 to 2022: in every state, the in-state investment share of HNWIs residing in that state exceeded the share of total early-stage financing from all U.S. investors raised by companies headquartered within that state.[20] On the other hand, given that HNWIs exhibit such home bias, if startups in states with more resident HNWIs were exposed to different economic shocks than those in states with fewer resident HNWIs, then the startup investments by HNWIs in the first set of states may have grown faster for reasons entirely unrelated to the reforms.

To overcome these threats to identification, we analyze how the QSBS reforms affected the in-state investments in early-stage companies by resident HNWIs relative to those by other types of investors—namely, resident institutional investors, non-resident institutional investors, and non-resident HNWIs. This comparison makes it possible to control for interacted state-year fixed effects. Figure 6a shows that, since 2008, in-state investments by resident HNWIs grew more than in-state investments by other investors. Moreover, the amounts invested by resident HNWIs and by other investors increasingly diverged as

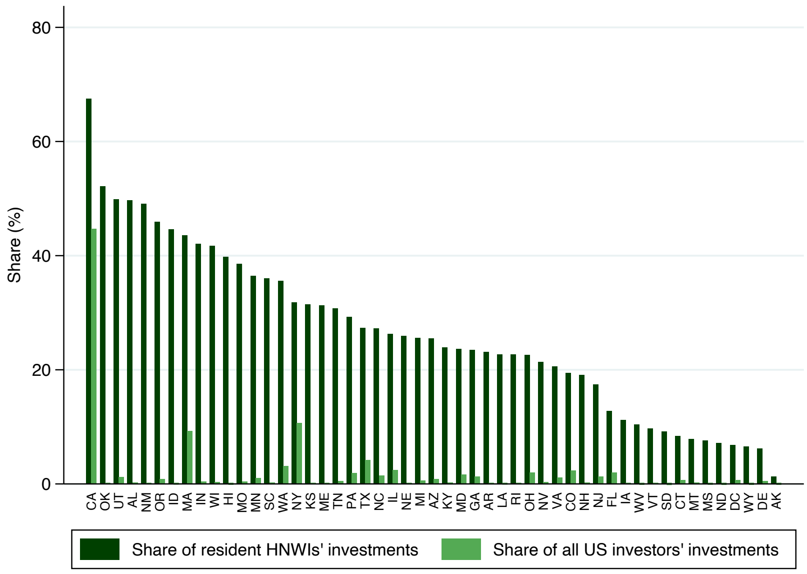
[20] Appendix Figure C1 shows that the same home bias prevails if we focus only on the pre-reform period from 2004 to 2008. Appendix Figure C3 further shows that, other than in the companies headquartered in their own state, HNWIs residing in all states also tend to invest only in companies headquartered in California (where Silicon Valley is located) and, to a lesser extent, Massachusetts.

Figure 5: Motivating How to Evaluate the Effect of the Reforms on Early-Stage Investments by HNWIs

(a) Early-Stage Investments by Resident HNWIs vs. Number of Resident HNWIs



(b) In-State Bias of Early-Stage Investments by Resident HNWIs: 2004-2022



Source: Pitchbook, GEOWEALTH-US.

Notes: Panel (a) plots the relationship between the average annual log millions of dollars invested by resident HNWIs in U.S. startups (on the vertical axis) and the log number of resident HNWIs in 2008 (on the horizontal axis), with each pair of points representing a different U.S. state (including the District of Columbia). State-year observations for which the amount invested is zero are dropped. Panel (b) compares the share of investments by each state's resident HNWIs invested in companies headquartered within that state to the share of investments by all U.S. investors invested in companies headquartered within that same state.

newly enacted legislation ensured the permanence of the 100% QSBS exclusion.

To formalize this finding, we estimate the following regression:

$$\ln Y_{i,s,t} = \beta_t(\ln X_{s,2008} \times \mathbb{1}_{i=\text{resident HNWIs}}) + \alpha_{i,s} + \gamma_{i,t} + \delta_{s,t} + \zeta_{i,t} W_{s,t} + \epsilon_{i,s,t}, \tag{2}$$

where $Y_{i,s,t}$ is the log millions of dollars invested by investors of type i in startups headquartered in state s in year t, $X_{s,t}$ stands for the log number of HNWIs residing in s in 2008, and $\mathbb{1}_{i=\text{resident HNWIs}}$ is a dummy variable equal to 1 for resident HNWIs and to 0 for resident institutions, non-resident institutions, and non-resident HNWIs. We also include investor type-state fixed effects $\alpha_{i,s}$, investor type-year fixed effects $\gamma_{i,t}$, state-year fixed effects $\delta_{s,t}$, and a vector of observable control variables $W_{s,t}$ whose effects $\zeta_{i,t}$ we allow to vary by both investor type and year.[21] If we exclude the coefficient β_{2008} when estimating Equation (2), then we can interpret the parameter of interest β_t as the change since 2008 in the elasticity of resident HNWIs' early-stage investments with respect to the number of resident HNWIs in 2008. In contrast to the change in slope plotted in Figure 4, Panel (b), β_t identifies a relative change in slope, netting out the average change in slope across the other types of investors. Thus, β_t more cleanly identifies the effect of the QSBS reforms on HNWIs' participation in early-stage markets, accounting for potentially confounding shocks to local investment opportunities.

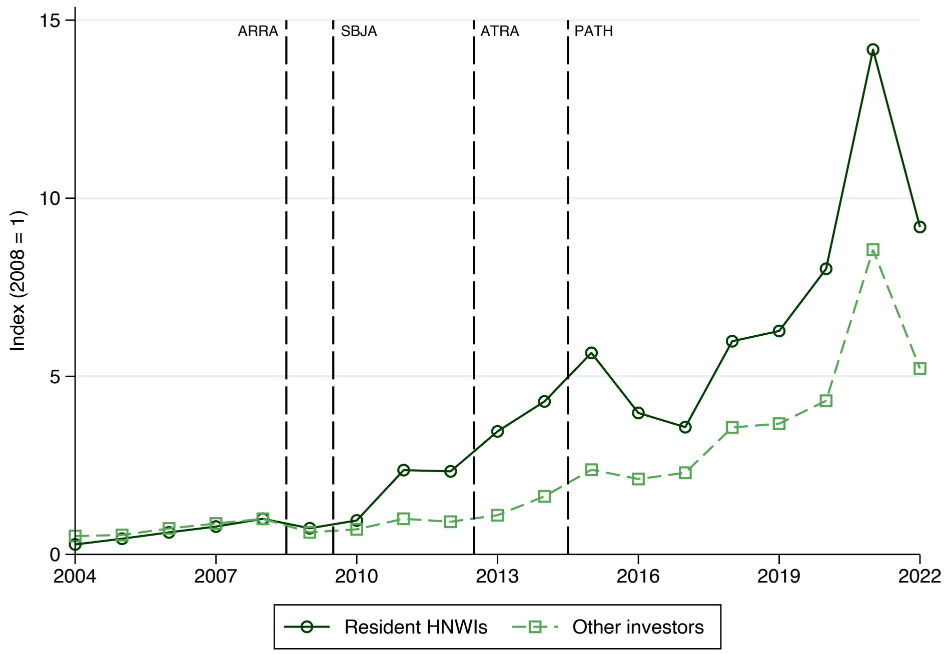
Figure 6b plots our baseline estimates of β_t from Equation (2) for 2004-2022, replacing $\ln Y_{i,s,t}$ with $\ln(1+Y_{i,s,t})$ as the outcome variable to ensure a balanced panel. The estimated coefficients for the pre-reform period are never statistically significant and exhibit no pre-trends. We find an immediate increase in resident HNWIs' early-stage investments after the initial temporary expansions of the QSBS tax exclusion in 2009-2010. We find even further increases during 2013-2014, when the 100% exclusion was repeatedly—but still only temporarily—renewed. Finally, when the full exclusion was made permanent in 2015, our estimated effect reaches its peak, remaining around this elevated level until 2022. The dynamic effects that we estimate are therefore consistent with the actual timing of the policy's introduction.[22] These state-level results are consistent with the increase in

[21] The only control variable that we include in this vector is the state-level long-term capital gains tax wedge on QSBS investments for individuals residing in state s. The purpose of this control variable is to account for likely differences between resident HNWIs and other investors in the responsiveness of their investment activity to any state-specific tax reforms related to QSBS. We further assume that such state-specific reforms were exogenous to resident HNWIs' investment activity.

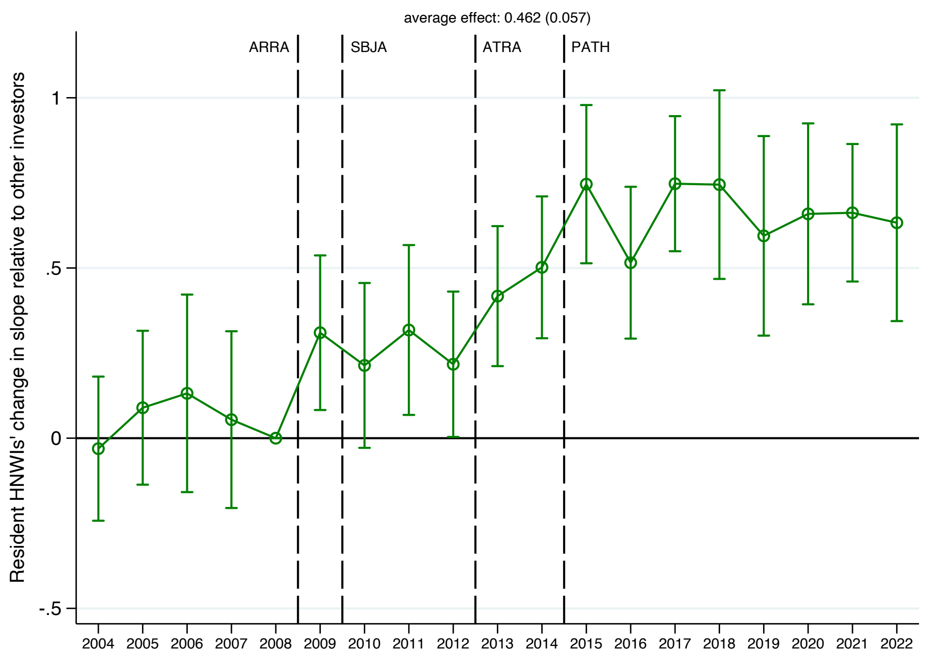
[22] Our baseline estimates are robust to the use of different regression specifications. First, we find similar results when estimating Equation (2) on unbalanced panels with $\ln Y_{i,s,t}$ as the outcome (i.e., dropping observations for which $Y_{i,s,t} = 0$), or with $Y_{i,s,t}$ but using a Poisson pseudo-maximum likelihood (PPML) estimator as suggested by Chen and Roth (2024) (see Appendix Figure C4). The average effect estimated used the PPML estimator is only 0.264 if we compare the post-reform period to the whole pre-reform period. However, this average effect becomes 0.396—close to our baseline estimate of 0.457—if we compare the post-reform period only to 2008. Second, Appendix Figure C5 further shows that the inclusion of the

Figure 6: Effect of the Introduction of the Policy on Resident HNWIs' In-State Early-Stage Investments

(a) In-State Investments by Resident HNWIs vs. Other Investors



(b) Difference in Differences Estimates from Equation (2)



Source: Pitchbook, GEOWEALTH-US.
Notes: Panel A shows the evolution of in-state investments by resident HNWIs vs. other investors over time, indexed to 1 in 2008. Panel B shows the coefficient estimates β_t in Equation (2) for HNWIs, where the dependent variable measures the log millions of dollars invested. The bars represent 95% confidence intervals. Standard errors are clustered at the state level. The average effect reported in Panel B is based on a modified version of Equation (2) where β_t is replaced with $\beta_{t:t>2008}$.

QSBS-eligible companies' probability of raising financing from HNWIs documented in Section 4.2.1.

Finally, we quantify the scale of the effects of the QSBS reforms relative to the overall growth in early-stage investments by HNWIs. We start by replacing the dynamic coefficient β_t from Equation (2) with the static coefficient $\beta_{t:t>2008}$ to obtain an average effect of 0.462. With approximately 6.4 million accredited investors residing in the U.S. in 2008, this estimate implies that the QSBS reforms explain $(6.5 \times 10^6)^{0.462} \approx \$1{,}400$ million = \$1.4 billion of the increase in early-stage investments by HNWIs between the average pre-reform year and the average post-reform year. Since the overall increase was \$6.5 billion (from \$0.8 billion to \$7.3 billion per yer on average), the QSBS reforms account for $1.4/6.5 \approx 21.4\%$ of the increase in HNWIs' participation in early-stage markets after 2008.

4.3.2 Effects on Income Inequality

This section studies the implications of the QSBS reforms on state-level income inequality. We rely on the taxable income distribution that we construct for every state in every year using the IRS Statistics of Income (SOI Tax Stats, see Appendix D.2.1). In particular, we decompose the distribution into 103 income groups, with each of the first ninety-nine groups conforming a percentile, while the top percentile is further split into four groups covering the 99th to 99.5th, 99.5th to 99.9th, 99.9th to 99.99th, and 99.99th to 100th percentiles. We split the top percentile because average income differs drastically across these four income groups. We then calculate the average taxable income $Y_{g,s,t}$ (in thousands of dollars) of the individuals belonging to each income group $g \in \{1, \ldots, 99, 99.5, 99.9, 99.99, 100\}$ of the distribution in state s in year t. Ultimately, we run the following regression:[23]

$$\ln Y_{g,s,t} = \alpha_{g,s} + \beta_t(\ln X_{s,2008} \times \mathbb{1}_{g>99.5}) + \gamma_{g,t} + \delta_{s,t} + \zeta_{G(g),t} W_{s,t} + \epsilon_{g,s,t}, \tag{3}$$

state-level long-term capital gains tax wedge on QSBS investments as a control variable meaningfully alters only the PPML estimates (Panel (c)) but not the ordinary-least-squares estimates with either $\ln(1 + Y_{i,s,t})$ (Panel (a)) or $\ln Y_{i,s,t}$ (Panel (b)) as the outcome. This suggests that state-specific reforms related to QSBS may affect the extensive margin of resident HNWIs' participation in early-stage markets (i.e., do any resident HNWIs participate in the market?) but not its intensive margin (i.e., how much do participating HNWIs actually participate?). Third, we also show that the increase in the triple-difference parameters identified in Equation (2) is driven by an increase in early-stage investments by resident HNWIs, rather than by a decrease in investments by other investors—exactly as we would have expected, given the policy's design (see Appendix Figure C6). Finally, we show that our baseline estimates are also robust to dropping California and the eight other states containing cities that are major U.S. technology hubs (see Appendix Figure C7).

[23] As we discuss in Section 4.1, only the first \$10 million of the long-term capital gains from each QSBS investment are exempted from the personal income tax. However, as we discuss in Section 5.2, QSBS investments that yield at least \$10 million in capital gains tend to yield much more than just \$10 million. We therefore expect the majority of QSBS capital gains to be taxable income.

where $X_{s,t}$ is the number of accredited investors residing in state s in 2008; $\mathbb{1}_{g>99.5}$ is a dummy variable equal to 1 for income groups $g > 99.5$ in the top 0.5% of the state-level income distribution, and 0 otherwise; $W_{s,t}$ is a vector of time-varying state-level controls whose dynamic effects $\zeta_{G(g),t}$ are the same for all income groups g within decile G of the distribution, except for the top income groups $g > 99.5$ that are assigned to their own G; and $\alpha_{g,s}$, $\gamma_{g,t}$, $\delta_{g,t}$ are interacted group-state, group-year, and state-year fixed effects, respectively. We can therefore interpret the parameter of interest β_t as the effect of the reforms on the average (log) income gap between the top 0.5% and bottom 99.5% of the state-level income distribution, which we choose to further interpret as the income gap between HNWIs and other income earners.[24]

Figure 7a plots our baseline estimates of Equation (3), replacing $\ln Y_{g,s,t}$ with the asinh$Y_{g,s,t}$ transformation to ensure a balanced panel, given that average income is negative for 1% of the observations.[25] We find that the average income gap between HNWIs and other income earners grew more after the reforms in those states with more ex-ante resident accredited investors than in those with fewer accredited investors.[26] This rise in income inequality is consistent with the findings that we presented previously—namely, that resident HNWIs' investments in local startups increased disproportionately more in those same states (see Section 4.1), and that these investments yielded excess returns relative to the returns that were available in public stock markets (see Section 3.2).

The increase in income inequality peaked five years after the QSBS reforms were introduced. This is consistent with the fact that investors needed to hold their QSBS investments for at least five years before selling them in order to benefit from the capital gains tax exemption (see Section 4.1). To corroborate that this increase in income inequality was driven by an increase in HNWIs' capital gains, we decompose the average taxable income for each income group into capital gains and all other types of income—namely, labor and capital income. Figure 7b shows that the gap between the capital gains of the top 0.5% and the bottom 99.5% increased much faster after the reforms than the gap for the other income component.[27] This increase in the inequality of capital gains was driven by an increase in the gains of the top 0.5%, rather than by a decrease in those of the bottom

[24] Our decision to split the state-level income distribution at the 99.5th percentile is not arbitrary. It is motivated by the fact that this is the largest top income group for which in every state and year the average income of individuals exceeded \$200,000—the individual income threshold for qualifying as an accredited investor (see Section 2.2). Hence, we are certain that accredited investors belong to that income group across all states and years.

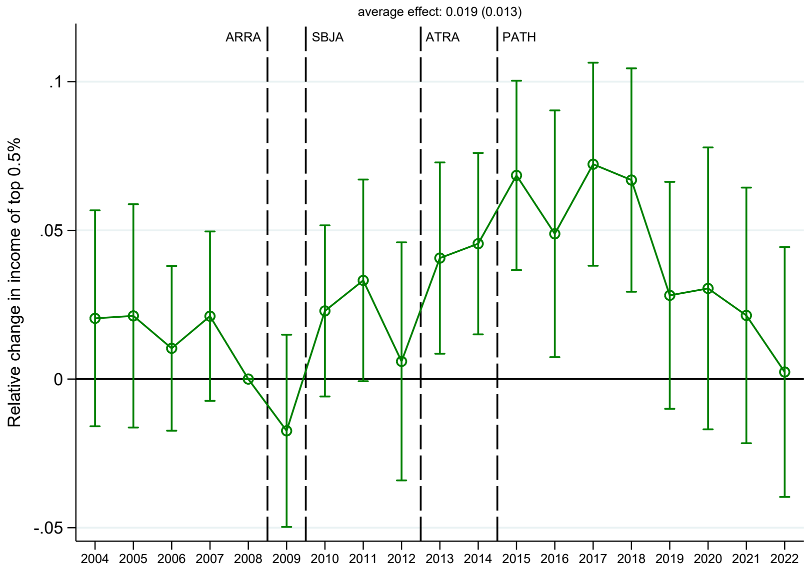
[25] Appendix Figure D5 shows that our baseline estimates using the asinh $Y_{g,s,t}$ transformation are robust to the alternative transformation $\ln Y_{r,s,t}$ based on unbalanced panels, as well as to asinh$(1000 \times Y_{r,s,t})$.

[26] We further show that this increase in income inequality was driven by an increase in the income of the top 0.5%, rather than by a decrease in that of the bottom 99.5% (see Appendix Figure D6a).

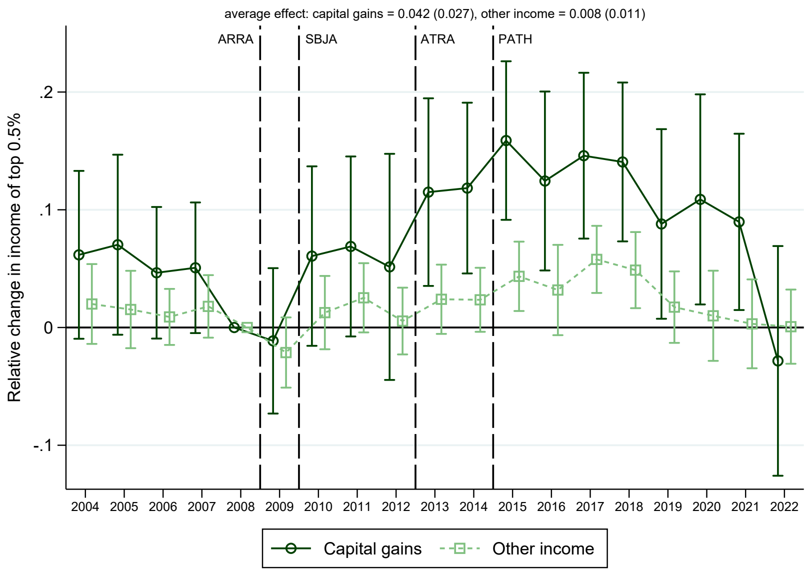
[27] We distinguish between the estimated effects on labor and capital income in Appendix Figure D7.

Figure 7: Income Gap between the Top 0.5% and Bottom 99.5% of the State-Level Income Distribution after the QSBS Reforms

(a) Total Income



(b) Capital Gains vs. Other Income



Source: SOI Tax Stats, GEOWEALTH-US.
Notes: This figure shows the coefficient estimates for β_t from Equation (3), which document the evolution of income of the top 0.5% in a given state over time post the QSBS reforms. The regression controls for time-varying state-level controls, interacted income group-state, income group-year, and state-year fixed effects. Panel (a) shows total income, while panel (b) shows capital gains and other income. Other income includes labor income and capital income. Bars represent 95% confidence intervals, and standard errors are clustered at the state level. The average effect reported in the figures is based on a modified version of Equation (3) where β_t is replaced with $\beta_{t:t>2008}$.

99.5% (see Appendix Figure D6b).[28]

For further reassurance, we also compare this increase in the inequality of capital gains to the growth in residents HNWIs' returns on their early-stage investments. Specifically, we replace the term $\beta_t(\ln X_{s,2008} \times \mathbb{1}_{g>99.5})$ in Equation (3) with $\beta \ln R_{s,t}$, where $R_{s,t}$ is the average returns earned in year t by HNWIs residing in state s from their accumulated investments in local startups.[29] Using the transformations asinh $Y_{g,s,t}$ and asinh $R_{s,t}$ to ensure a balanced panel, we estimate $\beta = 0.059$ (significant at the 1% level). This suggests that, for every 10% increase in HNWIs' early-stage returns, the gap between the capital gains of HNWIs and other income earners increased by about 0.6%.

Finally, we quantify by how much the QSBS reforms increased the average income gap between HNWIs and other income earners. For that, we compare our estimate of the reforms' average effect on resident HNWIs' investments in local startups (Figure 6b) to our estimate of its effect on state-level income inequality (Figure 7a). We find that the income gap between the top 0.5% and bottom 99.5% increased by $0.019/0.462 \approx 4.1\%$ for every 100% increase in HNWIs' early-stage investments. Since the reforms increased these investments by about \$1.4 billion per year relative to the pre-reform average of \$0.8 billion (see Section 4.1), they also increased this income gap by $1.4/0.8 \times 4.1\% \approx 7.2\%$.

5 Inequality Counterfactuals

The reduced-form analyses in Section 4.3.2 assessed the implications of the growing participation of HNWIs in private capital markets on state-level income inequality. However, they do not help us understand the overall effect on U.S. income (both pre- and post-tax) nor wealth inequality. In this section, we run counterfactual simulations to quantify how the increase in the participation of HNWIs in private capital markets has shaped overall

[28] In Appendix Figures D8 and D9, we show that our baseline estimates of the effect of the policy on the capital gains of the top 0.5% are sensitive to the specific transformation of the outcome—namely, asinh $Y_{r,s,t}$, $\ln Y_{r,s,t}$, or asinh$(1000 \times Y_{r,s,t})$—that we choose to use. This is because a higher share of observations were negative for capital gains (4.9%) than for overall taxable income (1.0%), especially in the first years after the QSBS reforms. These years coincide with the immediate aftermath of the 2008 financial crisis, explaining why a higher share of the observations for capital gains were negative.

We prefer the transformation asinh $Y_{r,s,t}$ for three reasons. First, the high share of negative observations makes the $\ln Y_{r,s,t}$ transformation inappropriate. Second, when we use the asinh$(1000 \times Y_{r,s,t})$ transformation, the difference-in-difference estimates for the bottom 99.5% are implausibly negative, with these estimates driving the implausibly positive estimates of the triple-difference coefficients. Finally, the IRS Statistics of Income report the aggregate income in each range of the state-level income distribution in thousands of dollars, making it more appropriate to measure the outcome variable in this unit.

[29] We calculate average returns as the thousands of U.S. dollars earned by resident HNWIs on their accumulated investments in local startups (as measured from Pitchbook), divided by the number of residents in the top 0.5% of the state-level income distribution (as measured from the Statistics of Income).

U.S. income (pre- and pos-tax) and wealth inequality over the last two decades.

5.1 Income Inequality

To carry the counterfactual simulations for pre- and post-tax income inequality, we rely as baseline on the pre- and post-tax income inequality series we estimate based on the IRS Statistics of Income. The methodology used to build the series is based on Blanchet et al. (2022), and it is briefly explained in Section 2.2 and detailed in Appendix D.2.1. We focus the counterfactual analysis on the top 0.5% income group, to ensure consistency with the state-level income inequality analysis in Section 5.1.

We run two different counterfactual simulations for the post-reform 2010-2019 period using the private and counterfactual public realized taxable gains derived from Pitchbook.[30] First, we re-estimate the pre- and post-tax income inequality series excluding pre- and post-tax private capital gains. We distribute the pre- and post-tax private capital gains proportionally on an annual basis so as to match the total income distribution of accredited investors who are full or partial owners of a C corporation or a partnership in the SCF. Given that 82% of early-stage investors belong to the 10% income group, capital gains are highly concentrated at the top. In particular, the top 10% income group accounts for approximately 93% of total capital gains over the 2010-2022 period. Second, we re-estimate the pre- and post-tax income inequality series replacing pre- and post-tax private capital gains by the counterfactual gains had these money been invested in the NASDAQ Composite Index. Note that because there is no exemption in the tax code for investing in public stocks, these capital gains are 100% taxable.

The main challenge we face when implementing the counterfactuals is how to distribute the actual and counterfactuals realized gains along the income distribution in the SCF. For the Forbes 400 individuals, this is not an issue, as we can match the individuals on the list to their Pitchbook profiles, as explained in Appendix Section A.6. We can thus directly attribute on an individual basis to each Forbes 400 individual the realized capital gains they earn on these investments. For assigning the remaining Pitchbook private business capital gains, we need to rely on an imputation procedure as—with the exception of the Forbes 400 individuals—we cannot match the identities of the Pitchbook investors to the identities of the anonymous households in the SCF. For that, we rank the population of accredited investors who are full or partial owners of a C corporation or a partnership in the SCF into 100 percentiles and distribute the realized private capital gains

[30] Appendix Section D.2.2 describes the methodology used to derive the private and counterfactual public realized taxable gains derived from Pitchbook. We start the simulations in 2010, since it is the first available post-reform year for which there is a wave of the SCF available.

proportionally to income on an annual basis. This methodology ensures heterogeneity in returns coming from the direct assignment of capital gains to the Forbes 400 individuals and from the differences in returns across the private business wealth distribution in the imputation procedure. Appendix D.2.2 explains in more detail the methodology used to carry out the pre- and post-taxanle income inequality simulations.

Figure 8a compares the baseline top 0.5% pre-tax income share to the counterfactual top 0.5% taxable shares under the two different counterfactual scenarios, while Figure 8b provides the same comparison for the top 0.5% post-tax income share. As expected, the top 0.5% pre- and post-tax income shares are lower than the baseline in the counterfactual scenarios in which HNWIs would have invested the money they put in early-stage markets in to the public stock market, and slightly lower, if they would have just not invested that money in early-stage markets. These findings are consistent with the fact that private business wealth is highly concentrated at the top of the income distribution and the return premium over public markets we document in Section 3.2.

Given that income shares are slow-moving variables, the differences between the baseline top 0.5% pre- and post-tax income shares and the counterfactual shares are not very large in absolute terms—less than 1 percentage point difference in 2019—. However, the effects are much larger when quantifying the contribution of this return channel for the overall growth of the taxable income share over the 2010-2019 period. In particular, private capital gains from early-stage investing account for 9% and 20% of overall growth in the top 0.5% pre- and post-tax income shares over 2010-2019, respectively.[31] Moreover, had U.S. HNWIs instead invested in the NASDAQ Composite Index, the top 0.5% pre- and post-tax income shares would have grown by 8% and 19% less than they actually did, respectively. We rely on the NASDAQ as the baseline public stock market index we use, since it is the one that puts more weight on tech companies and thus it is closer to the portfolio of early-stage companies (see Appendix Figure A4).

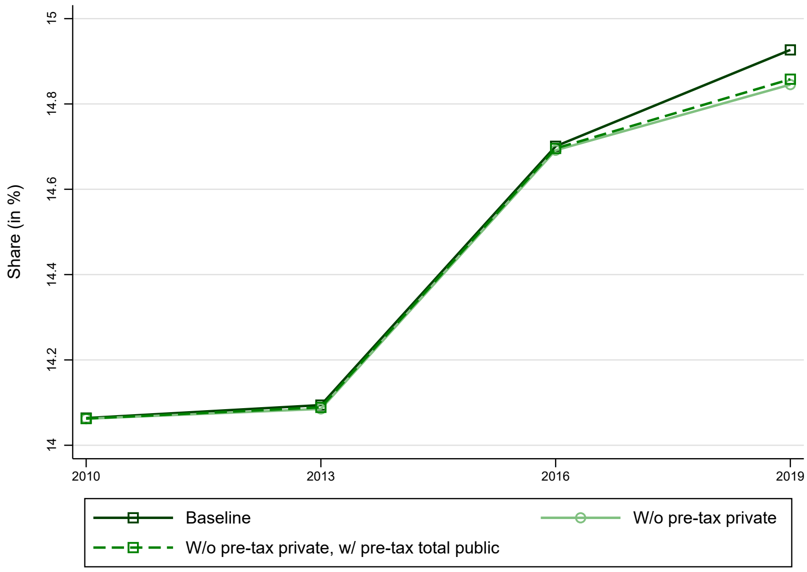
5.2 Wealth Inequality

To carry out the counterfactual simulations for wealth inequality, we estimate the baseline wealth distribution series based on the Distributional Financial Accounts (DFA) methodology developed by Batty et al. (2020), who combines the Financial Accounts (FA) with the Survey of Consumer Finances (SCF) to build U.S. wealth distribution series

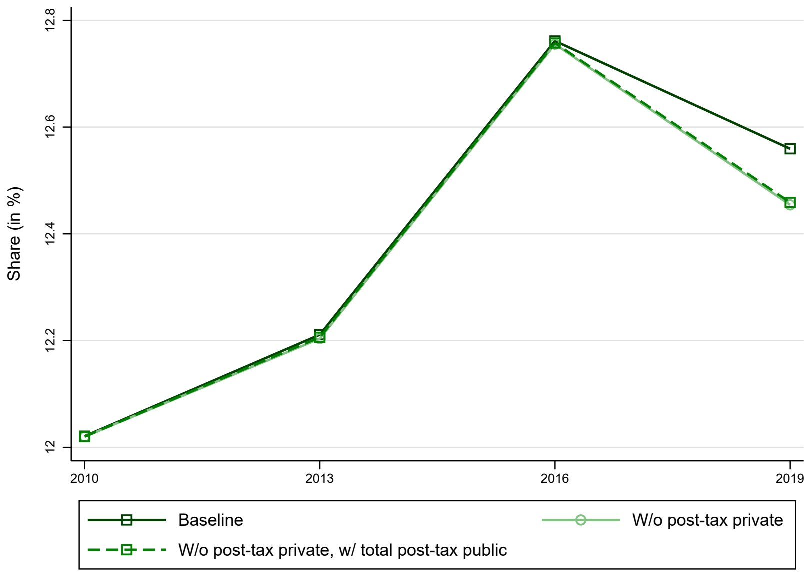
[31] We exclude from the main analysis the year 2022, as during the COVID-19 period there were fewer realizations. As a result, the differences between baseline and counterfactuals are exceptionally small. Private capital gains from early-stage investing account for only 1% and 2% of overall growth in the top 0.5% pre- and post-tax income share over 2010-2022, contrary to the 9% and 20% found over 2010-2019. Appendix Figure D10 depicts the pre- and post-tax income inequality simulations all the way up to 2022.

Figure 8: Pre- and Post-tax Income Inequality Simulations

(a) Top 0.5% Pre-Tax Income Shares



(b) Top 0.5% Post-Tax Income Shares



Source: SOI Income Stats, SCF, FA, Forbes.
Notes: This figure compares the baseline top 0.5% pre-tax and post-tax income shares to different counterfactual top 0.5% income shares over the 2010-2019 period. Panel (a) compares the baseline top 0.5% pre-tax income share to three different counterfactuals: no taxable private capital gains; no taxable capital gains but with counterfactual taxable public capital gains using NASDAQ as the public stock market index; and no taxable private capital gains but with total (both taxable and non-taxable) private capital gains. Panel (b) compares the baseline top 0.5% post-tax to three different counterfactuals: no post-tax private capital gains; and no post-tax capital gains but with counterfactual taxable public capital gains using NASDAQ as the public stock market index. The baseline income distribution series are derived by using the IRS Statistics of Income. Appendix D.2 provides more details about the methodology used to estimate the baseline income distribution series and carry out the counterfactuals.

consistent with macroeconomic aggregates. Following Batty et al. (2020), we also improve the SCF's ability to capture the top of the wealth distribution by adjusting the top of wealth distribution with the Forbes 400 rich lists.

We focus the counterfactual analyses on the top 10% wealth group and different subgroups within the top 10%, as this group accounts for 90% of the individuals we assign capital gains to. We run two main counterfactual simulations for the post-reform 2010-2022 period using the private and counterfactual public gains derived from Pitchbook and described in Appendix B.[32] In the first counterfactual, we re-estimate the wealth inequality series excluding cumulated private capital gains, that is, the wealth inequality series had HNWIs not invested in early-stage markets. In the second counterfactual, we re-estimate the wealth inequality series replacing the cumulated private capital gains by the counterfactual cumulated gains had HNIWs invested this money in the NASDAQ Composite Index.

The main challenge we face when implementing the counterfactuals is how to distribute the actual and counterfactuals accumulated gains along the wealth distribution in the SCF. For the Forbes 400 individuals, this is not an issue, as we can match the individuals on the list to their Pitchbook profiles, as explained in Appendix Section A.7. We can thus directly attribute on an individual basis to each Forbes 400 individual the accumulated capital gains they earn on these investments. For assigning the remaining Pitchbook private business capital gains, we need to rely on an imputation procedure as—with the exception of the Forbes 400 individuals—we cannot match the identities of the Pitchbook investors to the identities of the anonymous households in the SCF. For that, we rank the population of accredited investors who are full or partial owners of a C corporation or a partnership in the SCF into 100 percentiles and distribute the cumulated private capital gains proportionally to business wealth on an annual basis. We also distinguish in the imputation procedure between founding, non-founding investors, and individuals who are both founding and non-founding investors, as they may have a different weight in this market and also different investment patterns. Hence, they may obtain different returns and thus contribute differently to changing wealth inequality. This methodology ensures heterogeneity in returns coming from the direct assignment of capital gains to the Forbes 400 individuals, from the differences in returns across the private business wealth distribution, as well as from the distinction between founding and non-founding investors in the imputation procedure. Appendix D.3.2 explains in more detail the methodology used to carry out the wealth inequality simulations.

Figure 9 compares the baseline top 0.5% wealth share to the counterfactual top 0.5% wealth share under the scenario of no private capital gains and under the scenario of

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

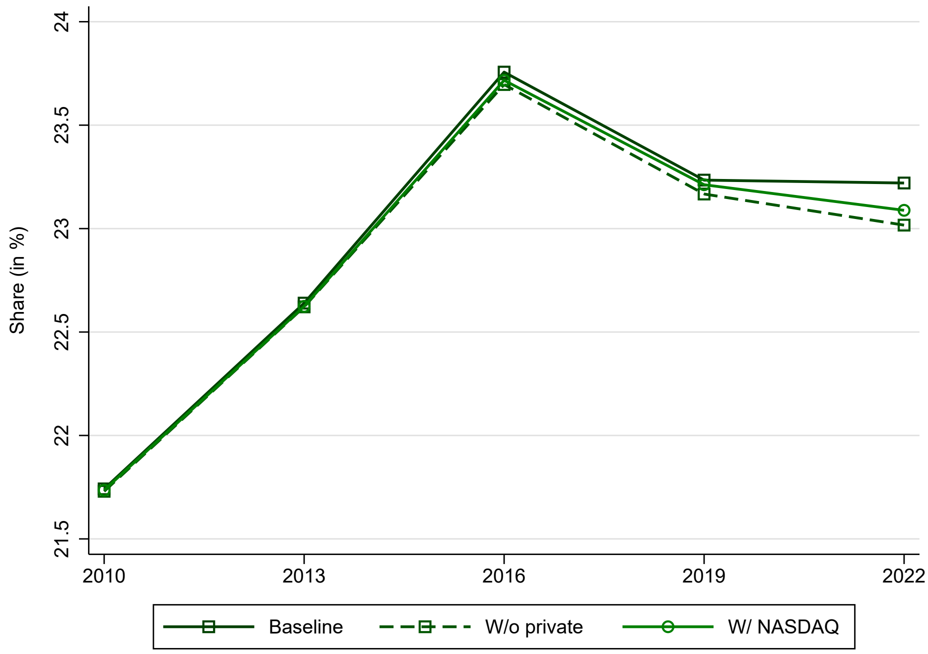
no private capital gains but with counterfactual public capital gains over the period 2010-2022. As expected, the top 0.5% wealth share is lower in the counterfactual scenario in which HNWIs would have invested the money they put in early-stage markets in to the public stock market, and even lower, if they would have just not invested that money in early-stage markets. These findings are consistent with the fact that private business wealth is highly concentrated at the top of the wealth distribution and the return premium over public markets we document in Section 3.2.

Given that wealth shares are slow-moving variables, the difference between the baseline top 0.5% wealth share and counterfactual series is not very large in absolute terms—less than 1 percentange point difference in 2022—. However, the effects are much larger when quantifying the contribution of this return channel for the overall growth of the wealth share over the 2010-2022 period. In particular, private capital gains from early-stage investing account for 14% of overall growth in the top 0.5% wealth share over 2010-2022. Moreover, had U.S. HNWIs instead invested in the NASDAQ Composite Index, the top 0.5% wealth share would have grown by 9% less than it actually did. We rely on the NASDAQ as the baseline public stock market index we use, since it is the one that puts more weight on tech companies and thus it is closer to the portfolio of early-stage companies (see Appendix Figure A4). We rerun our wealth counterfactual simulations using other public stock market and a hedge fund index as a robustness check and show our results are robust to them both in terms of direction and magnitudes (see Appendix Figure D11 and Appendix Table D2).

Finally, Figure 10 shows that the contribution of the excess return channel for the overall growth of the wealth share over the 2010-2022 period is heterogeneous across the wealth distribution. While for the top 10-5% and the top 5-1% wealth groups, private capital gains account for 2-3% of the overall growth of the their wealth share over the 2010-2022, for the top 0.1%-Forbes 400 and for the Forbes 400 they account for 7 and 15%, respectively. For the wealth groups within the top 1% and top 0.1% the counterfactuals lead to a higher growth in their wealth shares relative to the baseline shares, as these groups do lose wealth relative to the other wealth groups within the top 10% as result of the increasing participation of HNWIs in early-stage markets.[33] Appendix Table D2 also shows that the ones individuals who are both founding and non-founding investors contribute the most to the growth in top wealth shares over the 2010-2022 period.

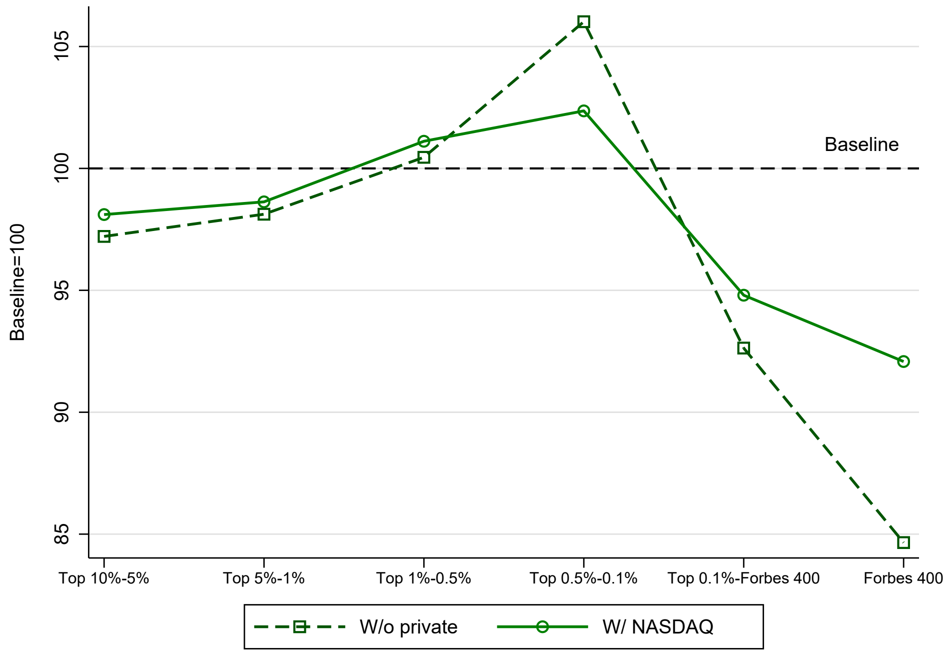
[33] Appendix Table D2 compares the different growth rates in top wealth shares over the 2010-2022 period for the different wealth groups and counterfactuals.

Figure 9: Wealth Inequality Counterfactuals (Top 0.5% wealth group)



Source: Survey of Consumer Finances, Pitchbook.
Notes: This figure compares the baseline top 0.5% wealth share to the counterfactual top 0.5% wealth share over the period 2010-2022 under two different scenarios: no private capital gains; and no private capital gains but with counterfactual public capital gains using NASDAQ as the public stock market index. The baseline wealth distribution series are derived by combining the Survey of Consumer Finances (SCF) with the Financial Accounts (FA) and the Forbes 400 rich lists following the Distributional Financial Accounts methodology developed by Batty et al. (2020). Appendix D.3 provides more details about the methodology used to estimate the baseline wealth distribution series and carry out the counterfactuals.

Figure 10: Heterogeneity of Wealth Share Growth Rates by Top Wealth Group



Source: SCF, Pitchbook, FA, Forbes.
Notes: This figure depicts how much of the 2010-2022 growth rate in the wealth of each of the wealth groups within the top 10% is accounted for by private capital gains (first counterfactual) and by counterfactual capital gains (second counterfactual) relative to the baseline actual growth rate. The baseline wealth distribution series are derived by combining the Survey of Consumer Finances (SCF) with the Financial Accounts (FA) and the Forbes 400 rich lists following the Distributional Financial Accounts methodology developed by Batty et al. (2020). Appendix D.3.2 provides more details about the methodology used to estimate the baseline wealth distribution series and carry out the counterfactuals.

5.3 Feedback Loop between Private Markets and Inequality

To what extent is the relationship between HNWIs' increasing participation in private capital markets and rising inequality self-reinforcing? We briefly consider this question in this last section, where we evaluate the effect of previous entrepreneurial success on a HNWI's later activity as an investor in other early-stage companies.

Specifically, we first identify investments by HWNIs to capitalize the companies of which they were founders themselves. We then calculate the distribution of these founders' lifetime rates of return on their capitalization investments, grouping the investments into three categories: those that yielded returns in the bottom 75% of this distribution, those ranked between the 75th and 90th percentiles, and those in the top 10%. Lastly, we estimate the effect of ranking in the top 10% of this distribution (relative to ranking either in the bottom 75% or in between the 75th and 90th percentiles) on the log amount invested by these former founders as part of their later early-stage investments, controlling for year fixed effects. We find that former founders who are in the top 10% of successful entrepreneurs invest almost 60% more as part of their later early-stage investments in comparison to those ranked in the bottom 75%. Even compared to those ranked in between the 75th and 90th percentiles, the most successful former founders invest 35% more. These results suggest the existence of a feedback loop between rising private capital market activity and rising economic inequalities.

6 Conclusion

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

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

Taken together, our paper reveals that private capital market dynamics may have non-negligible distributional implications due to the differences in portfolio composition and in returns across asset classes across the income and wealth distribution. Our analyses are based on reduced-form approaches and partial equilibrium counterfactual simulations. Further research is needed to quantify the distributional implications of changing private capital markets taking a general equilibrium approach.

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Appendix A: Private Capital Market Activity

In this appendix, we describe our procedure to clean the investment data that we obtained from Pitchbook. This data includes datasets on companies that have ever raised certain types of financing from capital markets; the deals as part of which they raised those and other types of financing; and the investors that participated in those deals, whether as direct investors or via private investment funds. In Section A.8, we report additional results related to Section 3.1 of the main text.

A.1 Versions

We merge together two versions of the data. One version (updated as of 28 April 2025) contains information 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 23 November 2023) also contains information on companies that have only ever raised debt financing, been acquired by another company, or been publicly listed. When a company appears in both versions of the data, we consider their information only from the more recent one.[34]

A.2 Companies

We observe 980,045 companies, 388,206 (39.6%) of which are headquartered in the United States. 64.1% of these U.S. companies appear in the more recent version of the data. For all but 1.2% of them, we also observe the U.S. state in which they are headquartered.

A.3 Deals

We observe at least one deal for all but 0.9% of U.S. companies. The median number of deals per company is 2, and the mean is 2.4. Of the 942,383 U.S. deals, we drop 1.8% that were cancelled or that have not yet been completed. We also drop a further 7.5% with unknown completion dates, since our analysis will depend on the timing of deals.

We distinguish between four different categories of deals. Based on the descriptions and the primary industry codes of acquired companies, we first identify real asset deals that involve other companies' acquisitions of real estate, infrastructure, or natural resource assets.

[34] Pitchbook also collects data on companies that are two years old or older but that have never raised any of these types of financing. However, these companies are missing from both of our versions of the data.

We then classify the remaining deals as either early-stage, private equity, debt, or other deals. In the early-stage category, we include capitalization, grant, crowdfunding, angel, accelerator/incubator, seed, early-stage, and late-stage investments; in the private equity category, we include buyouts, private investments in public equity, platform creations, growth/expansion investments, and general partner stakes; and, in the debt category, we include bonds, loans, mezzanine financing, and convertible debt. Of the 854,095 U.S. deals with known completion dates, we classify 40.5% as early-stage deals, 14.6% as private equity deals, 14.8% as debt deals, and 2.2% as real asset deals.

A.4 Participation in Capitalization Deals

We now identify the participants in a particular type of early-stage deals: the capitalization deals as part of which a company's founders, their family and friends, and other investors provide the company with its initial financing. Although these deals represent only 0.2% of all U.S. deals with known completion dates, they will offer us special insight into how founders' entrepreneurial successes translate into their accumulation of wealth.

From Pitchbook's synopsis of each capitalization deal, we interpret each string of consecutively capitalized words as the name of a participant. We also extract information about whether the company's founders or their family and friends participated in the deal, treating both as additional participants. The result of this extraction is a list of 2,433 participants across 1,419 U.S. capitalization deals with known completion dates.

We attempt to match each of these participants by name to one of the 408,312 investors or the 3,301,441 people for which Pitchbook maintains records, including information on the companies that they founded or on whose boards they serve. We first match 45.8% of the participants to founders, 2.8% to non-founding board members, and 3.5% to non-board members. In this way, for 411 of the deals in which founders participated, we successfully match at least one of the participants to a particular founder; for the 128 remaining such deals, we instead assume that all 347 of the founders were participants. In the end, we are left with 2,241 participants in U.S. capitalization deals with known completion dates. 66.1% of them are founders, and 22.1% are their family and friends.

A.5 Investments

We next identify the participants in other types of deals and the amounts invested by each, which Pitchbook itself records. Across U.S. deals with known completion dates, we observe 1,196,741 equity and 413,614 debt investments by individual investors.

We first combine this investment-level data on the amounts invested by individual investors with deal-level data on the total amounts raised by companies in order to impute the latter whenever they are unknown. In the deal-level data, we observe the overall amount of financing that companies raised for 55.3% of U.S. deals with known completion dates, the amount of equity financing that they raised for 27.5% of these deals, and the amount of debt financing that they raised for 14.2% of them. Using the identity that the overall amount of financing raised in a deal equals the sum of the equity and debt components, we impute the third unknown amount whenever we observe the other two.[35] If we instead observe fewer than two of these three amounts, then we also consider the investment-level data, in which we observe the amount of equity financing provided by at least one investor for 19.0% of these deals, as well as the amount of debt financing provided by at least one investor for 11.7% of them.[36] In this way, we impute the equity component for 16.0% of U.S. deals whose completion dates we observe, the debt component for 1.5% of these deals, and the overall amount of financing raised for 2.5% of them.

We then identify each investor's share of each deal. For capitalization and equity investments, we sum the observed amounts of equity financing provided by individual investors as part of a deal, subtract this sum from the total equity financing raised by the company as part of the deal, and distribute the remainder equally across all the other investors.[37] We repeat this step for debt investments, comparing the sum of the debt financing provided by individual investors as part of a deal to the total debt financing raised by the company.

Finally, we split the deals that have both an equity component and a debt component into separate deals. We also further split debt deals that have both traditional debt and private debt components into separate deals, distinguishing bonds and bank loans from all other forms of debt financing provided by non-bank lenders. After splitting the deals in these ways, we are left with 910,561 U.S. deals with known completion dates. We keep only the 553,122 private capital market deals, of which 62.6% are early-stage deals, 23.2% are private equity deals, 10.8% are private debt deals, and 3.4% are real asset deals. Together, they correspond to 1,277,796 private investments, as long as we also count as investments the 86,105 of these deals for which we observe no participants.

[35] We observe all three amounts only for 9,491 of U.S. deals with known completion dates. If we round these amounts to the nearest thousand U.S. dollars, then 95.4% of the deals satisfy the identity.

[36] Consider a debt deal for which we observe no information about the amounts of financing raised by the company in the deal-level data, but for which we observe the amount of debt financing provided by at least one investor in the investment-level data. We then impute both the overall and the debt financing raised by the company as the the sum of the debt financing provided by individual investors.

[37] In rare occasions, Pitchbook itself records the participants in capitalization deals, just like it does the participants in other types of deals. To avoid double counting, we drop 67 of the 2,241 participants in U.S. capitalization deals with know completion dates that we extracted from the synopsis of each deal.

A.6 Investors

We finally identify the investments made by high-net-worth individuals (HNWIs), who either invested in a company directly or committed capital to a private investment fund that intermediated their investment in the company. We also distinguish between founders and non-founders and between HNWIs ranked and unranked in the Forbes 400 rich lists.

We first consider intermediated investments. We observe 175,112 commitments by limited partners to private investment funds, 0.8% of which were made by HNWIs residing in the U.S. To identify each limited partner's share of each fund, we sum the observed amounts committed by individual limited partners to a fund, subtract this sum from the fund's total assets under management, and distribute the remainder equally across all the other limited partners. We then replace the 211,699 private investments in U.S. companies intermediated by funds with the corresponding 2,751,217 investments by the funds' limited partners. In the end, we are left with 3,817,314 private investments in U.S. companies, the remaining 1,066,097 of which are direct investments.

We then also identify which of these direct investments were made by U.S. HNWIs. After doing so, we find that 3.3% of all private investments in U.S. companies were by U.S. HNWIs. After further distinguishing between founders and non-founders, we find that 2.0% of these private investments by U.S. HNWIs were made by founders. Finally, we manually match the Forbes 400 rich lists with Pitchbook and find that 4.5% of HNWIs were part of the Forbes 400 rich list in the same year as they invested.

A.7 Alternative Data from Preqin

In the future, we will similarly clean the investment data that we obtained from Preqin, an alternative provider of data on investment activity in U.S. private capital markets. Comparing Preqin's data to that of Pitchbook, we will show that the latter has better data coverage than the former.

A.8 Additional Results

We now report additional results related to Section 3.1 of the main text.

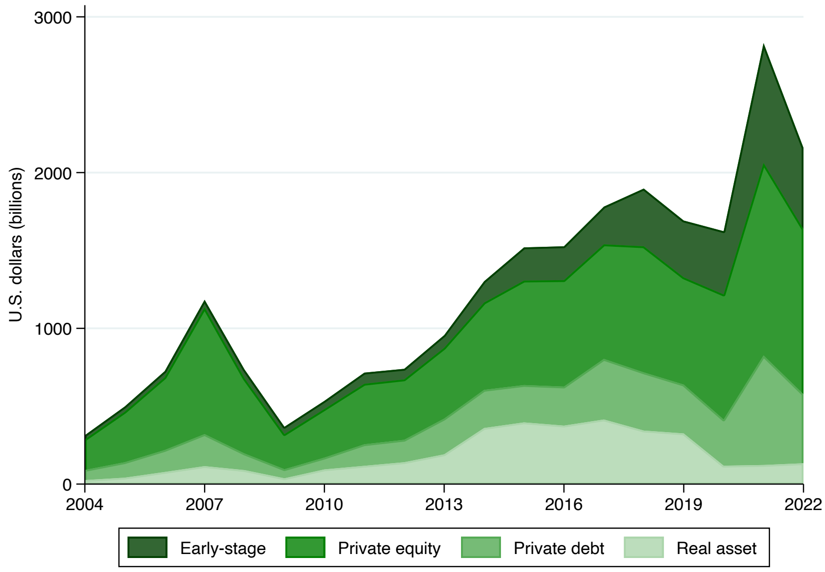
Table A1: U.S. HNWIs' Early-Stage Investments in U.S. Companies (2004-2022)

	Count		Amount invested	
	Number	Share	U.S. dollars (billions)	Share
Total	101,610	1.000	106.42	1.000
incl. individuals	76,896	0.757	67.90	0.638
incl. angel groups	13,264	0.131	11.93	0.112
incl. family offices	11,450	0.113	26.58	0.250
incl. Forbes 400	4,834	0.048	11.38	0.107
incl. non-Forbes 400	96,776	0.952	95.03	0.893
incl. founding investors	2,435	0.024	3.48	0.033
incl. non-founding investors	99,175	0.976	102.93	0.967
incl. direct	83,003	0.817	100.81	0.947
incl. intermediated	18,607	0.183	5.60	0.053
incl. information technology	51,970	0.511	47.05	0.442
incl. business-to-consumer	20,819	0.205	18.93	0.178
incl. business-to-business	12,022	0.118	11.38	0.107
incl. healthcare	11,598	0.114	19.24	0.181
incl. financial services	3,484	0.034	5.34	0.050
incl. natural resources	1,717	0.017	4.47	0.042

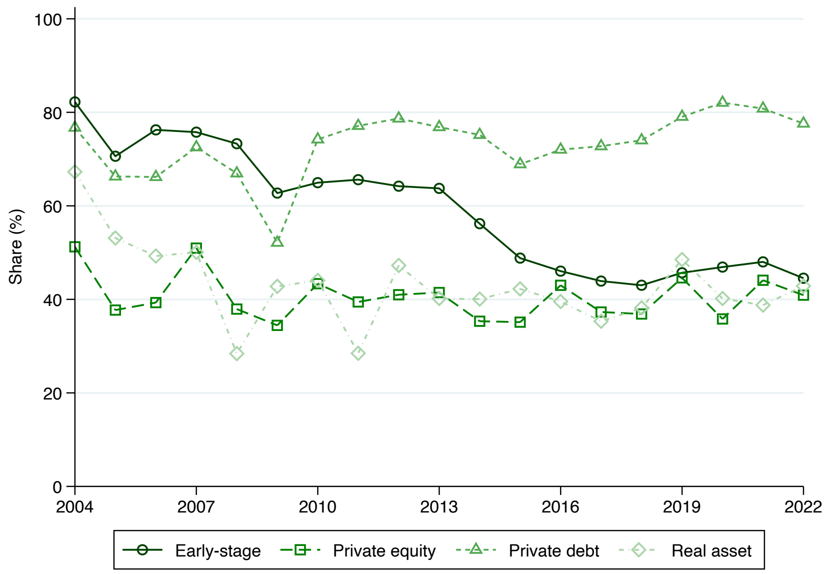
Source: Pitchbook.
Notes: This table shows summary statistics of U.S. HNWIs' early-stage investments in U.S. companies over the period 2004-2022. U.S. companies refer to companies headquartered in the U.S. U.S. high-net-worth individuals (HNWIs) refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S. Investments by Forbes 400 refer to those made by U.S. HNWIs ranked in the Forbes 400 rich list in the same year as they invested. Investments by founding investors refer to those made by the founders of the companies raising financing, while those from non-founding investors are made by other investors. Intermediated investments refer to those made via private investment funds. Natural resources refer to both the energy sector and the materials and resources sector. The amounts in U.S. dollars are expressed in nominal terms.

Figure A1: Private Investments Globally

(a) Investments in All Companies

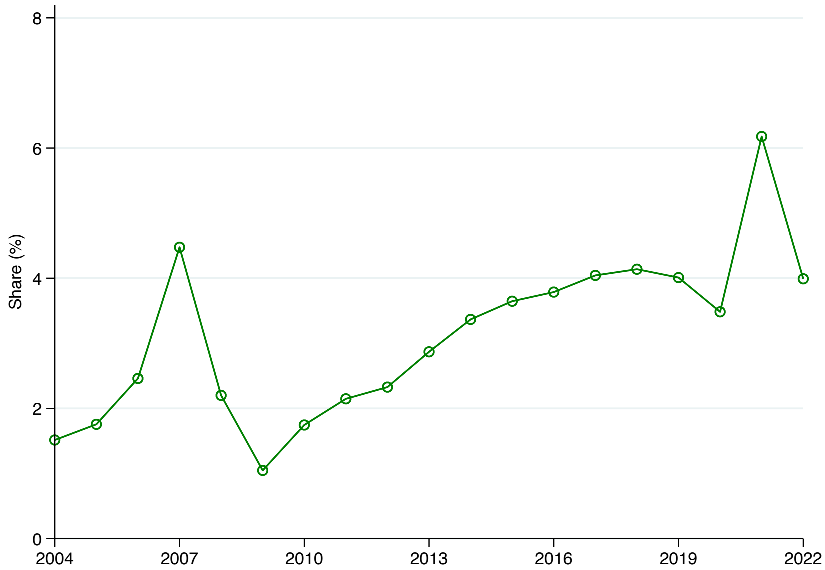


(b) U.S. Companies' Shares of Investments



Source: Pitchbook.
Notes: This figure depicts the evolution of private investments globally (panel (a)), as well as the share of investments in U.S. companies out of total global investments (panel (b)). Private investments refer to investments by individual investors across four different private asset categories: early-stage, private equity, private debt, and real asset deals. U.S. companies refer to companies headquartered in the U.S. The values in panel (a) are expressed in nominal terms.

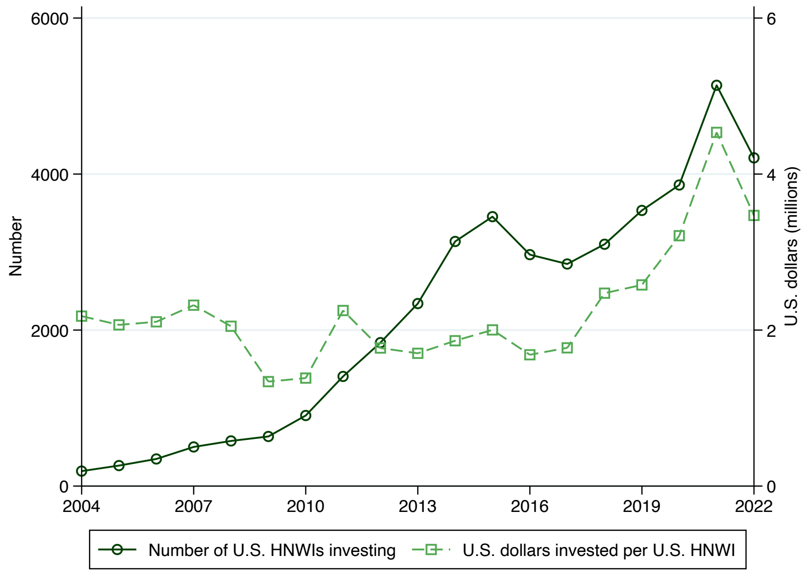
Figure A2: Private Investments in U.S. Companies as a Share of U.S. GDP



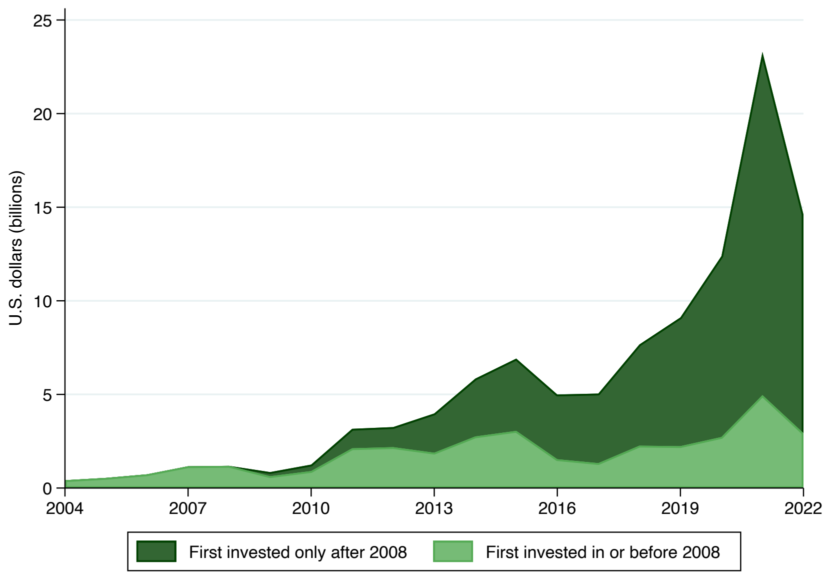
Source: Pitchbook, U.S. Bureau of Economic Analysis.
Notes: This figure depicts the evolution of private investments in U.S. companies as a share of total U.S. GDP. Private investments refer to investments by individual investors in early-stage, private equity, private debt, and real asset deals. U.S. companies refer to companies headquartered in the U.S.

Figure A3: U.S. HNWIs' Participation in U.S. Early-Stage Markets

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

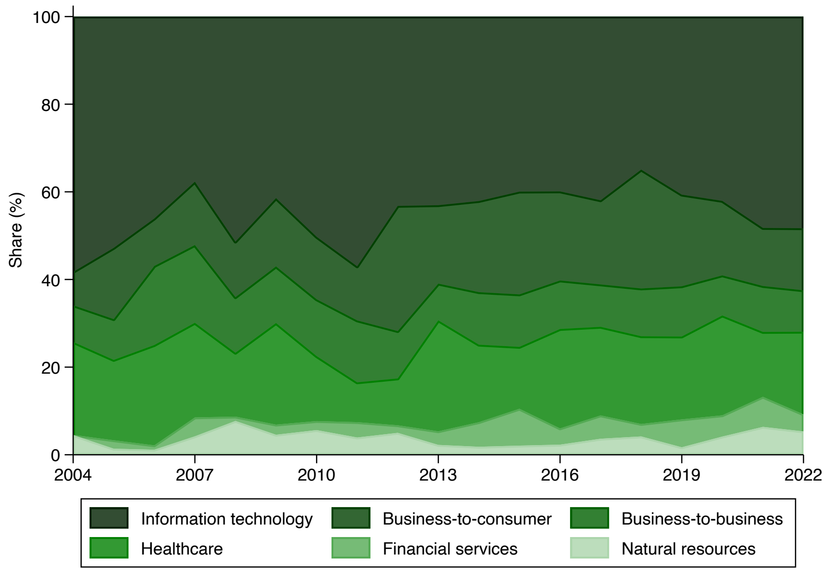


(b) U.S. HNWIs Early-Stage Investments in U.S. Companies



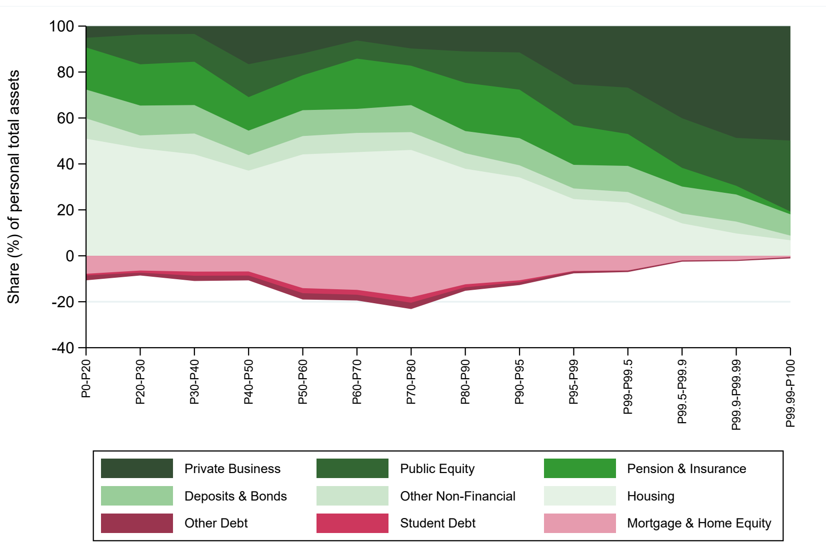
Source: Pitchbook, Forbes.
Notes: This figure depicts U.S. HNWIs participation in U.S. early-stage markets over the period 2004-2022. Panel (a) shows the number of unique U.S. HNWIs investing in early-stage U.S. companies, as well as the annual U.S. dollars invested per U.S. HNWI. Panel (b) distinguishes early-stage investments in U.S. companies of HNWIs who made their first early-stage investment prior to 2008 versus those made by HNWIs who made their first investment only after 2008. U.S. companies refer to companies headquartered in the U.S. U.S. high-net-worth individuals (HNWIs) refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S. The amounts plotted on the right axis in panel (a) and those in panel (b) are expressed in nominal terms.

Figure A4: Sector Composition of U.S. HNWIs' Early-Stage Investments in U.S. Companies



Source: Pitchbook.
Notes: This figure depicts the sectoral composition of U.S. HNWIs' early-stage investments in U.S. companies. U.S. companies refer to companies headquartered in the U.S. U.S. high-net-worth individuals (HNWIs) refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S. Natural resources refer to both the energy sector and the materials and resources sector.

Figure A5: U.S. Asset Composition by Income Level in 2022



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

Appendix B: Return Methodology

In this appendix, we describe our data-cleaning procedure for Pitchbook's valuation data and our return methodology. Section B.1 first describes how we clean the valuation data, impute valuations when they are missing, and construct alternative samples of valuations to evaluate the robustness of our returns analyses. Section B.2 then describes how we calculate HNWIs' returns on their early-stage investments based on these valuations. Finally, in Section B.3, we report additional results related to Section 3.2 of the main text.

B.1 Valuations

B.1.1 Observed Valuations

From Pitchbook's data on deals, we obtain the valuation history of each company. When a company raises financing as part of a deal, its investors and management agree on its new valuation. By comparing the company's new pre-money valuation (before accounting for the equity financing that it raised as part of the new deal) to its previous post-money valuation (after accounting for the equity financing that it raised as part of its previous deal), we calculate the annual returns that its existing investors earned on their investments (see Section B.2) after accounting for the dilution of their shares between deals.

Since our analysis of returns starts from 2004, we consider the 790,207 U.S. deals that were completed after 2003. Of these, we have complete information about the valuation of the company raising financing—that is, about both its pre- and post-money valuations as part of a deal, as well as the size of the equity component of that deal (see Appendix Section A.5)—in only 12.3% of cases.

96.4% of the above deals with complete valuation information satisfy (to the nearest thousands U.S. dollars) the identity that the equity component of a deal equals the difference between the company's post- and pre-money valuations. However, for 9.0% of these deals, Pitchbook only estimates the valuation rather than observing it. When constructing each company's valuation history, we consider only the 85,416 deals whose valuation information is complete, satisfies this identity, and is not estimated by Pitchbook.[38]

[38] 85,416 (observed valuations) = 790,207 (deals completed after 2003) × 12.3% (share with complete valuation information, i.e., subtracting 692,883 deals) × 96.4% (share for which equity component of the deal equals the difference between company's post- and pre-money valuation, i.e., subtracting 3,502 deals) × 91.0% (share with observed rather than estimated valuation information, i.e., subtracting 8,406 deals).

B.1.2 Missing Valuations

Next, we consider multiple different methods to impute missing valuations. We construct four alternative samples of valuations, each of which is based on a different method to impute missing valuations:

- Baseline: observed and imputed valuations, observed bankruptcies (85,416 + 5,312,317 + 39,752 = 5,437,485 valuations)
- Alternative 1: observed valuations and observed bankruptcies (85,416 + 39,752 = 125,168 valuations)
- Alternative 2: observed valuations, observed and imputed bankruptcies (85,416 + 39,752 + 224,464 = 349,632 valuations)
- Alternative 3: observed and imputed valuations, observed and imputed bankruptcies (85,416 + 5,312,317 + 39,752 + 224,464 = 5,661,949 valuations)

Observed Bankruptcies. There are 39,752 deals which identify when a U.S. company went bankrupt or out of business permanently. For these deals, we infer that the company's valuation falls sharply, even if we do not observe its exact valuation at liquidation. As the most conservative assumption, we assume that the company's valuation permanently falls by -100% in such cases. We do not apply this imputation when a company went bankrupt temporarily before raising financing at a later stage and being attributed a positive valuation as part of such a new deal. This happens in only 2.8% of the cases.

Imputed Bankruptcies. In case Pitchbook's records of bankruptcies are incomplete, we consider an additional conservative assumption: we assume that, if a company's last raised financing in or before 2021, then it went bankrupt four years after its last financing date. For example, we assume that a company that last raised financing in 2013 went bankrupt in 2017, that one that last raised financing in 2021 went bankrupt in 2025, and that one that last raised financing in 2022 did not go bankrupt.

Imputed Valuations. In order to calculate the returns yielded by investments in a company, we need to observe the company's valuation on at least two different dates. Given the large number of missing valuations in Pitchbook, we therefore use a sophisticated imputation methodology to estimate the valuation of each company i in every year y since its founding and until its eventual bankruptcy, if it ever occurs.[39] Using the valuations of each company whose valuation we observe on at least two different dates during or after

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

2004, we estimate a regression of its log post-money valuation on date t on company fixed effects and triple-interacted sector $s \times$ stage $a \times$ year y fixed effects:

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

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

After estimating Equation (B1), we predict each company's log post-money valuation in every year from its year of founding to its year of bankruptcy or going out of business as $\widehat{\beta}_{s(i),a(i,t),y(t)}$. To each predicted value, we then add the most recent difference between the observed value of the company's log post-money valuation and the corresponding prediction. Next, we exponentiate this adjusted prediction to impute the company's post-money valuation as part of each deal and in each year in which it is missing. To impute the pre-money valuation, we subtract the amount of equity financing the company raised during that year from the imputed post-money valuation. This methodology allows us to estimate returns for 301,377 additional companies, compared to the 51,743 that we can estimate returns for based on only observed valuations and observed bankruptcies.

B.2 Return Calculation

We calculate returns at the company level, before then calculating them at the investment level. In the investment-level analysis, investments by two different investors in the same deal (see Section will yield different rates of return over time only if the investors exit their investments at different dates.

B.2.1 Calculation of Company-Level Returns

We first calculate the number of days D_{i,t_1,t_2} between each consecutive pair of deals on dates t_1 and t_2 (where $t_1 < t_2$) for each company i. Let R_{i,t_1,t_2} be the percent change between its new pre-money valuation on date t_2 and its previous post-money valuation on date t_1 (see Korteweg and Sorensen, 2010):

$$R_{i,t_1,t_2} = \frac{\text{PreValuation}_{i,t_2} - \text{PostValuation}_{i,t_1}}{\text{PostValuation}_{i,t_1}} \tag{B2}$$

Assuming the company's valuation compounds at a constant daily rate between the dates we observe the valuations, we convert this percent change R_{i,t_1,t_2} into a daily compounded rate $r_{i,t}$ for company i on date t:

$$\begin{aligned}
(1+r_{i,t})^{D_{i,t_1,t_2}} &= 1+R_{i,t_1,t_2} \\
\implies \ln(1+r_{i,t}) &= \frac{\ln(1+R_{i,t_1,t_2})}{D_{i,t_1,t_2}} \\
\implies r_{i,t} &= \exp\left(\frac{\ln(1+R_{i,t_1,t_2})}{D_{i,t_1,t_2}}\right) - 1
\end{aligned} \tag{B3}$$

We use $r_{i,t}$ to construct the history of the daily rate of return on each company i from 2004 to 2024, applying a rate of zero to all dates preceding the completion date of the company's first deal and following the completion date of its last deal.

B.2.2 Calculation of Investment-Level Returns

Given $r_{i,t}$ as defined in (B.2.1), $EntryDate_j$ as the date on which investment j in company i entered, $ExitDate_j$ as the date on which investment j in company i exited, we calculate the rate of return investment j yields during a calendar year y as:

$$1+r^{ann}_{j,y} = \prod_{t=1/1/y}^{31/12/y} 1\{EntryDate_j \leq t < ExitDate_j\} \times (1+r_{i(j),t})$$

$$\implies r^{ann}_{j,y} = \prod_{t=1/1/y}^{31/12/y} 1\{EntryDate_j \leq t < ExitDate_j\} \times (1+r_{i(j),t}) - 1 \tag{B4}$$

If $EntryDate_j$ or $ExitDate_j$ were during year y, then the annual rate of return $r^{ann}_{j,y}$ in (B.2.2) would not be an annualized rate of return. If we define $D_{j,y} = \sum_{t=1/1/y}^{31/12/y} 1\{EntryDate_j \leq t < ExitDate_j\}$ as the number of days in year y during which investment j was held, and $D^{max}_y \in \{365, 366\}$ as the total number of days in the calendar year, then we can also calculate the annualized rate of return investment j yields in year y:

$$(1+r_{j,y})^{D_{j,y}} = 1+r^{ann}_{j,y} \tag{B5}$$

$$\implies r_{j,y} = \exp\left(\frac{\ln(1+r^{ann}_{j,y})}{D_{j,y}}\right) - 1 \tag{B6}$$

Using (B5) with D_y^{Max} is the annualized rate of return,

$$\implies (1 + r_{j,y})^{D_y^{max}} = 1 + r_{j,y}^{annualized} \tag{B7}$$

Substituting (B6) in (B7), we get,

$$\implies r_{j,y}^{annualized} = \left[\exp\left(\frac{\ln(1 + r_{j,y}^{ann})}{D_{j,y}}\right)\right]^{D_y^{max}} - 1 \tag{B8}$$

B.2.3 Counterfactual Returns

If we replace $r_{i(j),t}$ in (B.2.2) with the rate of return $r_{k,t}$ yielded by an index k on the same date t, then we can calculate the counterfactual investment-level return for investment j during calendar year y as:

$$r_{j,y}^{ann,k} = \prod_{t=1/1/y}^{31/12/y} 1\{EntryDate_j \leq t < ExitDate_j\} \times (1 + r_{k,t}) - 1 \tag{B9}$$

B.2.4 Aggregating Returns across Investments

Given $r_{j,y}^{ann}$ in (B.2.2), the initial amount of U.S. dollars $Value_{j,0}$ invested as part of investment j, and year y_j in which the investment was entered, then the accumulated value of j by the end of year y is:

$$Value_{j,y} = \begin{cases} Value_{j,0} & \text{if } y = y_j - 1 \\ Value_{j,0} \times \prod_{t=y_j}^{y} (1 + r_{j,y}^{ann}) & \text{if } y \geq y_j \end{cases} \tag{B10}$$

The return yielded by investment j in year y is then:

$$Return_{j,y} = Value_{j,y} - Value_{j,y-1} \tag{B11}$$

To aggregate returns across investments and thereby calculate an average rate of return that assigns an appropriate weight to each investment based on its net asset value (NAV) and number of days held, we calculate the 1-year NAV-to-NAV internal rate of return

(IRR) in year y as the solution to the following equation:

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

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

$$\implies r_y^{IRR} = (1+r_y)^{D_y^{max}} - 1 \tag{B12}$$

r_y^{IRR} in (B12) is an annualized rate of return. An alternative measure of the average annual rate of return, but one that is not annualized, is the 1-year total value to paid-in capital ratio (TVPI) minus one, which we calculate as follows:

$$r_y^{TVPI} = \frac{\sum_j Return_{j,y}}{\sum_j Value_{j,y-1}} \tag{B13}$$

Finally, we calculate the counterfactual 1-year NAV-to-NAV IRR and the counterfactual 1-year TVPI minus one by replacing $r_{j,y}^{ann}$ in (B10) with $r_{j,y}^{ann,k}$ for some index k from (B9) We then recalculate (B11)-(B13).

B.2.5 Risk-Adjusted Returns

In this section, we follow Korteweg and Sorensen (2010) to estimate risk-adjusted returns. Specifically, we estimate a CAPM-style valuation equation jointly with a dynamic selection model that accounts for concerns related to self-selection, survivorship bias, and stale prices. We compare the resulting estimates of the CAPM model's parameters to equivalent OLS and GLS estimates based on regressions that pool company-level returns over many different time horizons. Table B1 below depicts the results with 500 iterations of the Gibbs sampling procedure, with the first 200 discarded iterations discarded for burn-in.

The table shows estimates of the one-factor dynamic selection model (DSM) which accounts for selection. For DSM, we both show the full period, pre-, and post-policy period for comparison. For comparison, we also show the OLS and GLS estimates which do not account for selection. The OLS estimator regresses the log returns on the market factor, and the GLS estimator scales each observation with the inverse of the square root of the time since the last financing round.

The loading on the market factor β is 0.96 for 2004-2022, and ranges from 1.00 to 1.10 depending on the pre- or post-policy period. This is smaller than β (one-factor RMRF) of around 2.8 in Korteweg and Sorensen (2010). We find that α ranges from 3.7% to 3.57% in the pre- and post-period respectively. Compared to the GLS estimate of 5.98%, accounting for selection reduces the α. However, it is large and significant. Our DSM estimates of 3.5-3.7% are close to Korteweg and Sorensen (2010)'s own estimates of 3.2-3.3% based on a similar model estimated over the 1987-2005 period.

Table B1: Risk Adjustment of Returns following (Korteweg and Sorensen, 2010)

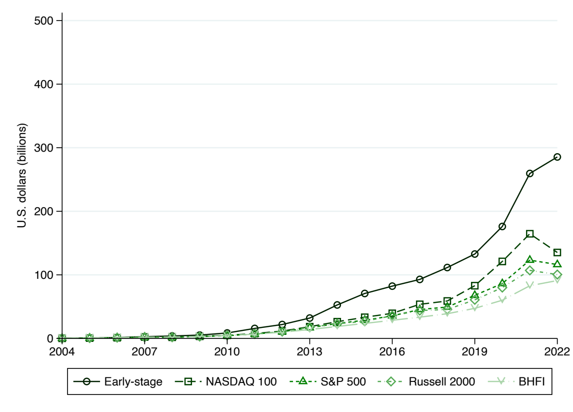
	OLS	GLS	DSM		
Valuation Equation	2004-2022	2004-2022	2004-2022	2004-2008	2009-2022
Intercept	-0.0087***	0.0037***	-0.0350***	-0.0048**	-0.0360***
	(0.0005)	(0.0007)	(0.0017)	(0.0020)	(0.0022)
RMRF	0.8312***	0.8330***	0.9640***	1.0028***	1.1040***
	(0.0485)	(0.0550)	(0.0465)	(0.0885)	(0.0850)
Sigma	1.2557	0.3355	0.3873***	0.2891***	0.3783***
			(0.0071)	(0.0050)	(0.0071)
Alpha		0.0598	0.0400***	0.0370***	0.0357***
			(0.0012)	(0.0019)	(0.0008)
Selection Equation					
Constant			-2.2286***	-2.2248***	
			(0.0089)	(0.0085)	
Return			0.3921***	0.3904***	
			(0.0056)	(0.0072)	
Time			0.0287***	0.0280***	
			(0.0009)	(0.0009)	
Time Squared			-0.0002***	-0.0002***	
			(0.000)	(0.000)	

Notes: The table generates OLS, GLS, and dynamic selection model estimates following (Korteweg and Sorensen, 2010). 2004-2022 is the full sample period of analysis in our setting. 2004-2008 is the pre-policy period, and 2009-2022 is the post-policy period. In the Valuation Equation, "Intercept" is the monthly intercept in excess of the risk-free rate, "RMRF" is the slope on the market log return in excess of the risk-free rate. "Sigma" is the estimated standard deviation of the error term. In the Selection Equation, "Return" is the log return earned since the previous financing event. "Time" is the time since this event (in years). ***, **, and * denotes whether zero is contained in the 1%, 5%, and 10% credible intervals, respectively.

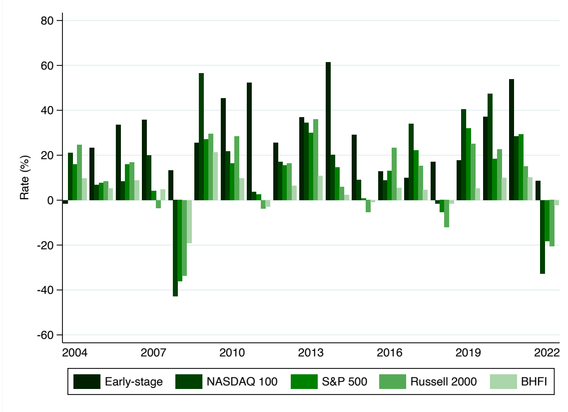
B.3 Further results

Figure B1: U.S. HNWIs' Returns on Early-Stage Investments in U.S. Companies (Alternative 1)

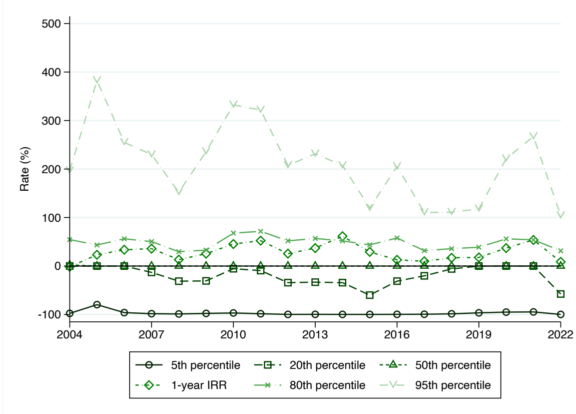
(a) Accumulated Value by End of Year



(b) 1-Year NAV-to-NAV IRR



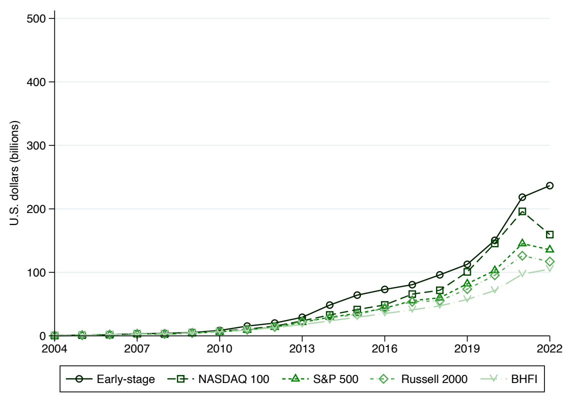
(c) Distribution of Annualized Rates of Return



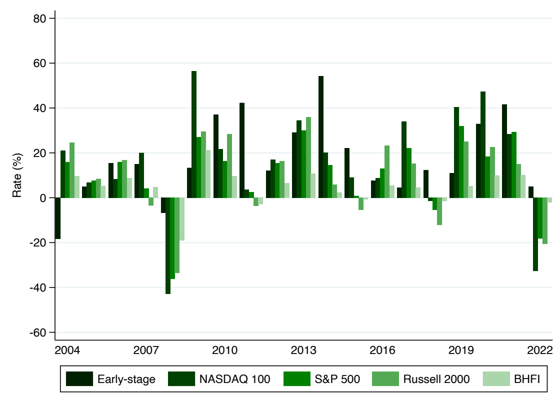
Source: Pitchbook, S&P, Bloomberg.
Notes: This figure depicts U.S. high-net-worth individuals' (HNWIs') returns on early-stage investments in U.S. companies from 2004 to 2022. Returns are based on observed valuations and observed bankruptcies (see Alternative 1 in Appendix B.2). Panel (a) compares the total accumulated value of U.S. HNWIs' early-stage investments in U.S. companies since 2004 with counterfactual scenarios where HNWIs had instead invested the same initial amounts in the NASDAQ 100, the S&P 500, the Russell 2000, or the Barclay Hedge Fund Index (BHFI). Panel (b) shows the 1-year NAV-to-NAV internal rate of return (IRR) of these early-stage and counterfactual investments, calculated following Phalippou (2024). Panel (c) shows various percentiles of the distribution of the annualized rates of return across U.S. HNWIs' early-stage investments in U.S. companies. U.S. companies refer to companies headquartered in the U.S. U.S. HNWIs refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S. BHFI refers to counterfactual returns based on the average daily rate of return across five Barclay Hedge Fund Indices: the Equity Market Neutral Index, the Distressed Securities Index, the Currency Traders Index, the Convertible Arbitrage Index, and the Equity Long Bias Index. The values in Panel (a) are expressed in nominal terms.

Figure B2: U.S. HNWIs' Returns on Early-Stage Investments in U.S. Companies (Alternative 2)

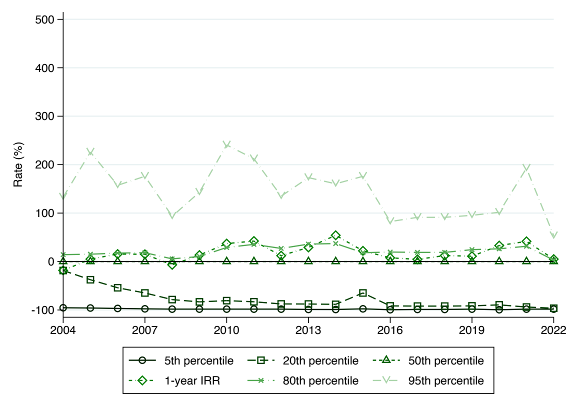
(a) Accumulated Value by End of Year



(b) 1-Year NAV-to-NAV IRR



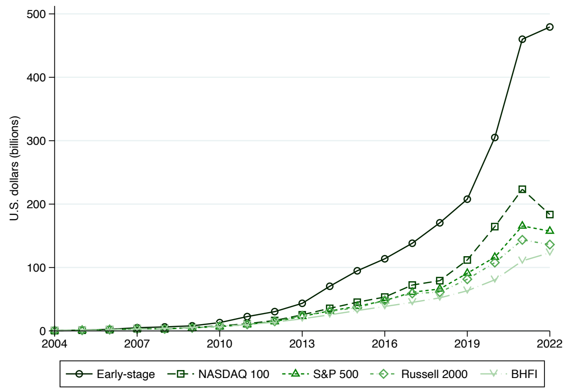
(c) Distribution of Annualized Rates of Return



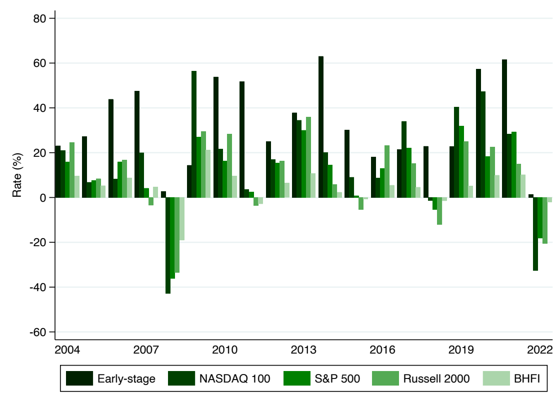
Source: Pitchbook, S&P, Bloomberg.
Notes: This figure depicts U.S. high-net-worth individuals' (HNWIs') returns on early-stage investments in U.S. companies from 2004 to 2022. Returns are based on observed valuations, as well as observed and imputed bankruptcies (see Alternative 2 in Appendix B.2). Panel (a) compares the total accumulated value of U.S. HNWIs' early-stage investments in U.S. companies since 2004 with counterfactual scenarios where HNWIs had instead invested the same initial amounts in the NASDAQ 100, the S&P 500, the Russell 2000, or the Barclay Hedge Fund Index (BHFI). Panel (b) shows the 1-year NAV-to-NAV internal rate of return (IRR) of these early-stage and counterfactual investments, calculated following Phalippou (2024). Panel (c) shows various percentiles of the distribution of the annualized rates of return across U.S. HNWIs' early-stage investments in U.S. companies. U.S. companies refer to companies headquartered in the U.S. U.S. HNWIs refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S. BHFI refers to counterfactual returns based on the average daily rate of return across five Barclay Hedge Fund Indices: the Equity Market Neutral Index, the Distressed Securities Index, the Currency Traders Index, the Convertible Arbitrage Index, and the Equity Long Bias Index. The values in Panel (a) are expressed in nominal terms.

Figure B3: U.S. HNWIs' Returns on Early-Stage Investments in U.S. Companies (Alternative 3)

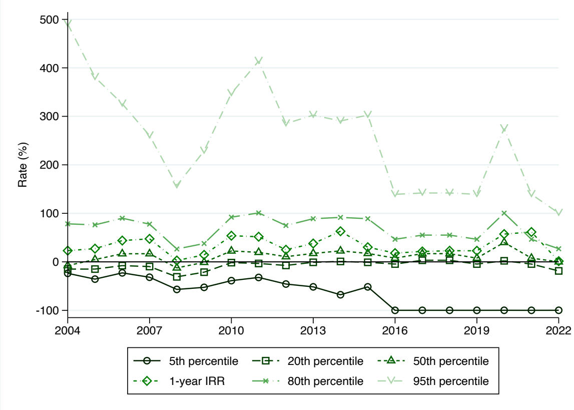
(a) Accumulated Value by End of Year



(b) 1-Year NAV-to-NAV IRR

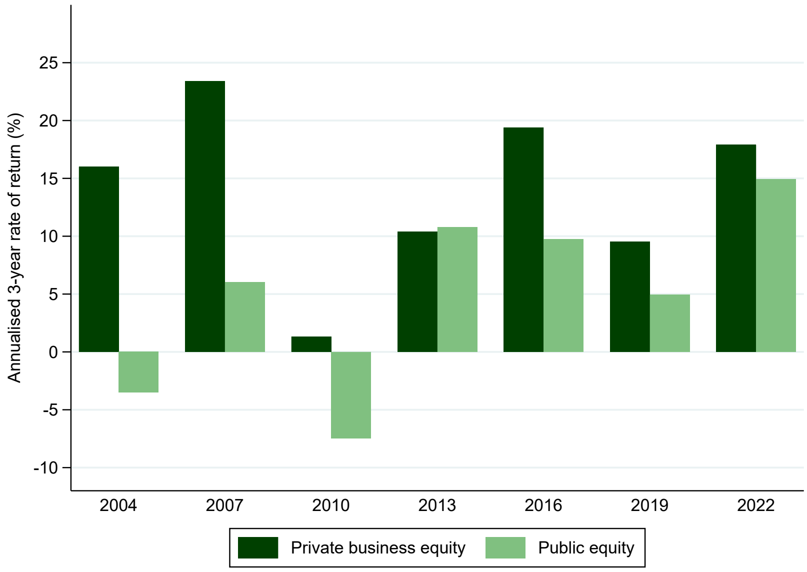


(c) Distribution of Annualized Rates of Return



Source: Pitchbook, S&P, Bloomberg.
Notes: This figure depicts U.S. high-net-worth individuals' (HNWIs') returns on early-stage investments in U.S. companies from 2004 to 2022. Returns are based on observed and imputed valuations, as well as observed and imputed bankruptcies (see Alternative 3 in Appendix B.2). Panel (a) compares the total accumulated value of U.S. HNWIs' early-stage investments in U.S. companies since 2004 with counterfactual scenarios where HNWIs had instead invested the same initial amounts in the NASDAQ 100, the S&P 500, the Russell 2000, or the Barclay Hedge Fund Index (BHFI). Panel (b) shows the 1-year NAV-to-NAV internal rate of return (IRR) of these early-stage and counterfactual investments, calculated following Phalippou (2024). Panel (c) shows various percentiles of the distribution of the annualized rates of return across U.S. HNWIs' early-stage investments in U.S. companies. U.S. companies refer to companies headquartered in the U.S. U.S. HNWIs refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S. BHFI refers to counterfactual returns based on the average daily rate of return across five Barclay Hedge Fund Indices: the Equity Market Neutral Index, the Distressed Securities Index, the Currency Traders Index, the Convertible Arbitrage Index, and the Equity Long Bias Index. The values in Panel (a) are expressed in nominal terms.

Figure B4: Annualized 3-year Rate of Return on All Private Business Equity and All Public Equity



Source: SCF.
Notes: This figure shows the annualized 3-year rate of return on all private business equity and all public equity in the U.S. between 2004 and 2022, following the exact same methodology as in Moskowitz and Vissing-Jørgensen (2002) and Kartashova (2014) using the SCF.

Appendix C: The Qualified Small Business Stock Capital Gains Tax Exclusion

In this appendix, we first describe our procedure to identify eligible issuers of qualified small business stock (QSBS) based on the data on companies and deals that we obtained from Pitchbook. Using this data, we can identify when each company is active and private, whether it is setup as a C corporation, whether it is active primarily in a qualified trade or business, and when it first exceeded $50 million in gross assets. We also present further results regarding the QSBS institutional setting and the analyses we carry out based on it.

C.1 Qualified Small Business Stock Eligible Companies

C.1.1 Active Private Companies

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

For 84.5% of the 388,206 U.S. companies that we observe, we observe the year in which they were founded. For an additional 14.6% of these companies, we impute their missing year founded as the year of their first deal with a known completion date.

We also identify the first date of bankruptcy for 10.8% of U.S. companies, the first date of being publicly listed for 4.2% of them, and the first date of being acquired for 50.4%.

C.1.2 C Corporations

To now identify C corporations, we parse each company's legal name. We observe this for 78.7% of U.S. companies, using their trade name whenever their legal name is missing.

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

We therefore classify only the remaining corporations ("Corp" or "Inc") as C corporations. The reason why we can classify all of them as C corporations is that companies seeking financing in private capital markets are unlikely to be taxed as S corporations, since these can have at most 100 shareholders (Polsky and Yale, 2023). By our classification, 49.6% of the U.S. companies that we observe are C corporations.

C.1.3 Qualified Trades and Businesses

To next identify U.S. companies that are active primarily in a qualified trade or business, we consider each company's primary industry code.[40] This is missing for less than 0.1% of U.S. companies; in this rare case, we assume that a company missing its primary industry code is active primarily in a disqualified trade or business.

We classify a company as active primarily in a disqualified trade or business if its primary industry code is related to either healthcare services, legal services, engineering services, accounting services, consulting services, financial services, performing arts, athletics, hospitality, agriculture, or natural resources. By this classification, 68.8% of the U.S. companies in the data are active primarily in qualified trades or businesses.

C.1.4 Gross Assets Exceeding $50 Million

To finally calculate a proxy for each company's gross assets, we calculate the total financing raised by the company up to the completion date of each of its deal. For the purposes of this calculation, we consider only deals as part of which we would expect the financing raised by the company to have increased the amount of gross assets on its balance sheet. For example, this excludes buyouts and debt refinancing deals.

We can calculate the total financing raised as of the end of 2008 for 28,605 U.S. companies, 80.3% of which had not raised financing exceeding $50 million. Similarly, of the 160,469 U.S. companies which we at least once observe raising financing that we would expect to have increased their gross assets, 86.2% never raised more than $50 million.

To validate this proxy, we consider data on financial statements available for 21.6% of U.S. companies. For 3,450 of these companies, in addition to their total financing raised as of the end of 2008, we can also calculate their true gross assets as of the end of 2008 as the sum of their cash and cash equivalents and their net property, plants, and equipment.[41]

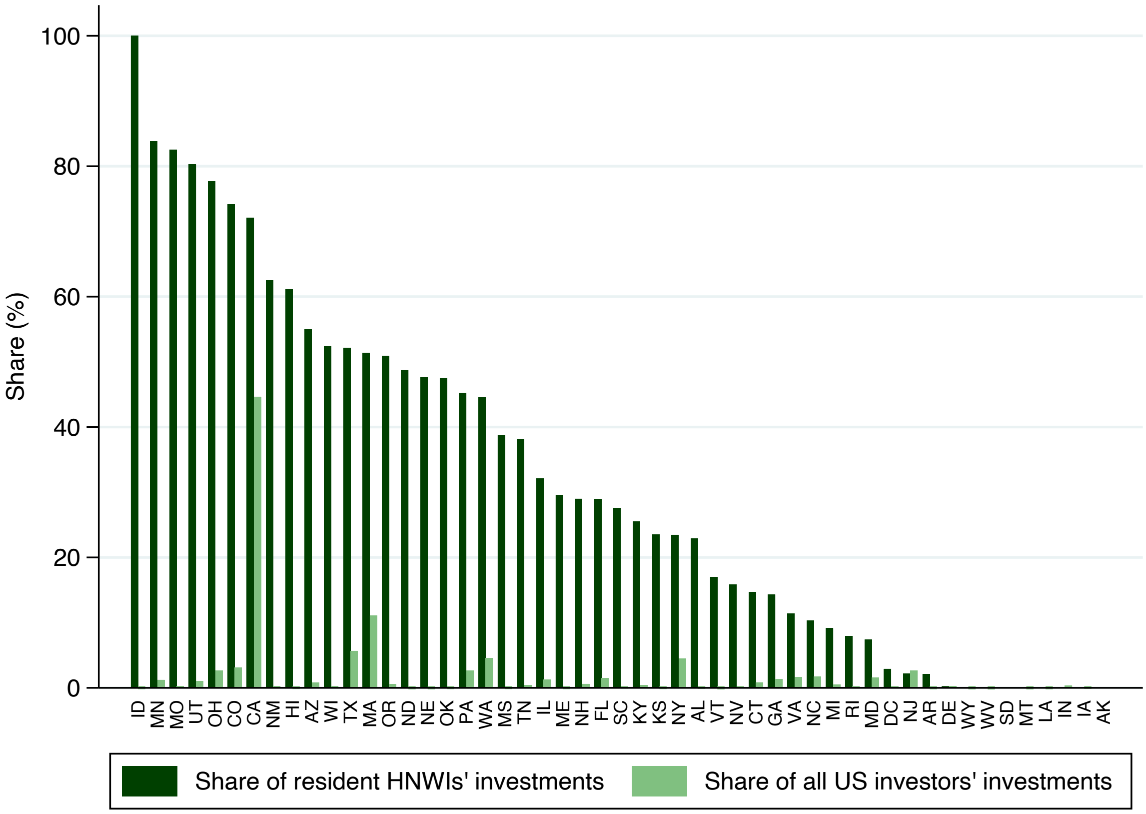
[40] Section 1202 of the Internal Revenue Code requires that at least 80% of the company's assets be used in the active conduct of one or more qualified trades or businesses (Polsky and Yale, 2023). However, since we cannot observe how much of its assets a company actually uses in each trade or business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

[41] 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

The proxy and the true measure lie on the same side of the $50 million threshold for 66.2% of these companies. Furthermore, the accuracy of this proxy improves to 79.9% when considering only the 329 of these companies that were founded after 2000.

C.2 Further results

Figure C1: In-State Bias of Early-Stage Investments
by Resident HNWIs: 2004-2008

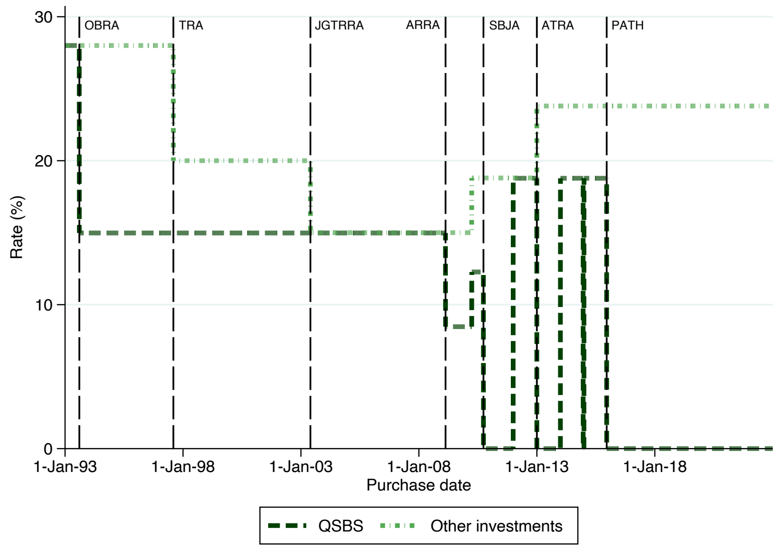


Source: Pitchbook.
Notes: The plot compares the share of investments by each state's resident HNWIs invested in companies headquartered within that state to the share of investments by all U.S. investors invested in companies headquartered within that same state.

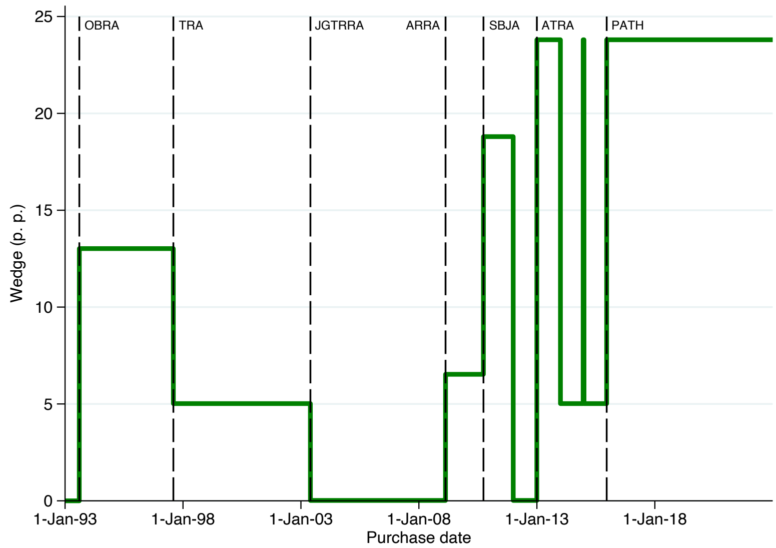
property" (Polsky and Yale, 2023). Net property, plant, and equipment proxies for their sum.

Figure C2: History of the Federal Tax Exemption on QSBS Capital Gains

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

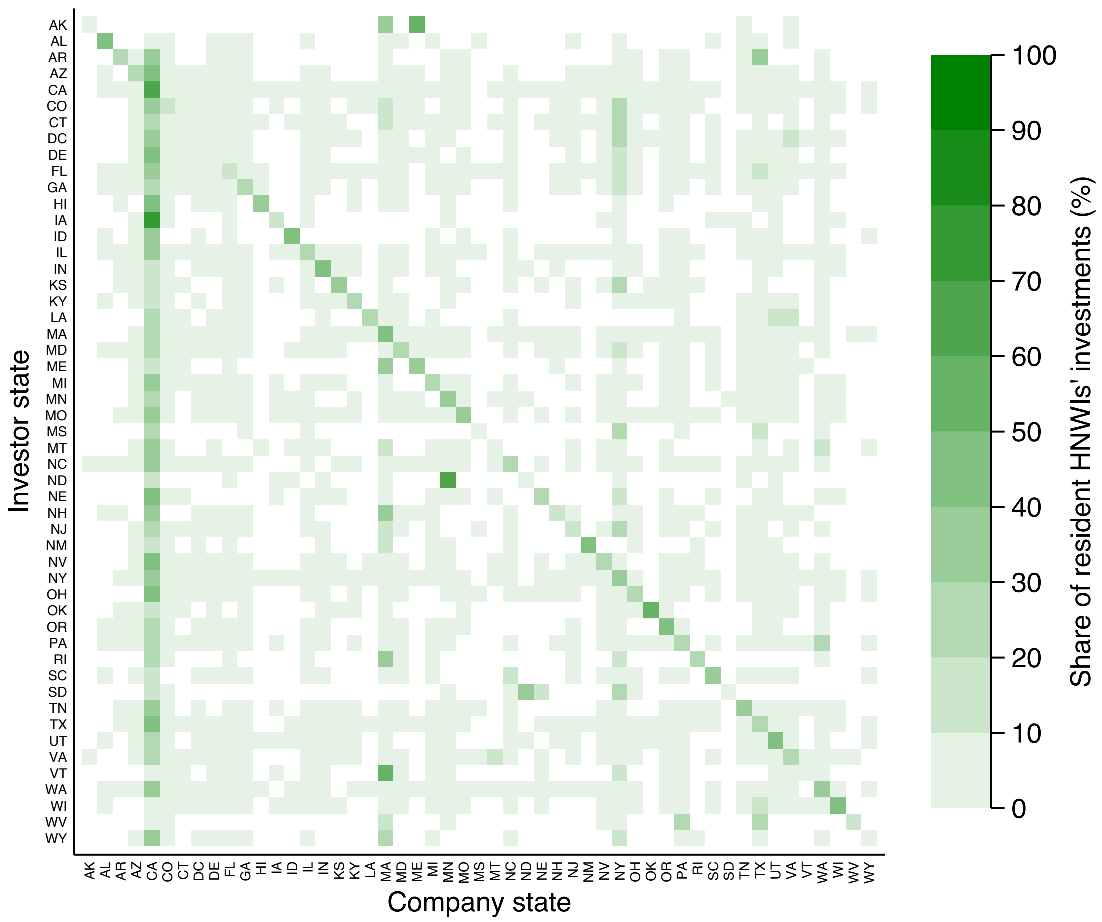


(b) Expected Tax Wedge on QSBS



Source: Polsky and Yale (2023).
Notes: This figure compares the historical evolution of the federal tax exemption on QSBS capital gains since its introduction in 1993, with that of the long-term capital gains tax rate on other alternative investments. Panel (a) depicts the evolution of the two expected rates—QSBS and other investments—separately, while panel (b) plots the difference between the two lines in panel (a), that is, the expected tax wedge. Note that the rates shown are expected and not actual rates, as those are the rates that individuals are expected to be subject to as of the purchase date, but the rates may change (and indeed, they changed) ex-post. The dashed vertical black bars indicate the different Acts that changed QSBS legislation, in particular, the Omnibus Budget Reconciliation Act (OBRA), the Taxpayer Relief Act (TRA), the Jobs and Growth Tax Relief Reconciliation Act (JBGTRRA), the American Recovery and Reinvestment Act (ARRA), the Small Business Jobs Act (SBJA), the American Tax Payer Relief Act (ATRA), and the Protecting Americans from Tax Hikes Act (PATH).

Figure C3: Distribution of Early-Stage Investments by Resident HNWIS across States: 2004-2022

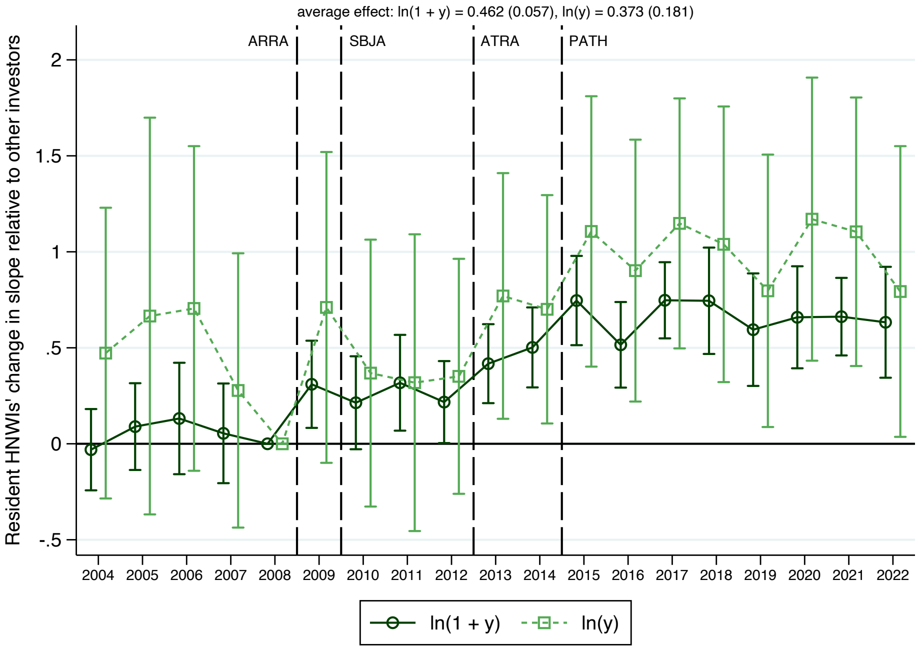


Source: Pitchbook.

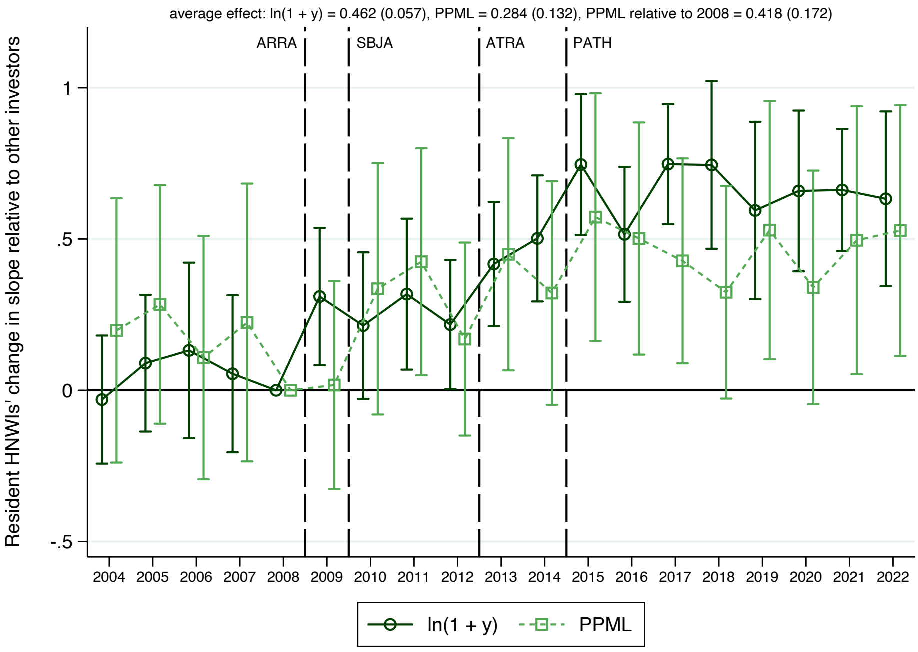
Notes: The plot reports the share of investments by each (investor) state's resident HNWIs invested in companies headquartered each (company) state.

Figure C4: Robustness of Estimates of Equation (2) to Alternative Estimators

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



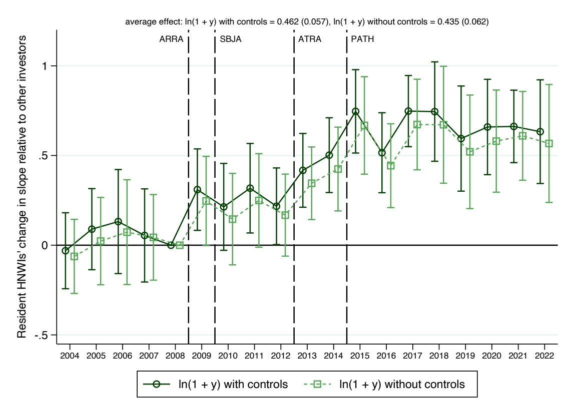
(b) $\ln(1 + Y_{i,s,t})$ vs. PPML



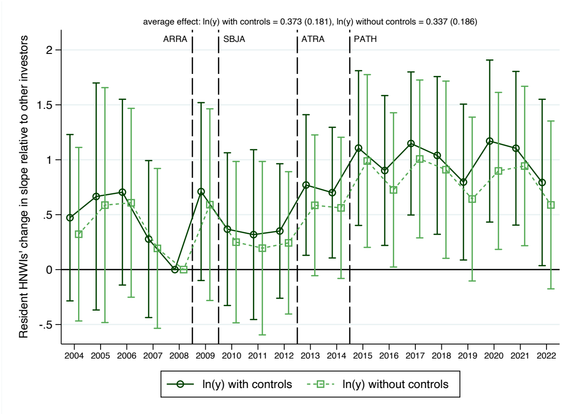
Source: Pitchbook, GEOWEALTH-US.
Notes: The average effects reported are based on a modified version of Equation (2) where β_t is replaced with $\beta_{t:t>2008}$ (or with both $\beta_{t:t<2008}$ and $\beta_{t:t>2008}$). The regression with $\ln(1 + Y_{i,s,t})$ as the outcome is based on 3,876 state-year observations. In Panel A, the regression with $\ln(Y_{i,s,t})$ as the outcome is based on 3,296 observations. In Panel B, the regression is estimated using a Poisson pseudo-maximum likelihood (PPML) estimator with $Y_{i,s,t}$ as the outcome and is based on 3,812 observations.

Figure C5: Robustness of Estimates of Equation (2) to Inclusion of Controls

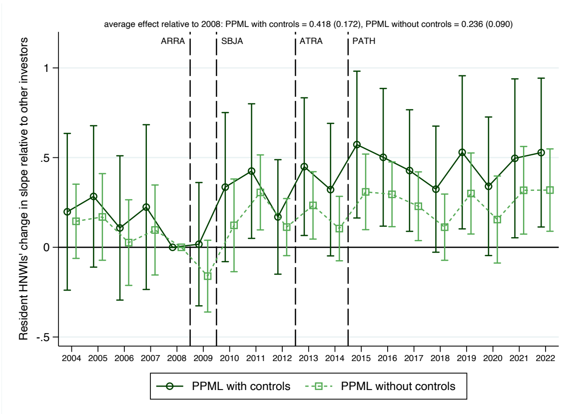
(a) $\ln(1 + Y_{i,s,t})$



(b) $\ln Y_{i,s,t}$

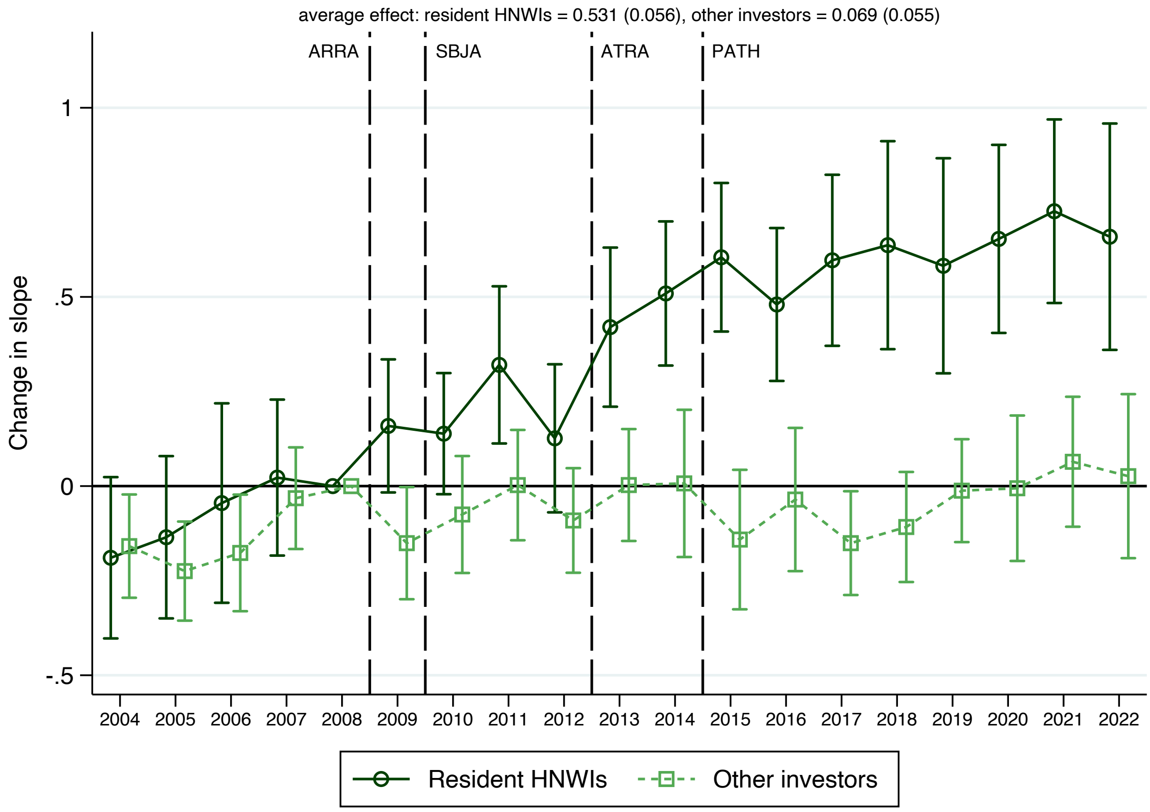


(c) PPML



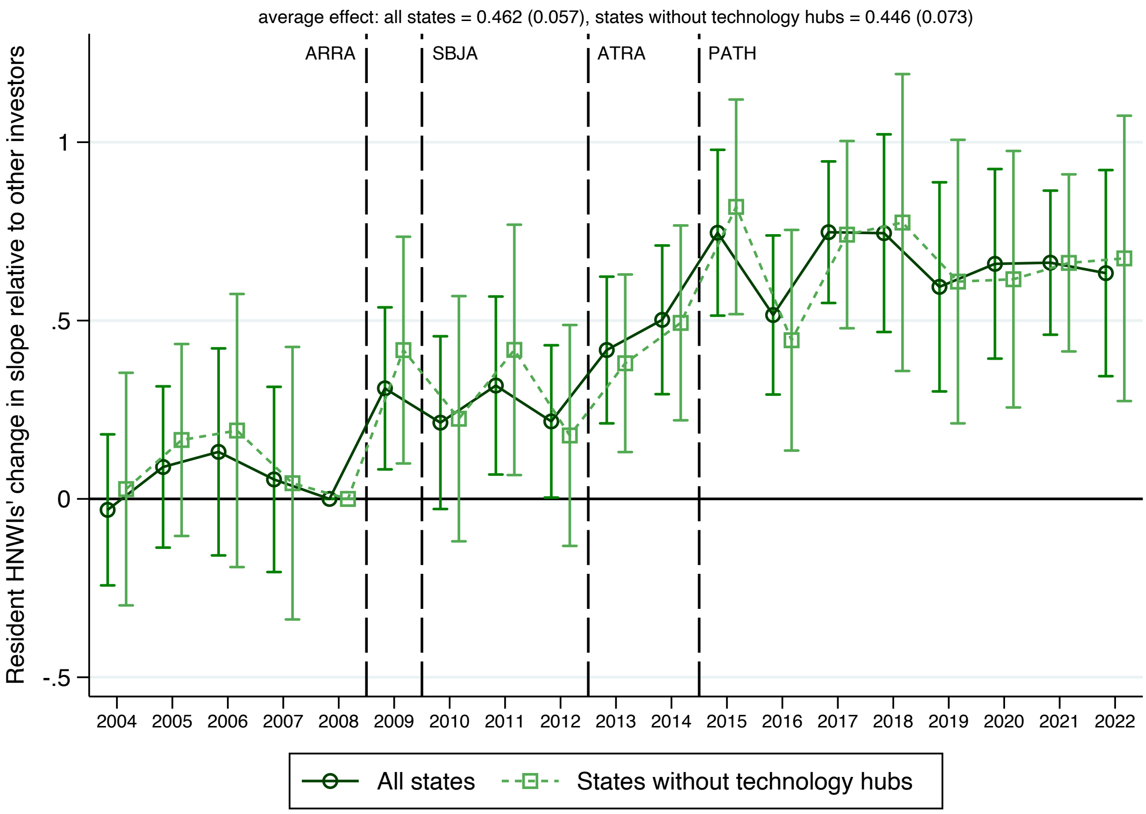
Source: Pitchbook, GEOWEALTH-US.
Notes: The average effects reported are based on a modified version of Equation (2) where β_t is replaced with $\beta_{t:t>2008}$ (or with both $\beta_{t:t<2008}$ and $\beta_{t:t>2008}$). In Panel A, the regression with $\ln(1 + Y_{i,s,t})$ as the outcome is based on 3,876 observations. In Panel B, the regression with $\ln(Y_{i,s,t})$ as the outcome is based on 3,296 observations. In Panel C, the regression is estimated using a Poisson pseudo-maximum likelihood (PPML) estimator with $Y_{i,s,t}$ as the outcome and is based on 3,812 observations. The controls include only the local long-term capital gains tax wedge on QSBS investments for individuals residing in state s.

Figure C6: Difference-in-Difference Estimates Underlying Estimates of Equation (2)



Source: Pitchbook, GEOWEALTH-US.
Notes: The difference-in-difference estimates are based on a modified version of Equation (2) where $\mathbb{1}_{i=\text{resident HNWIs}}$ and $\delta_{s,t}$ are dropped. The average effects reported are then based on a further modification that replaces β_t with $\beta_{t:t>2008}$.

Figure C7: Robustness of Estimates of Equation (2) to Subsamples of States



Source: Pitchbook, GEOWEALTH-US.
Notes: The regression is based on 3,192 state-year observations. The average effects reported are based on a modified version of Equation (1) where β_t is replaced with $\beta_{t:t>2008}$. The states without technology hubs exclude only those 9 states (California, Colorado, District of Columbia, Georgia, Illinois, Massachusetts, New York, Texas, and Washington) that contain a city listed as a technology hub on the website of the U.S. technology networking company Built In: https://builtin.com/tech-hubs.

Table C1: Average Effects of the QSBS Reforms on QSBS-Eligible Companies

	Probability of Raising Financing from at Least One U.S. HNWI						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
QSBS_i	0.024	0.019	0.001	0.010	0.032	0.018	0.034
	(0.005)	(0.004)	(0.002)	(0.004)	(0.005)	(0.005)	(0.016)
Aggregated	Yes	No	Yes	Yes	Yes	Yes	Yes
Pooled	No	Yes	No	No	No	No	No
Company FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CC-Year FEs	Yes	Yes	No	Yes	Yes	Yes	Yes
QTB-Year FEs	Yes	Yes	No	No	Yes	Yes	Yes
$50M-Year FEs	Yes	Yes	No	No	No	Yes	Yes
Fine-Year FEs	No	No	No	No	No	Yes	No
2D-Year FEs	No	No	No	No	No	No	Yes
No. observations	188,543	188,543	188,543	188,543	188,543	188,543	188,543
No. companies	14,408	14,408	14,408	14,408	14,408	14,408	14,408

Source: Pitchbook.

Notes: This figure depicts the effects of the QSBS reforms on various outcomes for QSBS-eligible companies. Panel (a) plots the reforms' effects on a QSBS-eligible company's probability of raising financing from at least one U.S. HNWI in each year. Panel (b) plots their effects on the log accumulated amount of financing (in millions of U.S. dollars) that it raised from U.S. HNWIs. Panel (c) plots their effects on its probability of going bankrupt, staying private, and being acquired. In Panels (a) and (b), companies are considered only from the year in which they were founded until the year in which they first went bankrupt, became publicly listed, or was acquired. In contrast, for each outcome in Panel (c), companies are still considered even after the value of that outcome changes; for example, when estimating the effects on the probability of a QSBS-eligible company going bankrupt, the company is still considered even in the years after the year in which it went bankrupt. For all columns except Column (2), after we first estimate a separate regression for each founding year cohort of companies, we then take a weighted average of the estimates across cohorts, setting each cohort's weight equal to its share of all the companies considered in each year; for Column (2), we instead estimate a single pooled regression and further interact all year fixed effects with founding year cohort fixed effects. QSBS-eligbible companies refer to U.S. companies structured as C corporations ("CC"), active primarily in a qualified trade or business ("QTB"), and with less than $50 million in total financing raised by the end of 2008 ("$50M"). U.S. HNWIs refer to investors that are individuals, angel groups, and family offices and that reside or are headquartered in the U.S. "Fine-Year FEs" refer to year fixed effects specific to finer categories of legal entity types, industries, and amounts of financing raised by the end of 2008. "2D-Year FEs" refer to year fixed effects specific to each pair of the three eligibility criteria.

Appendix D: HNWIs, Income and Wealth Inequality

D.1 High-net-worth Individuals

This section validates the state-level measure of the number of resident HNWIs that we use in the regression analyses of Sections 4.2.1 and 5.1 in the main text. As described in Section 2.2, we rely on the measure from the GEOWEALTH-US dataset built by Suss et al. (2024), who define HNWIs so as to resemble the SEC's legal definition of accredited investors—that is, those whose net worth (excluding the value of their primary residence) exceeds $1 million, or whose household income exceeds $300,000. We validate this baseline measure by comparing it with alternative estimates of the number of HNWIs residing in the U.S. provided by the Phoenix/MarketCast Wealth and Affluent Monitor, the Forbes 400, the Credit Suisse/UBS Global Wealth Report, and the Survey of Consumer Finances.

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

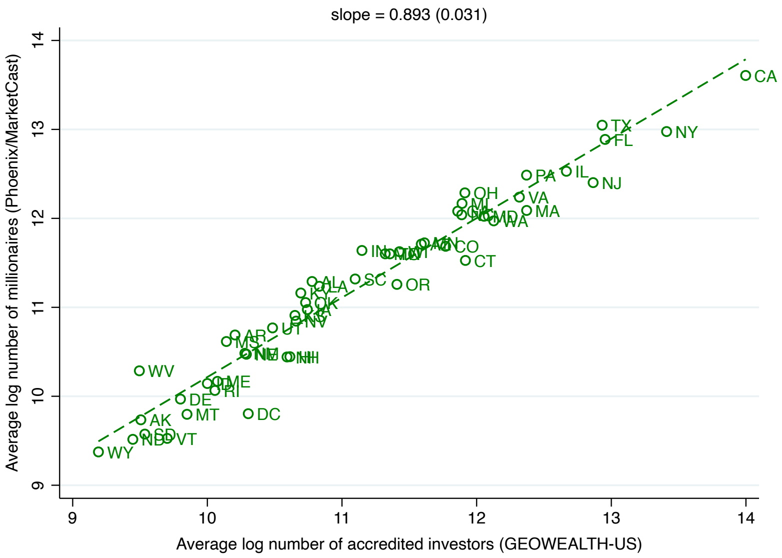
Figure D1b further depicts the correlation between the same state-level average from the GEOWEALTH-US and an analogous measure based on the Forbes 400 list of the richest Americans, which has been published in every year since 1982. We use the digitized and harmonized series from Saez and Zucman (2022).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

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

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

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

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

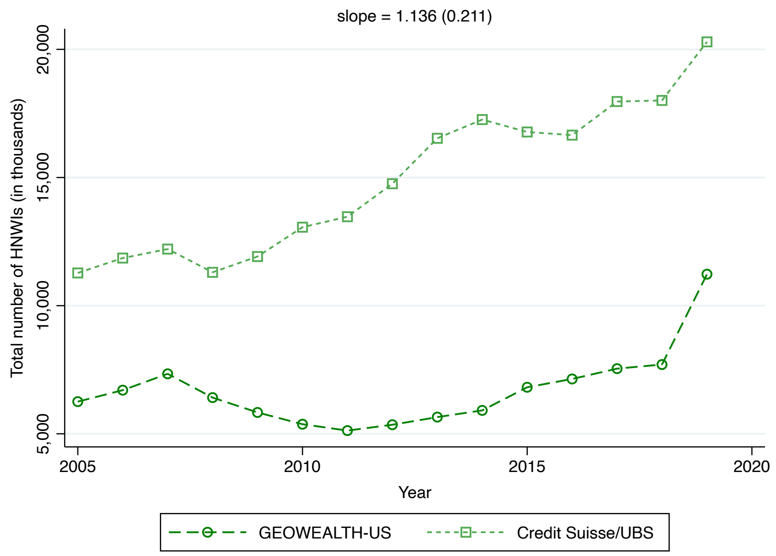


(b) GEOWEALTH-US vs. Forbes 400

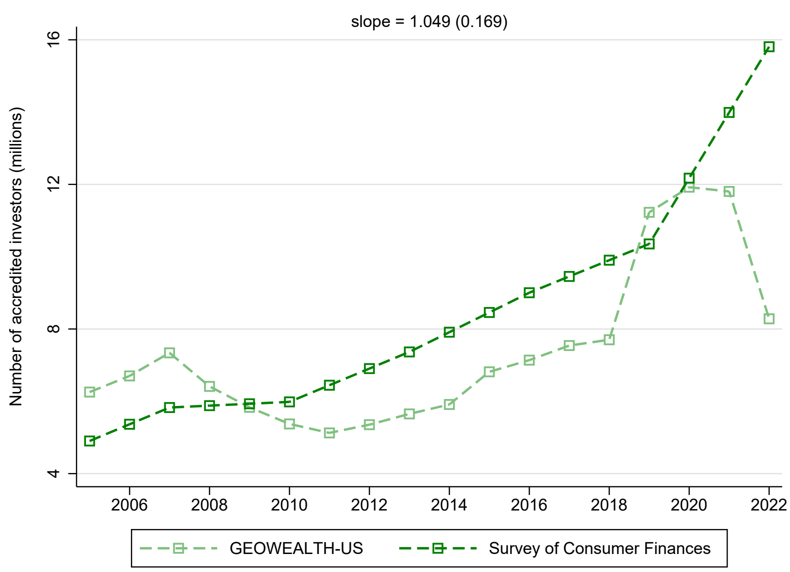
Source: Suss et al. (2024), Phoenix/MarketCast, Forbes.
Notes: This figure correlates the average log number of U.S. accredited investors across U.S. states between 2006 and 2019 in Suss et al. (2024), with the average log number of U.S. accredited millionaires across U.S. states derived by Phoenix/MarketCast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

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

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



(b) GEOWEALTH-US vs. SCF



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

D.2 Income inequality

D.2.1 Construction of the income distribution series

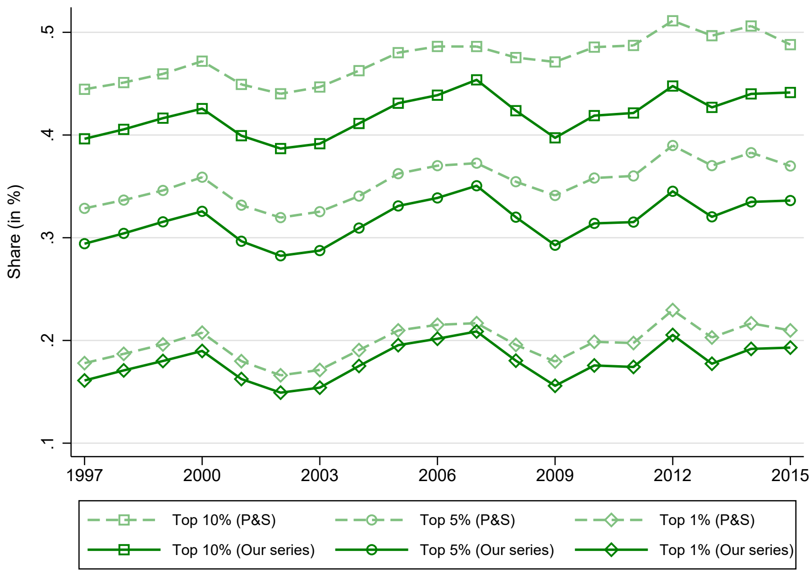
We build pre- and post-tax income distribution series for the U.S. by relying on the Internal Revenue Service's historical Statistics of Income (SOI) Tax statistics available for the period 1996-2022. For each state in each year, these data files provide personal income tax tabulations disaggregated by bracket of adjusted gross taxable income (i.e., gross taxable income net of deductibles and other adjustments[42]) and by income component (i.e., wages, business profits, capital gains, interest, etc.). The personal income tax tabulations also contain information about personal income tax liabilities by adjusted gross taxable income bracket, so that we can use these statistics to derive federal and state-level pre- and post-tax income distribution series.

We build the pre-tax income distribution series by applying the generalized pareto interpolation (GPI) method developed by Blanchet et al. (2022). GPI is a non-parametric approach that avoids the assumptions of a Pareto approximation, which are often violated by empirical data. The unit of observation in the IRS statistics is the tax unit. For every state in each year, we construct the individual and state-level pre-tax income distribution series, assuming that the reported income of couples filing jointly is shared equally between spouses. We are able to generate income shares for 127 g-percentiles, whereby the top percentile is split into smaller income groups up to the top 0.01%. Our series are consistent with those of Sommeiller and Price (2018), who build state-level pre-tax income inequality series for the U.S. also using IRS personal income tax tabulations available between 1917 and 2015 (see Appendix Figure D3). The differences between Sommeiller and Price (2018) series and ours lies on the fact that they use tax units as the unit of observation and we use individual units. As there are more joint filers at the top of the income distribution, using tax units instead of equal-splitting these units—as we do in our methodology—increases the concentration of income at the top.

Using the information available on the composition of income for each tax bracket, we further decompose the income distribution series into its different subcomponents, namely labor income, capital income (i.e., dividends, interest income, and other income from investments), and realized capital gains. We rely on these three income components, as the specific sub-components that are reported in the raw statistics each year vary over time. Appendix Table D1 summarizes which are the different IRS income subcomponents that we attribute to each of the three subcomponents into which we decompose adjusted

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

Figure D3: Income concentration in the U.S. (Price & Sommeiller vs. Our series)



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

gross taxable income (i.e., labor income, capital income and realized capital gains).

Table D1: Income components and IRS counterparts

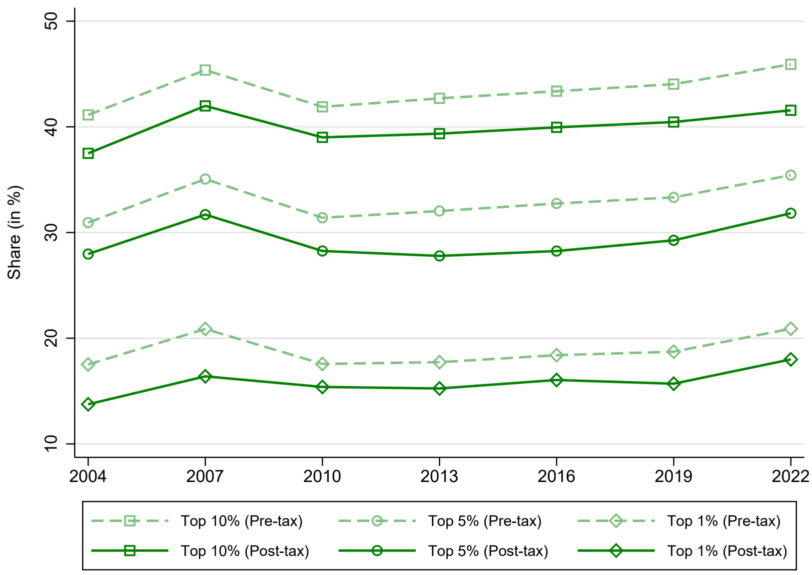
Component	**IRS equivalent**
Labor income	Salaries and wages; Social security and unemployment payments
Capital income	Taxable interest; Dividends; Business and professional net income
Capital gains	Net capital gains less losses

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

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

To build the post-tax income distribution series, we start from the federal pre-tax income distribution series. Since the raw IRS files provide information about the total personal income tax liabilities corresponding to each income bracket, we can also rely on the GPI to match the personal income tax liabilities in each bracket to its corresponding percentile. We can then estimate the post-tax income in each percentile by applying to the total pre-tax income in every percentile the average personal income tax rate corresponding to that percentile. We calculate the average personal income tax rate in each percentile as the ratio of the total personal income liabilities to the total taxable income in each percentile. Following this procedure, we obtain post-tax income distribution series for 127 g-percentiles in each year from 2004-2022. Appendix Figure D4 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.

Figure D4: Pre vs. Post-Tax Income Concentration in the U.S.



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

D.2.2 Income inequality simulations

To understand the implications of the increasing importance of U.S. HNWIs in U.S. private capital markets for the overall dynamics of income inequality, we start by carrying out two main counterfactual simulations using as baseline the U.S. actual taxable income distribution derived as explained in Appendix D.2.1. The first main counterfactual derives the income distribution had HNWIs not invested in private capital markets. The second main counterfactual estimates the income distribution had individuals instead invested the money they put in private capital markets in public capital markets, in particular, in the NASDAQ Composite Index. This is the baseline public stock market index we use, since it is the one that puts more weight on tech companies and thus it is closer to the portfolio of early-stage companies (see Appendix Figure A4). We rerun our counterfactual simulations using other public stock market and hedge fund indices as a robustness check. Results from these robustness checks analyses are presented in Appendix Table D2.

The income concept that is used to build the baseline taxable income distribution series consists of the sum of labor income, capital income and realized capital gains (see Appendix D.2.1). Hence, we cannot simply rely on the accumulated value of early-stage investments in each year depicted in Figure 2a to run our counterfactual income simulations. The reason is that this measure contains both unrealized and realized gains. Instead, we refine this measure so as to only capture the accumulated realized value of early-stage investments in each year. We do so by only considering the accumulated value of early-stage investments until the observed exit date. Whenever the exit date is not observed, we assume that the exit date happens five years after the investment was made. This assumption is based on the fact that the average exit time for observed exits for early-stage investments in Pitchbook is 4.7 years. We can thus obtain a time series of accumulated realized gains out of early-stage investments for the period 2004-2022, with which we can run the counterfactual income inequality simulations.

To run the first counterfactual, we subtract from the baseline taxable income distribution the taxable private business realized capital gains from Pitchbook, that is, the accumulated realized value of early-stage investments in each year. To run the second counterfactual, we subtract from the baseline taxable income distribution the same private business capital gains from Pitchbook and add the counterfactual taxable accumulated realized value of public gains, had they invested in the NASDAQ Composite Index.

The main challenge we face when carrying out the counterfactuals is how to distribute the actual and counterfactuals realized gains along the income distribution in the SCF. For the Forbes 400 individuals this is not an issue, as we can match the individuals on the list to their Pitchbook profiles, as explained in Appendix A.6. We can thus directly attribute on an individual basis to each Forbes 400 individual the realized capital gains they earn on these investments. In 2022, we are able to match up to 16.5% of the Forbes 400 individuals to their Pitchbook profiles.

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

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

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

We further carry out two post-tax income inequality counterfactual simulations using as baseline the U.S. actual post-tax income distribution derived as explained in Appendix D.2.1. The first main counterfactual derives the post-tax income distribution had HNWIs not invested in private capital markets. The second main counterfactual estimates the post-tax income distribution had individuals instead invested the money they put in private capital markets in public capital markets, in particular, in the NASDAQ Composite Index. Note that contrary to QSBS investments, all public stock investments capital gains are taxable.

D.3 Wealth inequality

D.3.1 Construction of the wealth distribution series

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

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

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

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

D.3.2 Wealth counterfactual simulations

To understand the implications of the increasing importance of U.S. HNWIs in U.S. private capital markets for the overall dynamics of wealth inequality, we carry out three main counterfactual simulations and compare them with the baseline U.S. actual wealth distribution derived as explained in D.3.1. The first counterfactual derives the wealth distribution had HNWIs not invested in private capital markets and thus had not obtained any gains out of these investments. The second counterfactual estimates the wealth distribution had individuals instead invested the money they put in private capital markets in public capital markets, in particular, in the NASDAQ Composite Index. This is the baseline public stock market index we use, since it is the one that puts more weight on tech companies and thus it is closer to the portfolio of early-stage companies (see Appendix Figure A4). We rerun our counterfactual simulations using other public stock market indices and a hedge fund index as a robustness check. Appendix Table D3 compares the baseline growth rate in the wealth share for different subgroups within the top 10% to the growth rate under the different counterfactual scenarios.

To run the first main counterfactual, we subtract from the baseline wealth distribution the private business capital gains from Pitchbook, that is, the accumulated value of early-stage investments in each year depicted in Figure 2a and estimated following the methodology explained in Appendix B. To run the second counterfactual, we subtract from the baseline wealth distribution the same private business capital gains from Pitchbook and add the counterfactual accumulated value of public gains depicted in Figure 2a, had they invested in the NASDAQ Composite Index.

The main challenge we face when carrying out the counterfactuals is how to distribute the actual and counterfactuals accumulated gains along the wealth distribution in the SCF. For the Forbes 400 individuals this is not an issue, as we can match the individuals on the list to their Pitchbook profiles, as explained in Appendix A.7. We can thus directly attribute on an individual basis to each Forbes 400 individual the accumulated capital gains they earn on these investments. In 2022, we are able to match up to 36% of the Forbes 400 individuals to their Pitchbook profiles.

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

To distribute the accumulated capital gains to these investors, we account for the possibility that investors with different levels of private business assets obtain different rates of return and thus different capital gains in proportion to their business assets. For that, we assume that the end-of-year value of accumulated private investments is proportional to their end-of-year business wealth and rank the population of investors in Pitchbook excluding the Forbes 400 individuals by their end-of-year value of accumulated investments every year into 100 percentiles. Similarly, we also rank the population of accredited investors in the SCF excluding the Forbes 400 individuals by their level of private business wealth (i.e., partnership or non-S corporation business wealth) into the same 100 percentiles. Since founding and non-founding investors may not be obtaining the same returns and thus may contribute differently to changing wealth inequality, we also distinguish in every percentile group between the gains accrued to founding investors, those obtained by non-founding investors, and those received by founding investors who also invest as non-founding investors. SCF households in each percentile of the business wealth distribution are randomly assigned as founding investors, non-founding investors, and founding and non-founding investors so as to match the composition of these individuals in Pitchbook. To ensure the Pitchbook distribution of end-of-year value of accumulated private investments and the SCF distribution of end-of-year private business wealth are comparable, we further adjust the SCF distribution so that the median of private business wealth for our population of investors in the SCF matches the median of accumulated private private investments in Pitchbook.

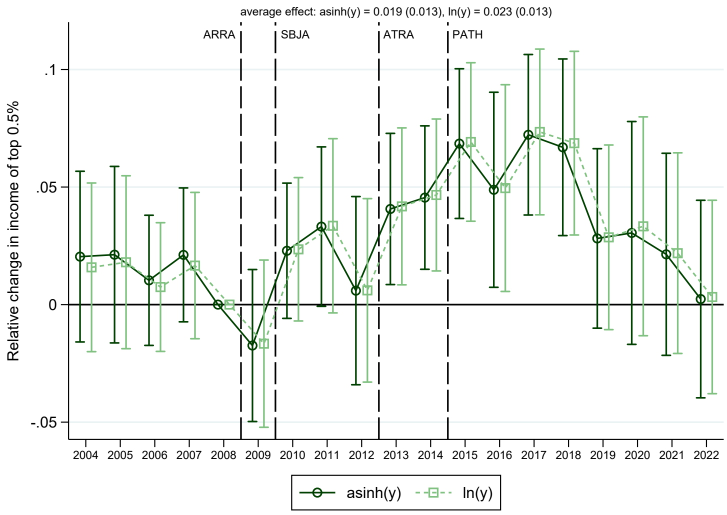
Finally, the end-of-year accumulated private investments from Pitchbook are distributed proportionally to each household's business wealth in the SCF on an annual basis. This methodology ensures heterogeneity in returns coming from the direct assignment of capital gains to the Forbes 400 individuals, from the differences in returns across the private business wealth distribution, and from the distinction between founding and non-founding investors in the imputation procedure.

We carry out the counterfactual simulations for different top wealth groups, in particular, the top 10%, top 5%, top 1%, top 0.5%, the top 0.1%, and the Forbes 400. We also decompose the effects within those wealth groups into those coming from founding, non-founding investors and both founding and non-founding investors.

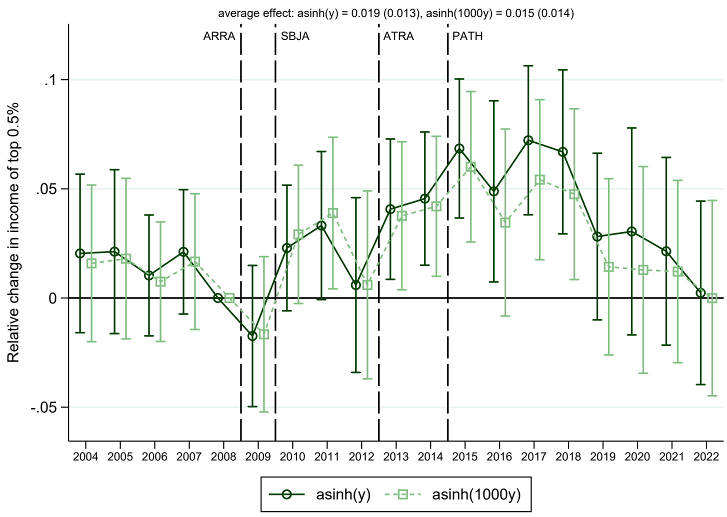
D.4 Further results

Figure D5: Robustness of Estimates of Equation (3) to Alternative Estimators

(a) asinh $Y_{r,s,t}$ vs. $\ln Y_{r,s,t}$



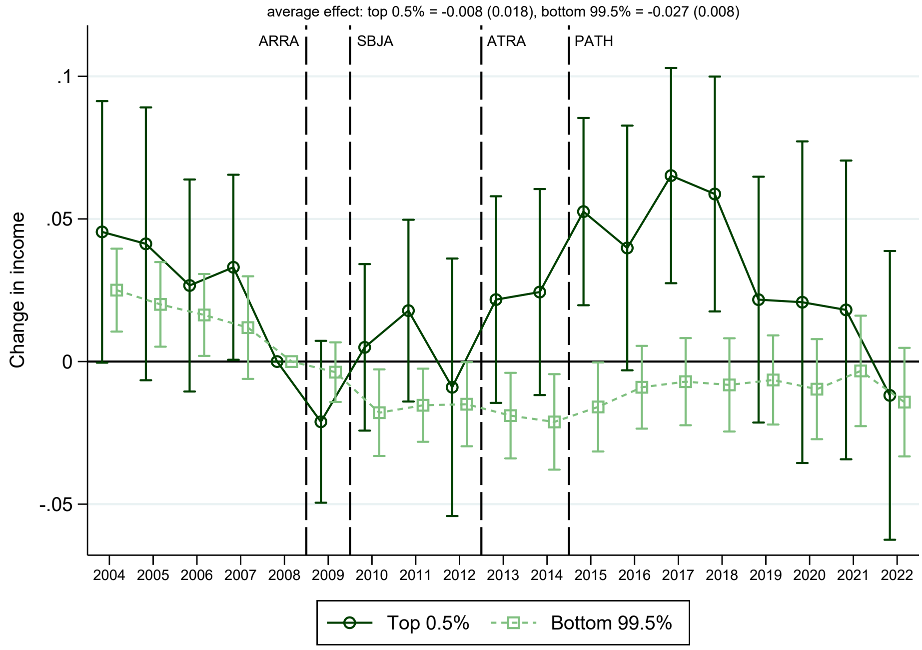
(b) asinh $Y_{r,s,t}$ vs. asinh$(1000 \times Y_{r,s,t})$



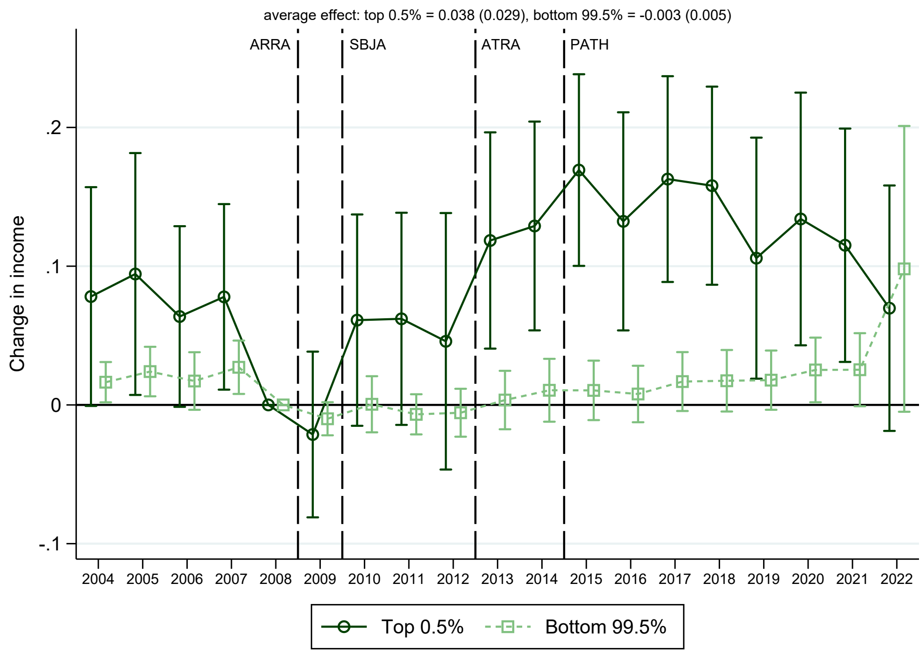
Source: SOI Tax Stats, GEOWEALTH-US.
Notes: The regression is based on 99,807 state-year observations. The average effect reported is based on a modified version of Equation (3) where β_t is replaced with $\beta_{t:t>2008}$.

Figure D6: Difference-in-Difference Estimates Underlying Equation (3)

(a) Total Income

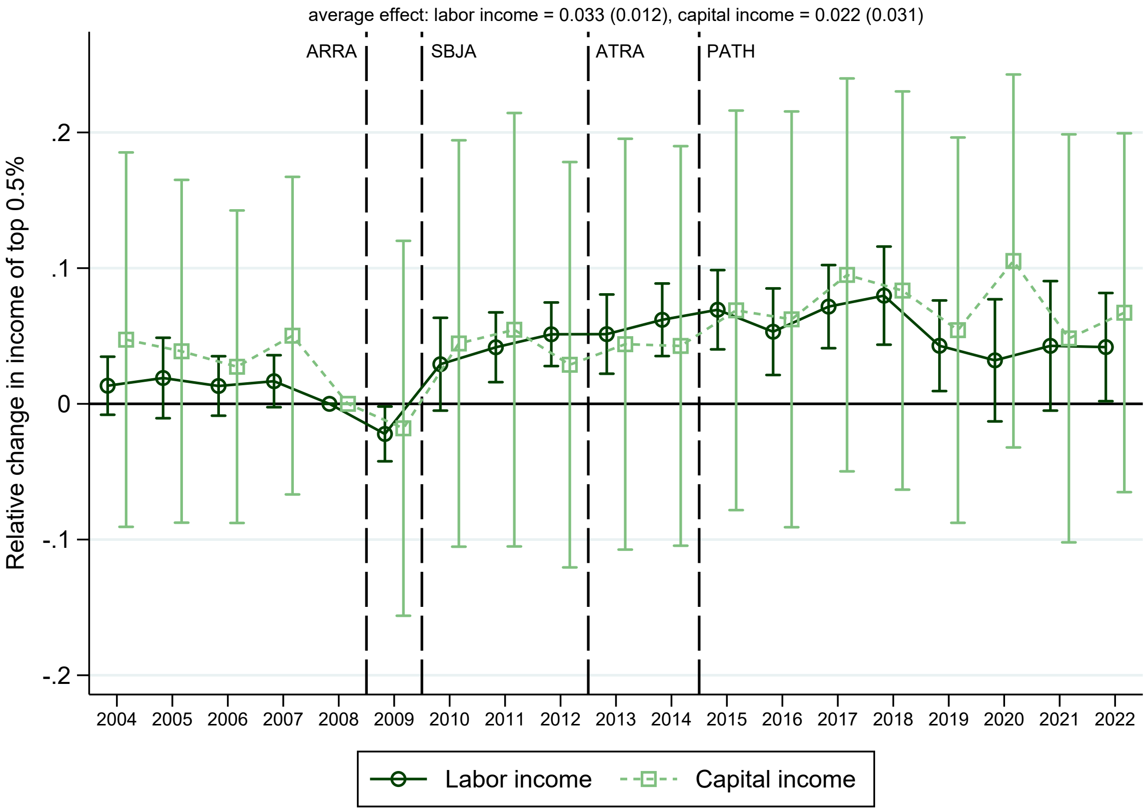


(b) Capital Gains



Source: SOI Tax Stats, GEOWEALTH-US.
Notes: The regression is based on 99,807 state-year observations. The average effect reported is based on a modified version of Equation (3) where β_t is replaced with $\beta_{t:t>2008}$.

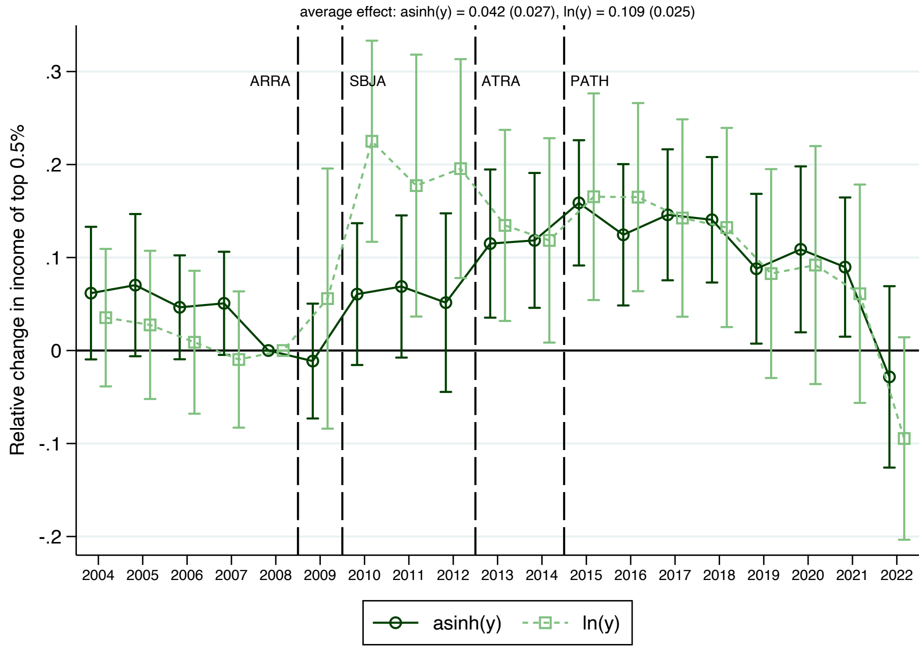
Figure D7: Decomposition of Other Income



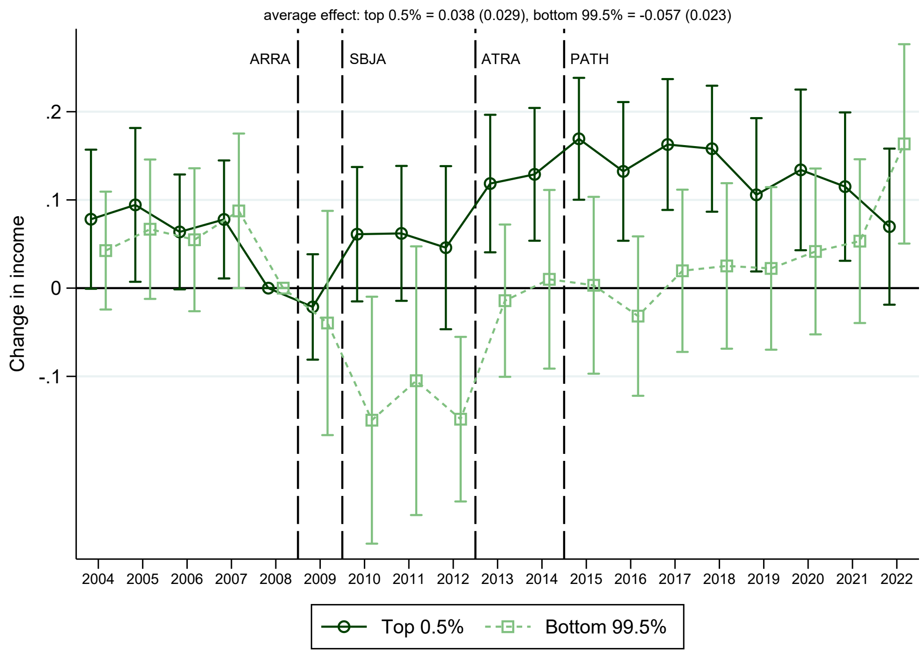
Source: SOI Tax Stats, GEOWEALTH-US.
Notes: The regression is based on 99,807 state-year observations. The average effect reported is based on a modified version of Equation (3) where β_t is replaced with $\beta_{t:t>2008}$.

Figure D8: Robustness of Effects on Capital Gains: asinh $Y_{r,s,t}$ vs. $\ln Y_{r,s,t}$

(a) Triple-Difference Estimates



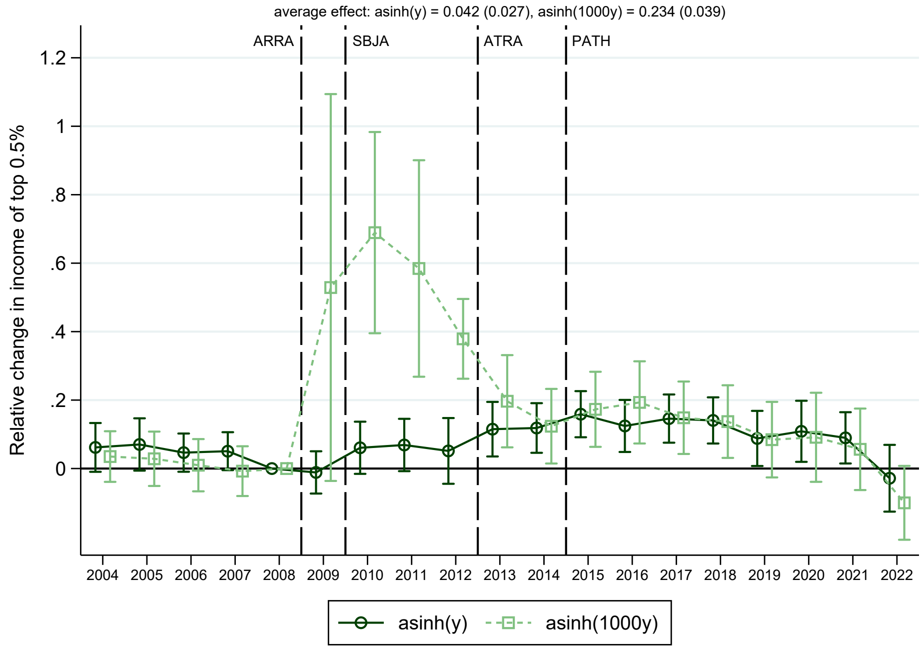
(b) Difference-in-Difference Estimates: $\ln Y_{r,s,t}$



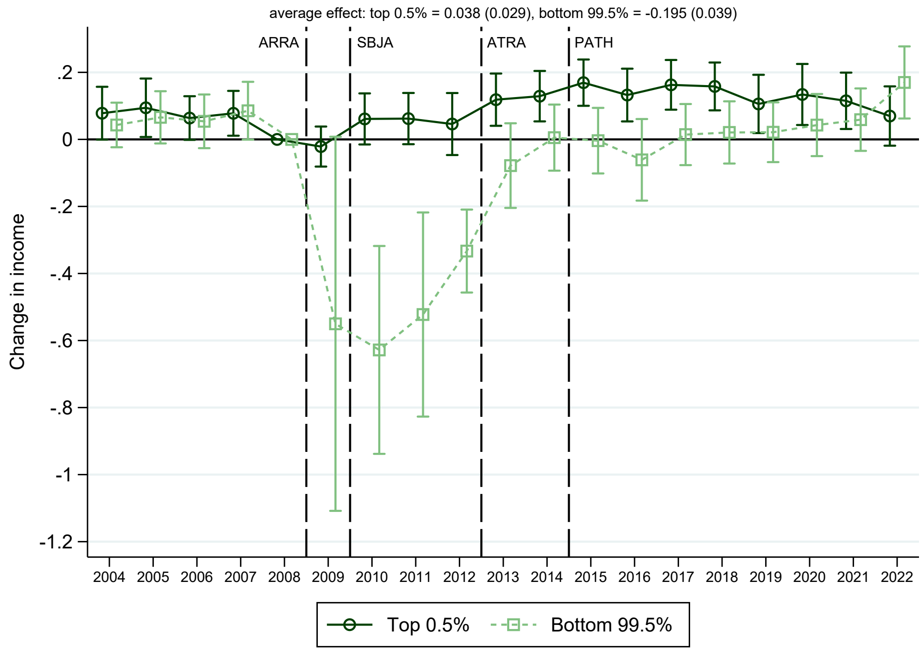
Source: SOI Tax Stats, GEOWEALTH-US.
Notes: The regression is based on 99,807 state-year observations. The average effect reported is based on a modified version of Equation (3) where β_t is replaced with $\beta_{t:t>2008}$.

Figure D9: Robustness of Effects on Capital Gains: asinh $Y_{r,s,t}$ vs. asinh$(1000 \times Y_{r,s,t})$

(a) Triple-Difference Estimates



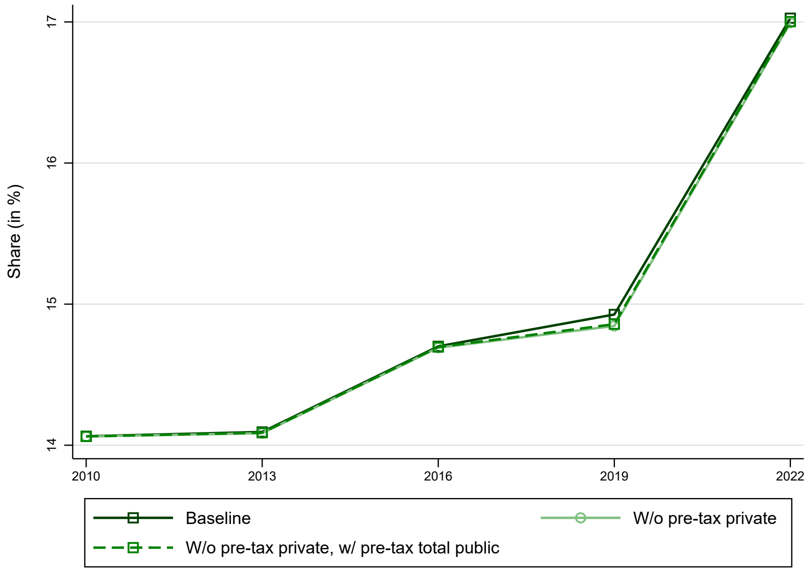
(b) Difference-in-Difference Estimates: asinh$(1000 \times Y_{r,s,t})$



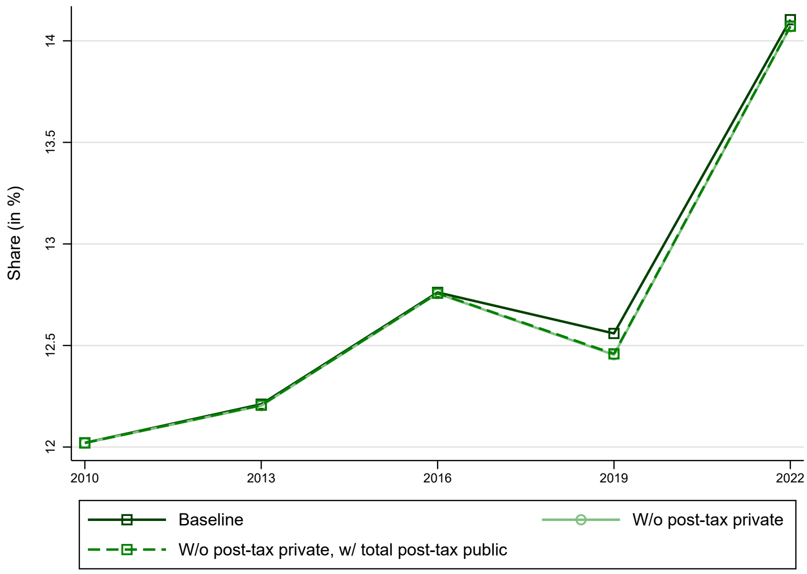
Source: SOI Tax Stats, GEOWEALTH-US.
Notes: The regression is based on 99,807 state-year observations. The average effect reported is based on a modified version of Equation (3) where β_t is replaced with $\beta_{t:t>2008}$.

Figure D10: Pre- and Post-Tax Income Inequality Simulations (2010-2022)

(a) Top 0.5% pre-tax income shares



(b) Top 0.5% post-tax income shares

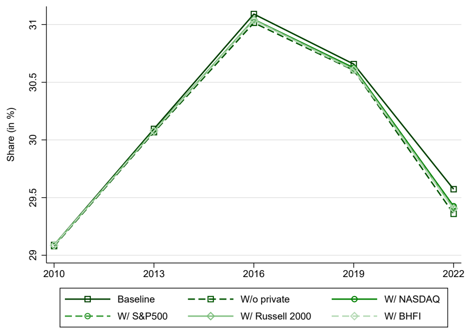


Source: IRS, SCF, FA, Forbes.
Notes: This figure compares the baseline top 0.5% pre-tax and post-tax income shares to different counterfactual top 0.5% income shares over the 2010-2022 period. Panel (a) compares the baseline top 0.5% pre-tax income share to three different counterfactuals: no taxable private capital gains; no taxable capital gains but with counterfactual taxable public capital gains using NASDAQ as the public stock market index; and no taxable private capital gains but with total (both taxable and non-taxable) private capital gains. Panel (b) compares the baseline top 0.5% post-tax income share to three different counterfactuals: no post-tax private capital gains; and no post-tax capital gains but with counterfactual taxable public capital gains using NASDAQ as the public stock market index. The baseline income distribution series are derived by using the IRS Statistics of Income. Appendix D.2 provides more details about the methodology used to estimate the baseline income distribution series and carry out the counterfactuals.

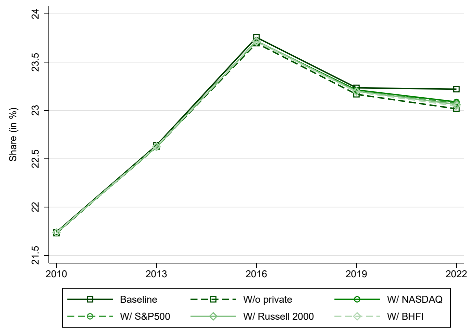
Figure D11: Wealth Inequality Counterfactuals for Different Wealth Groups

(a) Top 10%

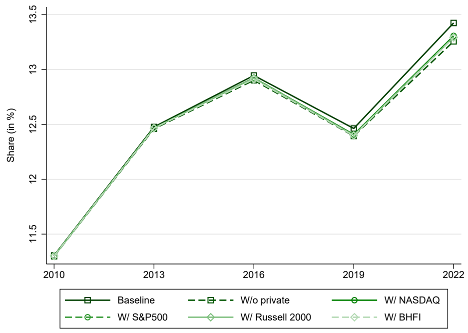
(b) Top 5%



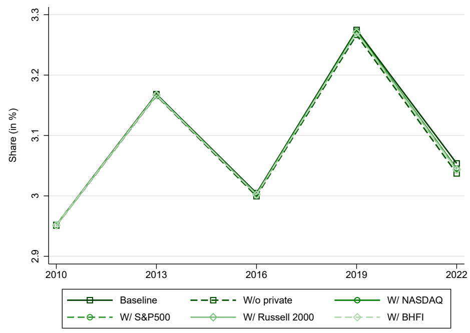
(c) Top 1%



(d) Top 0.5%



(e) Top 0.1%



(f) Forbes 400

Source: SCF, Pitchbook, FA, Forbes.
Notes: This figure compares the baseline wealth distribution series to the counterfactual wealth distribution series over the period 2010-2022 under five different scenarios: no private capital gains; no private capital gains but with counterfactual public capital gains using the NASDAQ Composite Index; no private capital gains but with counterfactual public capital gains using the S&P 500 Composite Index; no private capital gains but with counterfactual public capital gains using the Russell 200 Index; and no private capital gains but with counterfactual public capital gains using the Barclay Hedge Fund Index. Each panel compares the baseline series to each counterfactual series for different wealth groups: top 10% (a); top 5% (b); top 1% (c); top 0.5% (d); top 0.1% (e); and Forbes 400 (f). The baseline wealth distribution series are derived by combining the Survey of Consumer Finances (SCF) with the Financial Accounts (FA) and the Forbes 400 rich lists following the Distributional Financial Accounts methodology developed by Batty et al. (2020). Appendix D.3.2 provides more details about the methodology used to estimate the baseline wealth distribution series and carry out the counterfactuals.

Table D2: Wealth Inequality Counterfactuals (Growth Rates, 2010-2022)

	Baseline	W/o private	W/o private			
			W/ NASDAQ	W/ S&P 500	W/ RUT	W/ BHFI
Top 10% to 5%	-9.93%	-9.65%	-9.74%	-9.73%	-9.72%	-9.71%
	100	97	98	98	98	98
Top 5% to 1%	-11.12%	-10.91%	-10.97%	-10.95%	-10.95%	-10.94%
	100	98	99	98	98	98
Top 1% to 0.5%	-13.54%	-13.69%	-13.80%	-13.79%	-13.70%	-13.56%
	100	101	102	102	101	100
Top 0.5% to 0.1%	-6.14%	-6.50%	-6.28%	-6.36%	-6.37%	-6.38%
	100	106	102	104	104	104
Top 0.1% to Forbes 400	24.14%	22.36%	22.88%	22.79%	22.71%	22.66%
	100	93	95	94	94	94
Forbes 400	3.45%	2.92%	3.18%	3.15%	3.12%	3.11%
	100	85	92	91	90	90

Source: Survey of Consumer Finances, Pitchbook, FA, Forbes 400.
Notes: This table compares the growth rates between the baseline top wealth shares and the different counterfactuals (without private capital gains, and without private capital gains but with any of the different public market indices (i.e., NASDAQ, S&P 500, Russell 2000 and Barclay Hedge Fund Index)) between 2010 and 2022.

Table D3: Heterogeneity of Growth Rates by Type of Investor (Baseline vs. Counterfactuals)

	Baseline	W/o Private	NASDAQ	S&P 500	Russell	BHFI
Panel A: Top 10% to top 5%						
Nf investor	215.69%	211.30%	215.16%	215.02%	214.79%	214.74%
F investor	131.55%	131.46%	131.44%	131.41%	131.38%	131.37%
F & Nf investor	1223.59%	1224.37%	1224.53%	1224.16%	1223.98%	1223.77%
Non-investor	-5.69%	-5.61%	-5.68%	-5.68%	-5.68%	-5.67%
Panel B: Top 5% to top 1%						
Nf investor	68.33%	66.96%	68.36%	68.29%	68.15%	68.13%
F investor	40.20%	40.22%	40.15%	40.14%	40.14%	40.14%
F & Nf investor	300.56%	301.93%	302.80%	302.59%	302.39%	302.28%
Non-investor	-7.39%	-7.31%	-7.39%	-7.38%	-7.38%	-7.37%
Panel C: Top 1% to top 0.5%						
Nf investor	-36.38%	-35.22%	-36.89%	-36.94%	-36.60%	-35.98%
F investor	5.17%	5.59%	5.62%	5.62%	5.53%	5.36%
F & Nf investor	-60.42%	-60.39%	-60.53%	-60.44%	-60.48%	-60.45%
Non-investor	18.49%	17.93%	18.58%	18.59%	18.50%	18.31%
Panel D: Top 0.5% to top 0.1%						
Nf investor	26.07%	22.66%	24.41%	24.31%	24.05%	23.73%
F investor	-19.67%	-19.09%	-19.26%	-19.24%	-19.17%	-19.07%
F & Nf investor	38.80%	43.61%	43.39%	43.16%	43.19%	43.17%
Non-investor	-11.20%	-10.16%	-10.84%	-10.79%	-10.69%	-10.57%
Panel E: Top 0.1% to Forbes 400						
Nf investor	13.67%	12.89%	13.12%	13.09%	13.06%	13.04%
F investor	90.11%	91.88%	91.58%	91.54%	91.54%	91.51%
F & Nf investor	108.22%	103.59%	104.07%	104.02%	104.00%	103.96%
Non-investor	-39.92%	-38.43%	-38.74%	-38.69%	-38.66%	-38.63%

Source: Survey of Consumer Finances, Pitchbook, FA, Forbes 400.
Notes: This table compares the growth rates of the wealth shares of non-founding investors, founding investors, both founding and non-founding investors and non-investors out of the total wealth share in each wealth group (different panels document these numbers for different top wealth groups). The growth rates are estimated for baseline top wealth shares and the different counterfactuals (without private capital gains, and without private capital gains but with any of the different public market indices (i.e., NASDAQ, S&P 500, Russell 2000 and Barclay Hedge Fund Index)) over the 2010 to 2022 period.