Taxing Top Wealth: Migration Responses and their Aggregate Economic Implications*

Katrine JAKOBSEN, Henrik KLEVEN, Jonas KOLSRUD,
Camille LANDAIS, and Mathilde MUNOZ

June 12, 2025

Abstract

Using administrative data from Scandinavian countries, we provide evidence on international migration responses to wealth taxes and evaluate their aggregate economic implications. We find significant migration responses among the wealthy: a 1pp increase in the top wealth tax rate decreases the stock of wealthy taxpayers by about 2%. A large fraction of the wealthy are business owners, and their businesses are negatively affected by owner out-migration. The aggregate effects are nevertheless modest: the migration responses to a 1pp increase in the top wealth tax rate reduce employment by 0.02%, investments by 0.07%, and value-added by 0.10%.

*Jakobsen: University of Copenhagen (katrinemariejakobsen@gmail.com); Kleven: Princeton University, NBER, and CEPR (kleven@princeton.edu); Kolsrud: Linnaeus University (jonas.kolsrud@lnu.se); Landais: London School of Economics and CEPR (c.landais@lse.ac.uk); Munoz: UC Berkeley, NBER, and CEPR (mathildemunoz@berkeley.edu). We are grateful to numerous seminar and conference participants for helpful comments and reactions. We thank Rebecca Cambrini, Ethan Ward, Lukas Rodrian, Quirin von Blomberg, Thomas Mikaelsen, and Nicolas Grimprel for outstanding research assistance. Munoz acknowledges financial support from the Stone Center at UC Berkeley. Jakobsen acknowledges financial support from the Independent Research Fund Denmark, Grant #0128-00007B. We also acknowledge support from the Center for Economic Behavior and Inequality (CEBI) at the University of Copenhagen (Grant #DNRF134).

1 Introduction

Following recent proposals to introduce wealth taxes on the rich (Saez and Zucman, 2019), a growing academic literature studies the effects of wealth taxation on behavior and wealth accumulation (Seim, 2017; Duran-Cabré et al., 2019; Jakobsen et al., 2020; Scheuer and Slemrod, 2021; Brülhart et al., 2022; Londoño-Vélez and Avila-Mahecha, 2023). While this literature has produced a number of important insights, it offers little guidance on an issue that looms large in the public debate: migration responses.

Two claims often dominate the debate on wealth taxation. First, that wealthy individuals will relocate abroad *en masse* in response to such taxes. Second, that because many of the wealthy are entrepreneurs and business owners, their departure will generate large negative spillovers on the broader economy. These concerns underpin the argument that migration responses make wealth taxes economically self-defeating. Yet there is little empirical evidence to support—or to rule out—the significance of these effects. International migration responses to wealth taxes are understudied due to both data limitations and a scarcity of credible identifying variation in wealth taxes.

We leverage administrative data on wealth, entrepreneurship, and migration from Sweden and Denmark to study these questions. At the core of our analysis is a novel two-step approach for estimating the aggregate economic effects of migration responses to wealth taxes. First, we estimate migration responses using quasi-experimental variation from three major wealth tax reforms. Second, we map these responses into aggregate effects using event studies of out-migration, not relying directly on the tax reforms.[1] The richness of our data allows us to conduct these event studies across a wide range of outcomes, including individual-, firm-, and market-level measures. We discuss and validate the statistical assumptions of our approach. By addressing challenges related to both identification and statistical precision, the method provides a framework for estimating general equilibrium effects and externalities that could be applied to a range of other settings.

An important feature of our data is that we observe all components of taxable and non-taxable wealth, including business assets (in listed and unlisted firms) controlled directly or indirectly by the wealthy.[2] This allows us to show that the wealthy are indeed disproportionately represented among entrepreneurs, and that the businesses they own are important for the aggregate economy. Through their privately held businesses, individuals in the top 2% of the wealth distribution control close to 10% of total Swedish employment and 15% of total Swedish value added. In light of existing research showing that the death or retirement of entrepreneurs have strong negative effects

[1] Importantly, while the event studies of migration are independent of the wealth tax reforms, we implement the analysis on a sample of households who are at the margin of reform-induced migration.

[2] The rich administrative data infrastructure alleviates some of the traditional concerns related to measurement error in wealth when capitalizing incomes (Agrawal et al., 2023), relying on public non-administrative wealth information (Moretti and Wilson, 2023), or using self-reported measures of taxable wealth (Brülhart et al., 2022).

on their businesses (Smith et al., 2019; Jäger and Heining, 2022), it seems *a priori* reasonable to believe that the out-migration of wealthy entrepreneurs also has significant negative effects on business activity in the country they leave.

Our empirical analysis is divided into three main parts. First, we leverage the unexpected repeal of the Swedish wealth tax in 2006 to estimate international migration responses to wealth taxation. Using a difference-in-differences design, we find clear evidence of migration responses. After the reform, out-migration dropped sharply for those exposed to the wealth tax shock relative to those unexposed. We estimate that the wealth tax repeal reduced the propensity of wealthy individuals to leave Sweden by 30%. Expressing the effects as semi-elasticities of migration rates with respect to the net-of-tax rate on wealth, we find that a one percent increase in the net-of-tax rate reduces the out-migration rate by 0.17pp and increases the in-migration rate by 0.05pp. The effects on migration *flows* translate to modest effects on the stock of wealthy individuals: a one percent increase in the net-of-tax rate increases the stock of wealthy taxpayers by about 2 percent in steady state. The modest stock effect is due to the fact that migration flows, while being quite responsive to wealth taxes, constitute a small fraction of the stock of wealthy households.

A rare feature of our study lies in the ability to replicate the analysis in a different context, taking advantage of two large wealth tax reforms in Denmark. The Danish reforms happened earlier, in the 1980s and 1990s, and the Danish wealth tax was structured differently than the Swedish one.[3] We find similar migration elasticities in Denmark, lending support to the internal and external validity of our results.

Second, we find clear and precisely estimated effects of out-migration events among wealthy taxpayers. These events create large declines in taxable wealth, income, and tax payments along with significant changes in portfolio composition and business assets. We show that, when an entrepreneur subject to the wealth tax leaves the country, the employment in their businesses drops by 33%, gross investments by 22%, value-added by 34%, and tax payments by 51%. These effects are driven mostly by the extensive margin of firm closure. Our data allow us to go beyond firm-level effects, exploring the potential reallocation of economic activity around out-migration events. We find a substantial reallocation of activity: 45% of the firms closed by their wealthy owners upon out-migration end up being absorbed by other firms in Sweden, and the employees of these firms experience limited persistent losses in earnings and employment. Hence, our results suggest that the economy-wide impact of migration by wealthy entrepreneurs is mitigated by reallocation forces in the Swedish economy. We also investigate the presence of market-wide externalities at the local and sectoral level, but find limited evidence of such spillovers.

[3] See Jakobsen, Jakobsen, Kleven and Zucman (2020) for an analysis of *intensive* margin effects of the Danish wealth tax reforms on wealth accumulation.

Third and finally, we combine the event studies of migration (giving the effects of migration on a wide range of outcomes) with the quasi-experimental estimates of migration elasticities (giving the effect of taxes on migration) to quantify the aggregate economic implications of tax-induced migration among the wealthy. We find that the effects of tax-induced migration on aggregate economic activity are modest. A one percentage point increase in the top wealth tax rate decreases aggregate employment by 0.02%, aggregate investment by 0.07%, and aggregate value-added by 0.10% in the long run. Note that the effects are modest despite the fact that wealthy entrepreneurs account for a substantial share of overall economic activity through the firms they control directly and indirectly.

Even after accounting for the fiscal externalities on other tax bases, the revenue implications of migration responses are much too small to make the abolition of wealth taxes pay for themselves. For each additional dollar of revenue raised by the wealth tax, only 0.22 dollars are lost due to migration responses. This can be compared to the 0.54 dollars lost due to intensive margin responses—through changes in savings, investments, avoidance, and evasion—which we calculate based on the estimates in Jakobsen, Jakobsen, Kleven and Zucman (2020). Our findings imply that migration threats, while taking center stage in the public debate, are far less important for welfare and policy design than intensive margin responses.

Our paper contributes to several strands of literature. First and foremost, we contribute to the nascent literatures on behavioral responses to wealth taxation (e.g., Seim, 2017; Duran-Cabré et al., 2019; Jakobsen et al., 2020; Brülhart et al., 2022) and on migration responses to taxes more broadly (e.g., Kleven et al., 2013, 2014, 2020; Akcigit et al., 2016; Advani et al., 2023). The former is virtually silent on international migration responses, while the latter is focused mostly on migration responses to *income* taxes. Evidence on migration responses to wealth taxes is scarce and focused almost entirely on within-country mobility (Brülhart et al., 2022; Agrawal et al., 2023; Moretti and Wilson, 2023; Iacono and Smedsvik, 2024).[4] Top wealth holders tend to own businesses—much more so than top income earners—and studying their international migration decisions is therefore critical. The notion that tax-induced migration of wealthy entrepreneurs will have large spillovers on business activity is widespread in the policy debate. A key contribution of our paper is to develop a simple two-step procedure to quantify such "trickle-down" effects: quasi-experimental estimates of migration responses combined with event study estimates of the effects of migration on outcomes. An advantage of the approach is that it can be applied across a range of settings.

[4] Agrawal, Foremny and Martínez-Toledano (2023) compare the number of wealthy individuals filing taxes inside and outside of Madrid, following a wealth tax change in Madrid. Brülhart, Gruber, Krapf and Schmidheiny (2022) use a similar empirical strategy, comparing two cantons in Switzerland. Moretti and Wilson (2023) investigate mobility across US states due to the estate tax—a one-time wealth tax at death—among Forbes 400 individuals. Iacono and Smedsvik (2024) consider mobility responses to a municipal wealth tax reform in Norway. Finally, Dray, Landais and Stantcheva (2023) focus on the introduction of property taxes in the US in the 19th century.

Using this approach, we find that spillover effects of tax-induced migration, while real, are modest in size.

Our results also speak to a body of work investigating the impact of managers and CEOs on firm performance, using variation from retirements, family successions, and deaths. These studies have found large negative effects of CEO death and retirement on firm performance (e.g., Smith et al., 2019; Jäger and Heining, 2022). We study a different type of owner-specific event (migration) which is both policy-relevant (as policy can directly affect it) and salient in the public debate. Consistent with this literature, we show that owners matter: out-migration of wealthy individuals affects the economic outcomes of the firms they control. At the same time, our effects are much smaller in magnitude due to mitigation from firm restructuring and sale.[5] Many owners retain control of their firms when they move abroad, or they restructure their activity rather than shutting the firms down entirely. For these reasons, migration is less disruptive for business activity than retirement or death.[6]

The rest of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 describes our data and provides descriptive statistics on the international migration patterns of the wealthy. Section 5 estimates the causal impact of wealth taxes on migration flows using tax reforms. Section 6 investigates the impact of migration events on individual-level, firm-level, and market-level outcomes. Section 7 draws policy implications, while section 8 concludes.

2 Institutional Background

2.1 Wealth Taxation in Sweden

Sweden has a long history of progressive wealth taxation: it levied an annual progressive wealth tax on the net value of assets between 1911 and 2007. Our focus is on the twenty-year window around the abolition of the Swedish wealth tax in 2007. We describe below the key institutional features of the wealth tax during this period.

Tax Base: Taxable wealth was defined at the household level and included financial and non-financial assets, assessed at market value, minus debt. A number of exemptions affected the tax base. First, a 25% exemption was applied to the value of real estate assets. Second, pension savings, art, and jewellery were fully exempt from the wealth tax. Third, certain stocks benefited

[5] For example, Smith et al. (2019) find a 26 percentage point decrease in firm survival and an 82% drop of profits per worker following owner retirement events (after accounting for buy-outs). Even after conditioning on firm survival, they find a 45% decrease in profits per worker. By contrast, we find almost no effect of owner out-migration after we condition for firm survival.

[6] We note that a recent policy report (Bach et al., 2023) studies what happens to firm outcomes when a direct owner emigrates from France, finding similar results to ours in terms of magnitudes.

from exemptions depending on firm type and ownership structure. Stocks registered on a stock exchange ("A-list" shares) were taxable at 80% of their full market value.[7] From 1991 onwards, shares of unlisted firms and firms on the so-called O-list of the Stockholm stock exchange (small caps and start-ups) were exempt. Finally, and most importantly, wealth from privately-held businesses where individuals owned more than 25% was fully exempt from taxation. Despite these exemptions, as we show later, the effective wealth tax rate was substantial: an *average* tax rate of about 0.5% at the top of the wealth distribution.

Reporting & Enforcement: The reporting and enforcement system for the Swedish wealth tax was very sophisticated compared to most other countries operating a wealth tax. Assessment was conducted each year, with most wealth components being third-party reported. At the end of each year t, third parties sent detailed reports on taxpayers' financial and real estate assets, as well as liabilities, to the tax authority. The government used market prices from stock and real-estate markets to value those assets. The information was recorded in comprehensive administrative registries: the land registry and the financial asset registry (KURU). Taxpayers received pre-populated wealth tax returns at the beginning of year $t+1$ and had to self-report any wealth components that were not third-party reported. Self-reported components of taxable wealth included cars and other durables as well as stocks in closely-held businesses. For the latter, taxpayers were required to report on a dedicated form (K10 tax form) the number and prices of shares in closely-held businesses, as well as any transaction or dividend payments related to those shares. To prevent people from evading wealth taxation by artificially registering their personal assets as (tax-exempt) business assets, firm owners had to prove to the tax authority that such assets were essential to their firm's operation. The tax authority used several methods to enforce these rules. For instance, if a firm's *quick ratio* (i.e., the ratio of quickly available or liquid assets to current liabilities) exceeded 200 percent, then the excess liquidity was not counted as a business asset and was taxed as personal wealth.[8]

Despite the best efforts of the tax administration, evidence suggests that tax evasion was prevalent at the top of the wealth distribution (Boas et al., 2024). In section 5.5, we explore the implications of tax evasion for the robustness of our results using available estimates of the amount of wealth tax evasion.

Residence Rules: The Swedish wealth tax applied to Swedish residents, who were taxed on the value of their worldwide assets. For tax purposes, residence is evaluated as living in Sweden or being a permanent resident of Sweden. Furthermore, according to the "five-year rule", individu-

[7] This rule varied over time: A-list shares were taxable at 75% of their value from 1978 through 1996 and at 100% prior to 1978.

[8] The tax authority used a second rule of thumb, by which a firm could not have liquid assets amounting to more than 1 million SEK. Amounts exceeding 1 million were not considered as pivotal for the firm's operations and therefore were subject to the wealth tax.

als continue to be considered Swedish residents for five years, counting from the day they move abroad, if they maintain a significant economic connection to Sweden. Tax authorities assess residency on a case-by-case basis.[9]

Even if households were deemed non-resident for tax purposes, they had to pay wealth taxes on the assets still held in Sweden, according to the "limited tax liability regime" (*begränsad skattskyldighet*).[10] Hence, emigrating from Sweden was not enough to avoid wealth taxation by itself: one would also have to reallocate taxable assets out of Sweden, creating potential distortions to wealth allocation and investment decisions. In practice, however, the enforcement of the wealth tax for non-residents was relatively weak, and special bilateral treaties offered an additional grey area. Therefore, little is known about the impact of the Swedish wealth tax on asset location. Our empirical analysis will shed light on this question.

Tax Schedule: The Swedish wealth tax was levied at the household level. After the 1991 reform and until its abolition in 2007, the Swedish wealth tax had a simple two-bracket structure: the marginal tax rate was equal to 1.5% above a threshold and zero below it.[11] The exemption threshold varied over time: starting from SEK 800K in 1991, it was increased in the early 2000s to reach 3,000K in 2006. In 2001, a separate (lower) threshold was introduced for single individuals. Panel A of Appendix Figure I.1 shows the evolution of the wealth tax threshold over time, expressed as a percentile of the household taxable wealth distribution. The threshold was at its lowest in 1999, at which point all households above the 92nd percentile of the taxable wealth distribution were taxable. In 2006, the threshold had been raised significantly so that only couples belonging to the top 2% of the taxable wealth distribution were liable to the tax. For singles, the lower threshold meant that in 2006 they remained taxable if their taxable wealth was above the 92nd percentile of the distribution. Panel B of Appendix Figure I.1 shows a similar exercise, but where we rank households by their net wealth (instead of their taxable wealth). The graph shows, for different quantiles of the net wealth distribution, average taxable wealth against the exemption threshold for singles and couples in 2006. It confirms that singles were taxable if they belonged to the top 8% of the net wealth distribution, while couples were taxable if they belonged to the top 2% of the distribution. In what follows, we consider households with total net wealth in the top 2% of

[9] See e.g. https://skatteverket.se/privat/internationellt/bosattutomlands. Note that ownership of a business, in the absence of active management or control, is generally not deemed a sufficient condition to establish a significant economic connection to Sweden. Furthermore, Sweden has double-tax agreements with many countries to prevent double taxation and offer tax reliefs if taxes are paid in the new country of residence.

[10] We also note that there are no formal exit taxes in Sweden, but the government introduced a rule to tax realized capital gains upon migration. The "ten-year rule" allows the Swedish government to tax capital gains realized within a period of ten years after a Swedish resident left Sweden.

[11] A tax credit for the wealth tax applied through a tax ceiling mechanism capping the amount of wealth tax owed by taxpayers as a fraction of their taxable income. However, the wealth tax could not be reduced below the amount due on 50% of taxable wealth, which provided a floor for wealth tax payments.

the distribution as *fully treated* by the wealth tax over the period 1999-2006, while we consider households with total net wealth between the top 8% and top 2% of the distribution as *partially treated*. Households with net wealth below the 92nd percentile are considered untreated.

Other Taxes on Capital and on Income from Capital: Sweden has a dual income tax system, where capital income (fixed income, dividends, capital gains, etc.) is taxed at a flat 30 percent rate, above an exemption threshold. Special rules (known as "3:12 rules") apply to closely held businesses to prevent that entrepreneurs avoid progressive taxation of wage income by shifting labor income to dividends.[12] The 3:12 rules put a cap on the amount of dividends and capital gains that can be taxed at the flat rate on capital income.

In addition to the wealth tax, Sweden levied two other taxes that applied to the stock of wealth rather than to the income flow from wealth. First, Sweden has a property tax. This tax was administered centrally until 2008, after which it was replaced by a municipal-level "fee." Second, Sweden used to levy inheritance taxes, but these were gradually abolished between 2003 and 2005. Importantly, even before their abolition, inheritance taxes had little bite on top wealth holders as they had been greatly reduced in 1991. What is more, inheritance taxation was unlikely to affect the location decisions of the wealthy because stringent residence rules made it hard to avoid inheritance taxes by migrating out of Sweden. Deceased individuals were required to have lived outside Sweden and stopped being tax residents for at least ten years prior to the time of death for their assets not to be subject to inheritance taxation in Sweden. For these reasons, inheritance tax changes in the early 2000s are unlikely to confound our estimates of migration responses to wealth tax reform in 2007.

Abolition of the Wealth Tax in 2007: During the period 1999-2006, despite the increase in the exemption threshold, the Swedish wealth tax continued to generate substantial revenue.[13] For households in the top 2% of the wealth distribution, the wealth tax played a central role compared to other forms of capital taxation. Their average wealth tax rate remained stable at around 0.5% of total net wealth in the years leading up to the 2007 reform (see Figure 3, Panel A). This number can be compared to their total capital tax payments (the sum of capital income taxes, property taxes, inheritance taxes, and wealth taxes), which amounted to about 1% on their total net wealth (see Appendix Figure I.3). In other words, wealth taxation in Sweden, prior to its repeal, represented roughly 50% of all taxes on the capital stock and capital income of the wealthy.

At the general election in October 2006, the Social Democratic Party experienced a surprise loss

[12] For the purpose of the 3:12 rules, closely held businesses (*fåmansbolag*) are defined by the Swedish authorities as companies where the four largest owners (or fewer) together have more than 50% of the votes in this company. All close relatives are counted as one owner to avoid that family members split ownership to avoid being subject to the 3:12 rules.

[13] Annual wealth tax payments accounted for $\approx 1.2\%$ of total annual tax revenues over that period.

of power. A coalition of centre-right parties took office and decided to abolish the wealth tax with immediate effect in January 2007.[14] We use this large and unanticipated decline in wealth taxation to estimate elasticities of migration with respect to the wealth tax rate. We discuss the validity of this reform for identification in section 5.

2.2 Wealth Taxation in Denmark

Sweden's experience with wealth taxation is not an exception in Europe. To provide an out-of-sample validation of our results for Sweden, we complement them with a similar analysis for Denmark. As small open economies in the European Union and with high levels of redistribution, Sweden and Denmark share many features. Like Sweden, Denmark used to levy progressive wealth taxes and eventually abolished them. We compare the Swedish and Danish wealth tax systems in Appendix Table II.1.[15] A key difference between the two systems is that business assets were fully included in the Danish wealth tax base. Hence, studying the Danish context is useful for evaluating if our estimates are portable to a setting where assets in closely-held businesses are taxed. We take advantage of two major reforms of the Danish wealth tax: a reduction in 1989 and the abolishment in 1996. These reforms allow us to estimate migration elasticities in a different wealth tax system and using different identifying variation, offering a check on the internal and external validity of our main results based on Sweden.

3 Data

Our analysis relies on exhaustive administrative data on wealth, income, firm ownership structure, and migration. Our data infrastructure is rare because it covers, for the universe of Swedish households, all assets (and not just taxable wealth) without censoring or top-coding, and enables us to link households to all the domestic business assets they control, including assets held in Swedish non-public firms. Furthermore, the information can be matched to precise records of the universe of migration events in and out of Sweden.

3.1 Wealth, Income, and Migration Registers

The data on wealth comes from the wealth tax register (*Förmögenhetsregistret*), which covers the asset portfolios of the universe of Swedish individuals. This register includes detailed third-party

[14] The main argument used by the coalition to justify the wealth tax abolition pertained to its purported effects on migration among wealthy entrepreneurs. For example, Bengt Westerberg, the leader of the Swedish Liberal Party who spearheaded the wealth tax abolition said during the legislative debate: *"The wealth tax rate must be so low that successful entrepreneurs are not forced to move from Sweden due to taxation. The owners of all the companies that've grown large during the post-war period—IKEA, Tetra Pak, Hennes & Mauritz—have all moved abroad."*

[15] Further details on the Danish wealth tax are provided in Appendix I.2. We refer to Jakobsen et al. (2020) for an empirical analysis of the Danish wealth tax, focusing on effects along the intensive margin.

reported information on the stock of all financial assets and real estate assets as of December in each year, between 1993 and 2007. For financial assets, we have information on all savings categorized by asset class, including bank accounts, bonds, stocks, mutual funds, private retirement accounts, and more. The dataset also contains information on the total outstanding debt including mortgage debt, consumer credit and student debt. For real estate, we observe all asset holdings at market value, as used for the property tax assessment. We also incorporate comprehensive information on financial asset transactions and real estate transactions using financial and housing registers from 1999 to 2007.[16]

We link the wealth tax register with the longitudinal dataset LISA, which merges several administrative and tax registers for the universe of Swedish individuals aged 16 and above. In addition to rich socio-demographic information (such as age, occupation, and education), LISA contains exhaustive information on labor income, capital income, taxes, and transfers on an annual basis for the period 1990 to 2017.[17] We merge this data with matched employer-employee registers (RAMS) between 1985-2017, which provide information on the universe of individual employment contracts in Swedish firm establishments. Finally, we complement the data with information on individuals' cognitive and non-cognitive abilities as measured by army enlistment tests.

We note that, following the repeal of the wealth tax, the asset reporting requirements changed. As a result, we do not observe the same comprehensive components of household wealth after 2007. Certain assets such as liquid bank accounts and listed stocks are no longer reported, but we still observe many wealth components such as real estate (through real estate registers) and closely-held business assets (as we explain below). To construct a consistent measure of wealth before and after 2007, we build a prediction model of household total net wealth that we train on data from before 2007, using it to predict net wealth after 2007. The model exploits two important features of the data. First, we continue to observe many elements of household wealth after 2007. Second, for the elements we no longer observe, we have precise information on the past value of all assets and on all income flows, both of which are related to the current value of assets through iterating the law of motion of household wealth. We describe the model in detail in Appendix II.1. Our prediction model performs exceptionally well as shown in Appendix Figure II.1, much better than capitalization methods often used to proxy wealth in the absence of proper administrative data on wealth (Saez and Zucman, 2016).

Firm Ownership Registers: We complete our measure of wealth with information on business assets held by Swedish residents.

Shares of publicly traded companies directly owned by individuals are observable in the financial

[16] For a detailed presentation of the information available in Swedish wealth registers, see Kolsrud et al. (2020).
[17] Additional information on this dataset can be found in Kolsrud et al. (2018).

register (KURU). For unlisted firms, we have access to the administrative register of closely-held businesses in Sweden, covering the period from 2000 to 2017. This register builds on the K10 tax form. It was established to monitor dual income tax avoidance and ensure that wages are not reclassified as dividends for tax minimization purposes. The K10 tax form is filed annually for each company when an individual either owns the firm or works in the firm *and* at least 50% of the ownership is shared by at most four individuals.[18] This register thus records the number of shares held by Swedish residents actively participating in the firm, along with the tax identifier of that firm. We also measure the dividends distributed to individuals linked to these shares, in addition to tracking any profits or losses associated with the sale of these shares on an annual basis.

A limitation of K10 register is that it only links individuals to the firms they own directly. This is an issue because the largest closely-held businesses may be held through holding companies and other complex ownership vehicles. To overcome this challenge, we use an additional dataset that records the ownership structure of all private companies in Sweden based on information sent to the Swedish Companies Registration Office. *Serrano* tracks all ownership links between Swedish firms and provides detailed consolidated and unconsolidated financial statements for subsidiaries and parent companies. We develop an algorithm to map the entire network of ownership links among Swedish private companies.[19] We then calculate integrated ownership shares for every company in the country. We match the ownership links and corresponding integrated ownership shares to our K10 tax files on closely-held firms. This enables us to identify all closely-held firms that are part of a group, either as a parent or subsidiary company. This gives us a complete mapping between individuals and all the firms that they control both directly and indirectly in Sweden. [20]

Each listed and unlisted company that appears in our individual-level business asset dataset can then be matched to firm-level financial data collected by Statistics Sweden. This dataset contains rich balance-sheet information such as value added, wage bills, investments, and assets. Note that we use this rich information to improve the accuracy of our measure of the market value of privately-held business assets. As business assets are untaxed, the direct information available in wealth registers on the value of these assets is of relatively limited quality. Instead, we can use information on profits and sales, and rely on standard valuation techniques used for private equity. In practice, we follow closely the approach of Smith et al. (2022). First, we use valuation multiples from similar businesses for profits, sales, and assets, and we apply a 10% liquidity discount. Second, for smaller businesses with less than $50M in profits, we account for the human capital

[18] Members of the same family are considered one person for the K10 reporting requirements. Hence, one cannot avoid reporting ownership on the K10 by giving away shares to a spouse, children, or grandchildren.

[19] We detail our matching algorithm in Appendix II.4.

[20] Alstadsæter et al. (2016) have previously used a similar approach using linked individual and firm data from Norway to measure top income shares.

contribution of profits estimated in Smith et al. (2019).

Figure 1 shows the importance of measuring entrepreneurship among wealthy taxpayers. We focus on closely-held businesses (firms over which the individual has direct control, defined as having more than 20% of voting rights) and LLCs (limited liability companies) operating in Sweden. We exclude ownership of foreign companies. Panel A describes direct ownership of companies by level of net worth and type of firm. The fraction of business owners increases strongly at the top of the wealth distribution: while less than 3% are business owners in the bottom half, more than 10% are business owners in the top decile. The fraction of business owners increases to 19% in the top 2% and 37% in the top 0.1%. Business ownership is therefore a key characteristic of wealthy households.

Not only are wealthy individuals much more likely to own a business, their contribution to aggregate business activity is quite granular. Panel B illustrates this fact by showing the contribution of firms controlled by wealthy taxpayers to the aggregate level of employment in the Swedish economy.

A significant fraction of Swedish employment is concentrated in firms privately held by the wealthy. Firms owned by individuals in the top 2% of the wealth distribution account for 9.2% of total Swedish employment. Considering the extreme tail of the wealth distribution, we see that individuals in the top 0.1% control more than 3% of total Swedish employment through the businesses they privately own. Panel B highlights another important insight: measuring the indirect ownership of firms through holdings is crucial to fully understand the aggregate employment impact of wealthy taxpayers. Without our unique data on ownership links in Sweden, we would underestimate the economic activity linked to the firms held by the wealthy by a factor of three. The reason is that about a third of unlisted firms owned by the wealthy act as parent companies for at least one subsidiary in the Swedish economy.

Migration Register: Migration registers enable us to measure precisely when migration events occur and for how long. Upon arrival in Sweden, any taxpayer is required to request a national identification number. Similarly, if taxpayers want to stop paying taxes in Sweden, they need to report their move to the local tax authorities. Furthermore, Swedish citizens leaving the country for 12 months or longer must annually report the number of days spent in Sweden for population registry purposes and to determine tax residency. This means that we can investigate migration decisions both at the extensive and the intensive margin, by using our information on the time spent in Sweden each year. In our baseline specification, an out-migration event is defined as an individual starting the year in Sweden and ending the year outside Sweden.[21] Conversely, an in-

[21] Note that people can move multiple times during the same year, but our definition of migration events naturally aggregates all moves at the annual level.

migration event is defined as individual starting the year outside Sweden and ending it in Sweden. In Appendix Figure III.1, we show the probability to remain outside Sweden following an out-migration event at different levels of wealth. Mobility is quite persistent over time. One year after out-migration, the probability to remain outside Sweden is around 90% for both wealthy and non-wealthy individuals. Five years (ten years) after out-migration, the probability to remain outside Sweden is around 60% (50%) among wealthy individuals. Hence, migration is an absorbing state for many individuals, although return migration is fairly common too.

Wealth and Migration Data in Denmark: The administrative data on wealth and migration for Denmark, used in our validation exercise, share many attractive features with the Swedish data described above. We provide details of this data in Appendix II.3 and summarize in Appendix Table II.1 the main differences between the Swedish and Danish data.[22]

3.2 Migration Patterns Among the Wealthy

We start by providing descriptive statistics on migration flows. The first important fact, shown in Figure 2, is that migration rates at the top of the wealth distribution are small. The figure shows out-migration rates (left column), in-migration rates (middle column), and net migration rates (right column) by level of net wealth in Sweden (top row) and Denmark (bottom row). For out-migration, we rank individuals by their net wealth in year t and compute the fraction of individuals who out-migrate in year $t+1$ for each wealth fractile. Similarly, for in-migration, we rank individuals by net wealth in year t and compute the fraction who in-migrated in year $t-1$. We focus on the period where the wealth tax was still in place, 1999-2006 for Sweden and 1989-1996 for Denmark.

Out-migration flows are smaller at the top of the wealth distribution than at the bottom. In Sweden, about .2% of individuals in the top decile leave the country each year, compared to about .65% in the bottom half.[23] However, the out-migration rate starts increasing at the very top of the wealth distribution, among households located above the exemption threshold for the wealth tax. In the top .1% of the distribution, the fraction of individuals out-migrating is about twice as large as it is just below the exemption threshold. Nevertheless, the outflows remain quite small in magnitude. This finding is robust to re-weighting outflows by the level of wealth: The taxable wealth of out-migrants subject to the wealth tax represents only 0.09% of total taxable wealth in Sweden.

While the public debate tends to focus on the departure of wealthy people, it is equally important to consider arrivals. As shown in the figure, the in-migration patterns are similar to the out-migration

[22] The main advantage of the Danish data is that the tax administration continued to gather the same information on taxable wealth after the repeal of the wealth tax. This enables us to identify the elasticity of in-migration with respect to the net-of-tax rate on wealth in section 5. A downside of the Danish data is that they do not include the rich information on closely-held businesses available for Sweden, preventing us from tracking the effect of migration on the economic outcomes of firms owned by wealthy migrants in Denmark.

[23] In Appendix Figure III.3, we show out-migration rates for each decile of the distribution separately.

patterns. Inflows are small on average, but increase somewhat at the very top of the wealth distribution. Putting inflows and outflows together, we observe very small, positive net migration rates across the entire wealth distribution. The rate is about 0.05% at the top of the wealth distribution. That is, Sweden experienced small migration *gains* at the top during the years preceding the abolition of the wealth tax.[24]

We find that the magnitude and variation of out-migration and in-migration flows are extremely similar in Denmark, a country where business assets were fully included in the wealth tax base. The out-migration rate is about 0.1% in the top decile and starts increasing as we move into the extreme tail: about 0.4% of individuals in the top 0.1% of the wealth distribution out-migrate from Denmark every year. The net migration rate is marginally positive, except for the top 0.1% where it turns marginally negative.

In Appendix section III, we provide additional insights on the migration patterns of the wealthy. We show that low-tax countries and countries with beneficial tax treaties are over-represented among the destinations chosen by the wealthy. We also provide an in-depth analysis of selection into out- and in-migration. We find that the lower migration rates at the top of the wealth distribution are partially explained by the fact that wealthy taxpayers are older and have characteristics that correlate negatively with migration.

4 Empirical Roadmap

We are interested in the economic consequences of tax-induced migration by the wealthy. In this section, we propose a transparent two-step procedure that facilitates a mapping between migration responses to wealth taxes and their aggregate economic implications.

4.1 A Two-Step Procedure

To motivate our approach, it is useful to explain why we cannot use the repeal of the Swedish wealth tax described in section 2 to estimate the effects on aggregate economic outcomes. Estimating aggregate effects of migration responses to wealth tax reform (and to tax reform more broadly) poses several major challenges. First, wealth tax reforms—especially national reforms like the Swedish one—do not provide credible quasi-experimental variation for studying general equilibrium effects as they are present in both treatment and control groups. Second, even if we could use wealth tax reforms to estimate aggregate effects, say using cross-country variation, the estimates would not capture the effects coming from migration specifically. The exclusion restriction would be violated because wealth taxes affect the economy through channels other than

[24] See Appendix Figure III.4 for year by year out-migration and in-migration figures.

migration (including savings, capital accumulation, and avoidance/evasion at the *intensive* margin). Third, wealth tax reforms are underpowered for studying economy-wide effects: even large tax variation like the repeal of the Swedish wealth tax, while allowing for well-identified effects on migration *flows*, are likely to be associated with relatively small effects on the *stock* of wealthy individuals, given the low international migration rates already described in Section 3.2. It would be difficult to detect the aggregate economic effects of small changes in the population stock.

These challenges are not unique to our setting. They make it difficult to evaluate aggregate economic effects and trickle-down in any setting, explaining the scarcity of credible evidence on such effects. We propose a simple and transparent two-step procedure to circumvent these issues. Our approach is based on a simple decomposition of the effect of wealth tax-induced migration on any aggregate outcome Y into three terms:

$$(1) \qquad \frac{dY/Y}{d(1-\tau)/(1-\tau)} = \underbrace{\frac{\partial N}{\partial(1-\tau)} \cdot \frac{1-\tau}{N}}_{\text{Migration Elasticity}} \times \underbrace{\frac{\partial Y}{\partial N} \cdot \frac{1}{Y^w}}_{\text{Migration Impact}} \times \underbrace{\frac{NY^w}{Y}}_{\text{Top Wealth Share of Y}}$$

where Y^w is the average outcome (e.g., employment, investment or tax payments) generated by wealthy entrepreneurs and N is the population of wealthy individuals. The first term in the decomposition (the *migration elasticity*), captures the impact of the change in the average wealth tax-rate on the overall stock of wealthy individuals via migration responses. The first step of our methodology, presented in Section 5, consists in estimating this migration elasticity using wealth tax reforms.

The second step consists in estimating the impacts of migration events on economic outcomes, the second term of the formula. Importantly, this approach, presented in Section 6, does not rely directly on any wealth tax experiment. Specifically, using an event study design, we estimate the impacts of out-migration on individual-level, firm-level, and market-level outcomes. This approach is statistically precise because it allows us to pool a large number of individual migration events over a relatively long time period. The event study design also relies on transparent identification assumptions, some of which can be easily tested in the data.

We can then measure the aggregate economic implications of tax-induced migration by the wealthy by combining our estimates of (i) the migration elasticity (step 1), and (ii) the impact of migration on various economic outcomes (step 2). When combining these estimates, we simply need to account for the share that the wealthy represent in the aggregate outcome Y (the third term in decomposition (1)). Looking at a range of key outcomes, our approach allows to shed light on the potential trickle-down effects of taxing high-wealth individuals. We do so in Section 7 of the

paper.

4.2 Heterogeneous Treatment Effects of Migration and LATE Estimation

Our approach combines estimates from two different designs: tax reforms for the migration elasticity and event studies for the impact of migration. To quantify the aggregate economic effects of wealth tax-induced migration events, we need a LATE in the latter design that corresponds to the same target population as the LATE from the former design. Our two-step procedure thus requires that the LATE for migration impacts are based on the same population as the LATE for migration elasticities. That is, we need to identify the effects of migration for the group of individuals m who are *at the margin* of deciding to migrate when the wealth tax is abolished:

$$\frac{\partial Y}{\partial N} \cdot \frac{1}{Y^w} = \underbrace{\frac{\partial Y}{\partial N}\bigg|_m \cdot \frac{1}{Y^m}}_{\text{Migration Impact for Marginals}} \times \underbrace{\frac{Y^m}{Y^w}}_{\text{Selection into Migration}} \tag{2}$$

In the absence of treatment effect heterogeneity, things are straightforward. But in the presence of heterogeneity, event studies based on all migration events may not give the relevant LATE for estimating the economic effects of migration responses to the wealth tax repeal.

To account for this, in section 7.1.1, we measure (i) the average characteristics Y^m of the marginals and compare that to the average characteristics Y^w of the wealthy; and (ii) the average treatment effect of migration $\frac{\partial Y}{\partial N}\big|_m \cdot \frac{1}{Y^m}$ for the marginals. We apply standard methods used in the literature on selection in insurance markets (e.g. Hendren et al., 2021), taking advantage of the fact that we can measure the effects of migration for all events happening before vs after the wealth tax repeal. The difference in estimates before vs after the tax reform is directly informative about the impacts of migration for marginal individuals, who used to migrate before the reform, but stopped migrating because of the abolition of the wealth tax.

5 International Migration Responses to Wealth Taxation

In this section, we exploit large changes in wealth tax rates on wealthy individuals in Sweden and Denmark to estimate their elasticity of migration with respect to wealth taxation.

5.1 Identification and Graphical Evidence

Our main source of identifying variation is the repeal of the wealth tax in Sweden. As described in section 2, the reform led to a sharp, unanticipated, and persistent reduction in wealth taxes for households at the top of the wealth distribution. The magnitude of the Swedish reform makes

it one of the largest (and cleanest) sources of variation available for estimating causal impacts of wealth taxes. The statutory marginal tax rate above the exemption threshold, which had been stable at 1.5% prior to 2007, suddenly dropped to 0%. For individuals at the top end of the wealth distribution, this drop implied a significant and permanent reduction in the taxes paid on their assets. Panel A of Figure 3 shows the evolution of the effective average tax rate on total net wealth for the richest 2% of Swedish households. The abolition of the wealth tax led to a sharp drop in their average wealth tax rate of about 0.5%.

Difference-in-Differences Strategy: We take a difference-in-differences approach, comparing treated individuals at the top of the wealth distribution to untreated individuals further down the wealth distribution. The treated group consists of individuals in the top 2% of the distribution of net wealth. As discussed in section 2.1, this group was always liable to the wealth tax prior to its repeal. As a baseline specification, the control group consists of individuals located between the top 20% and top 10% of the wealth distribution. Two arguments motivate this choice. First, as shown in Appendix Figure I.1, individuals located between the top 10% and top 2% were partially affected by the wealth tax over the period 1999-2007 due to variation in the exemption threshold and the creation of a specific threshold for single individuals. Second, while considering a control group very close to the treatment group has advantages in terms of comparability, we also need to account for potential contamination bias. The reason is that, because individual wealth tend to grow over the lifecycle, households close to the exemption threshold may become liable for wealth taxes in the future. As a consequence, they could react to wealth tax variation in anticipation of future tax liability. Our control group avoids such contamination problems: among individuals in our control group in 1999, less than 1% end up in the top 2% by 2006.

By choosing a control group further down the wealth distribution, one may worry about the validity of the parallel trends assumption. The standard validity check to assuage such concern is to inspect pre-trends in the outcome of interest for treatment and control groups. Before turning to pre-trends, let us briefly comment on the potential identification threats posed by other changes in capital and labor taxation around the time of the 2007 reform that might have affected treatment and control groups differently. As discussed in section 2.1, there were some changes to labor and capital taxation around the time of the wealth tax abolition. To check if these changes differ between our comparison groups, Appendix Figure I.2 plots time series of the effective tax rates on labor and capital income (excluding the wealth tax) on individuals in our treatment and control groups. Panel A shows that the effective capital income tax rate did not drop for either treatments and controls in 2007 and that it evolved similarly over time in the two groups. Similarly, Panel B shows that the effective labor income tax rates evolved similarly in our treatment and control groups over time. Appendix Figure I.3 shows that it is only when wealth tax liability is taken into account that tax

rates on capital evolve differently across treatments and controls. Hence, confounding effects from non-wealth tax changes do not seem to pose concerns in this context.[25]

In Appendix Figure V.1, we start by providing direct difference-in-differences evidence. Specifically, we plot out-migration rates for taxpayers in the treatment group (red series) and the control group (blue series). The red vertical line marks the time of the Swedish wealth tax repeal. Three key insights emerge. First, before the reform, out-migration rates were significantly larger for individuals in the treatment group than for individuals in the control group. Second, while the migration *levels* were different among treatments and controls before the reform, migration *trends* were not. Both groups experienced a decline in out-migration rates during the first few years, followed by an increase in out-migration rates in the final years leading up to the reform. This lends support to parallel trend assumption of our approach. Third, after the repeal of the wealth tax, there is an immediate and large drop in the out-migration rates of individuals subject to the wealth tax, relative to those not subject to the wealth tax. The gap in out-migration rates of the two groups closes fully in just one year, in 2007. The figure provides strong evidence that the wealth tax repeal significantly reduced out-migration rates among wealthy taxpayers exposed to the reform.

Predicting Post-2007 Wealth to Estimate Long-Term Effects: The migration series in Appendix Figure V.1 stop in 2008 because of the break in how the administrative wealth data was collected after the abolition of the wealth tax. To quantify the aggregate economic implications of wealth taxes, it is critical to understand if the documented effects on migration are persistent over time. To do this, we use our measure of predicted wealth from the model described in section 3. Individuals are allocated to the treatment group if their predicted wealth belongs to the top 2%, and to the control group if their predicted wealth falls between the top 20% and top 10% of the distribution. Assigning treatment status according to predicted wealth (based on pre-reform variables) rather than actual wealth has an important empirical advantage: we avoid assigning treatment status based on a wealth variable that is endogenous to the current wealth tax level.[26]

We regress the probability of out-migrating Y_{it} on year fixed effects, a treatment group indicator, and the interaction between year and treatment group.[27] Specifically, we consider the following linear probability model:

[25] It is interesting to note again that the abolition of the inheritance tax does not appear to have had any significant effect on the effective tax rate on wealth for our two groups. As we explained in section 2.1, this is because the inheritance tax had remarkably little bite in Sweden before its abolition.

[26] In Appendix V.1, we provide more discussion about our strategy and illustrate how it works with a simple prediction model where we use initial level of wealth in 1996-1998 as sole predictor of future treatment status (Appendix Figure V.2).

[27] We cluster standard errors at the individual level.

$$Y_{it} = \alpha + \sum_j \beta_j \cdot \mathbb{1}(t = j) \cdot \mathbb{1}(T_{it} = 1) + \gamma_t + \delta \cdot \mathbb{1}(T_{it} = 1) + \nu_{it} \tag{3}$$

Figure 3 panel B plots the estimated coefficients β_j and their confidence intervals. The reform is associated with a large and permanent decrease in the probability of out-migrating for treated taxpayers. We find no evidence of significant pre-trends, confirming that the migration patterns of wealthy individuals just below the exemption threshold form a credible counterfactual for the migration patterns of the wealthy individuals above the threshold.[28] Our estimates indicate that, one year after the reform, the probability to out-migrate decreased by 0.05 percentage points among the wealthy. This represents a 30% reduction in the propensity to leave Sweden, relative to the pre-reform baseline. Two important insights emerge from these results: the effect of wealth taxes on out-migration is small in magnitude, but the wealth tax accounted for a substantial fraction of wealthy out-migration flows before the reform. About one-third of expatriation events among the top 2% wealthiest households were caused by the wealth tax before 2007.

5.2 Estimating Elasticities of Migration Flows to the Wealth Tax

Having shown visual evidence of international migration responses to the Swedish wealth tax reform, we proceed with estimating migration elasticities with respect to the wealth tax rate, the policy-relevant parameter in our context. For this, we relate the difference in out-migration changes for treatments and controls to the reform-induced change in the effective wealth tax rate. Specifically, we consider a 2SLS regression of the following form:

$$Y_{kt} = \alpha_0 + \varepsilon \cdot \ln(1 - \tau_{kt}) + \beta_1 \cdot \mathbb{1}(k = T) + \beta_2 \cdot \mathbb{1}(t \geq t_0) + \nu_{kt} \tag{4}$$

where Y_{kt} is the out-migration rate of group $k = T, C$ in year t, and t_0 is the year of the reform. τ_{kt} is the mean of the effective average net-of-tax rate for all individuals in group k. The log net-of-tax rate is instrumented using the dummy interaction $\mathbb{1}(k = T) \times \mathbb{1}(t \geq t_0)$. The parameter of interest is ε, a semi-elasticity of the out-migration rate with respect to the average net-of-tax rate on wealth. Because τ_{it} is a small number in our context (as in any typical wealth tax context), absolute and percentage changes in the net-of-tax rate are almost identical. Hence, ε can also be interpreted as the effect of increasing $1 - \tau$ (reducing τ) by 1 percentage point.

[28] A specific concern discussed in section 2 is that changes in inheritance taxation in 2004 could affect the very wealthy more than the moderately wealthy. However, the absence of pre-trends between 2004-2006 speaks against such concerns. This is consistent with the fact that the tax law limits the ability of individuals to avoid inheritance taxation through international migration via strict rules on tax residency definition at death.

The estimates are presented in Panel A of Figure 4. The semi-elasticity ε equals -0.17 in the full population of wealthy taxpayers in Sweden. This implies that a 1 percentage point increase in the average wealth tax rate increases the out-migration rate of wealthy taxpayers by 0.17 percentage points. The semi-elasticity does not vary much by age or education. We also investigate if the elasticity is different for the subpopulation of wealthy entrepreneurs. Since business assets were exempt from the wealth tax in Sweden, it is natural to first ask whether business owners were affected by the repeal of the wealth tax, and to what extent. Appendix Figure V.5 confirms that the 2007 repeal of the Swedish wealth tax led to a sharp drop in the effective tax rate on wealthy entrepreneurs in the treated group, and that it significantly reduced their likelihood of leaving Sweden.[29] As reported in Panel A of Figure 4, the implied semi-elasticity of out-migration among entrepreneurs are larger (but less precisely estimated) than in the general population of wealthy taxpayers.

In Appendix Figure V.3, we investigate the robustness of our estimates to the choice of control group. As explained above, the baseline specification is based on using individuals located between the top 20% and top 10% of the wealth distribution as a control group. Specifying a control group that is not immediately contiguous to the treatment group was done to avoid contamination bias, but it implies that the common-trend assumption is less likely to hold. To assuage such concerns, we have shown that there is no evidence of differential pre-trends. Reassuringly, we also find here that the estimated semi-elasticities are very similar if we choose control groups located closer to the treatment group.

5.3 Out-of-Sample Validation: Migration Responses in Denmark

As with any difference-in-differences strategy, and despite the evidence pointing to the absence of unobserved shocks simultaneous to the Swedish reform, one may still raise doubts about the internal validity of our estimates. Ultimately, the most compelling way to test for both the internal and external validity of our estimates is to reproduce a similar analysis in a different context.

For this purpose, we take advantage of two major wealth tax reforms in Denmark, offering an opportunity to cross-validate our migration elasticity estimates out-of-sample. In 1989, the marginal tax rate on wealth above an exemption threshold was reduced from 2.2% to 1%. The tax cut was phased in over three years, as shown in Appendix Figure V.6, Panels A-B. In 1997, the Danish

[29] In Panel A of Appendix Figure V.5, we plot the evolution of effective tax rates on wealth for the sub-sample of entrepreneurs in both the treated and control groups. The repeal of the wealth tax in 2007 led to a sharp reduction in effective tax rates for Swedish entrepreneurs previously subject to the wealth tax, compared to those in the control group. The drop in effective tax rates is smaller than that observed for the general population of wealthy taxpayers (as shown in Panel A of Figure 3), because entrepreneurs tend to hold a large proportion of their wealth in business assets, which were not subject to the wealth tax. In Panel B, we show that the propensity of wealthy entrepreneurs to leave Sweden decreased significantly—by 0.14 percentage points— within one year of the reform, a reduction of roughly 50%.

wealth tax was abolished entirely. These two reforms provide alternative identifying variation for studying migration responses to the wealth tax.

To estimate migration responses in Denmark, we use a similar identification strategy as for Sweden: we compare out-migration rates for taxpayers above and below the exemption threshold over time. In Denmark, the exemption threshold for the wealth tax was located higher in the distribution than in Sweden and the threshold was more stable over time. We assign individuals to the treatment group if they belong to the top 1% of the distribution, using individuals located between the top 5% and top 2.5% to form a control group.[30] Using specification (4), we provide estimates of the semi-elasticity of out-migration with respect to the net-of-tax rate on wealth—among all top wealth holders and in subsamples—in Panel A of Figure 4. As shown, the elasticity estimates are very similar in Denmark and Sweden, with the confidence intervals on the Danish estimates falling within the confidence intervals on the Swedish estimates. Interestingly, this is also true for the subpopulation of wealthy entrepreneurs: the semi-elasticity of migration for entrepreneurs appears similar in both countries despite important differences in the tax regime of business assets. To conclude, the estimates of out-migration elasticities with respect to wealth taxation are very similar in Denmark and Sweden, despite being based on different populations of wealth taxpayers, different time periods, and different wealth tax systems.

5.4 In-Migration Responses

Our empirical analysis so far has focused on estimating out-migration responses. However, the total effect of wealth taxes depends on their effect on *net* migration rates, out-migration minus in-migration. To estimate in-migration responses, we proceed in two steps.

First, we focus on return migration of wealthy Swedish citizens. We follow a simple strategy, which consists in taking all individuals in Sweden in 1999 and defining two groups based on their initial net wealth level: the "treatment" group are individuals in the top 2% of the distribution, and the "control" group is composed of individuals in the top 20% to top 10% of the distribution. Then, for all following years, we measure for each group a return probability which corresponds to the probability to observe a return migration conditional on having been out of Sweden. We compare the evolution of the return probability of both groups before and after the abolition of the wealth tax using the same difference-in-differences specification (3) used above. The estimated coefficients are plotted in Appendix Figure V.4 and show a significant but small positive effect of the reform on return migration, building up over time. While these results demonstrate that the in-migration margin does also respond to wealth tax rates, the approach fails to capture the full

[30] Appendix Figure V.6 depicts the out-migration rate for treatments (red series) and controls (blue series) between 1980-2006. The visual evidence suggests that the Danish wealth tax cuts, just like the Swedish ones, reduced out-migration at the top of the wealth distribution.

extent of these responses as it can only identify return migration from individuals whose wealth level has been previously observed in Sweden. To measure all in-migration responses, one would need to consistently observe wealth upon arrival both before and after the abolition of the wealth tax. This cannot be done given the structure of the Swedish data.

This is why in a second step, we turn our focus to Denmark, where the data on wealth remained consistent before and after the two large wealth tax reforms mentioned above. This allows us to measure in-migration by wealth level upon arrival, and replicate a difference-in-differences identification of in-migration elasticities similar to the one carried in section 5.3. Estimates are displayed in Panel B of Figure 4, and confirm the evidence from return migration in Sweden. We find that in-migration rates of the wealthy respond significantly to wealth tax variation, although the magnitude is about two to three times smaller than for out-migration. On average, our results suggest that a one percentage point increase in the effective tax rate on wealth decreases the in-migration rate by about 0.05 percentage points. We find limited evidence of heterogeneity across groups, although we arguably have limited power to conduct a thorough heterogeneity analysis.

We can finally put together out-migration and in-migration estimates, to measure the total effect of wealth tax rates on the net migration flows of the wealthy. Combining in- and out-migration semi-elasticities, we find that a one percentage point increase in the effective tax rate on wealth decreases net flow rates by 0.22 percentage points. This is a large effect with respect to actual flow rates: this suggests that a large fraction of migration flows among the very wealthy were motivated by tax reasons. But this is a small flow effect with respect to the overall size of the wealthy population.

5.5 Interpreting the Magnitude: Stock Elasticity

Although our estimated effects on migration flows are modest, these flow effects cumulate over time. This begs the question: how should we cumulate the flow effects to properly measure the stock effect? In Appendix VI, we show that the effect on the steady-state stock of wealthy individuals depends on the magnitudes of flow effects and the natural rate at which the stock of wealthy individuals regenerates itself through births and deaths and through the creation, destruction, and transmission of wealth. We explain the way these forces play out in the context of a simple OLG framework, deriving formulas for quantifying the stock elasticity. These formulas depend solely on our estimated semi-elasticities of net-migration flow rates and on moments that can be observed in the data.

Our preferred approach relies on the following simplified formula for the elasticity of population of wealthy individuals N with respect to the net-of-tax rate on wealth:

$$\frac{dN/N}{d(1-\tau)/(1-\tau)} \approx -\varepsilon \cdot \frac{(T+1)}{2} \tag{5}$$

This formula requires only our estimate of the average semi-elasticity of net migration flows ε (accounting for both out- and in-migration) and a measure of the average "lifespan" of wealthy individuals T. The lifespan T captures the speed at which the population of wealthy people re-generates itself in the absence of migration. In steady state, the longer is individuals' lifespan in the wealthy population, the lower is the birth rate of individuals into the population of wealthy individuals, i.e. the lower is the rate at which the population of wealthy individuals regenerates itself.[31]

Formula (5) has a simple interpretation: to get an estimate of the effect on the population stock N, we simply cumulate the flow effect ε for the half-life that individuals spend in the wealthy population. The larger the average lifespan T, the larger the effect on the stock. This is because a larger T implies a lower regeneration rate of the wealthy population absent migration. So when we lose wealthy individuals to migration, it is harder to replace them.

Based on our estimates, the elasticity of the stock of wealthy individuals equals

$$\frac{dN/N}{d(1-\tau)/(1-\tau)} = 1.77 \quad (0.47)$$

Hence, even when accounting for the fact that flow effects cumulate over time, the stock effect is modest: a 1% increase in the net-of-tax rate on wealth increases the stock of wealthy people by less than 2% in steady state. The main reason is that, because migration flow rates are small among the wealthy, even sizable effects on migration flows translate into a limited effect on the stock of people.

Sensitivity: In panel A of Figure 5, we explore the sensitivity of our estimates. We show that our conclusions are very robust to our assumptions on replacement rates of the wealthy population. They are also very robust to the presence of dynastic effects: while we can precisely detect that migration decisions of wealthy parents affect the location decisions of their heirs (Appendix Figure VI.1), these dynastic effects are very small and do not affect our baseline estimates of the impact of wealth taxes on the steady-state size of the wealthy population.[32]

Finally, we show that our results are robust to various assumptions regarding the extent of tax evasion happening at the top end of the wealth distribution in Scandinavia at the time. Offshoring wealth in tax havens has been shown to be a significant driver of tax evasion by the very wealthy (e.g., Boas et al., 2024). In the presence of tax evasion, the actual net wealth of top taxpayers is underestimated in the administrative data by a factor $(1-e)$, where e is the fraction of wealth that

[31] In a steady state, the birth rate of individuals into the population of wealthy individuals is simply the inverse of the lifespan of wealthy individuals T, i.e. $B = 1/T$.

[32] All details regarding our sensitivity analysis can be found in Appendix VI.

is evaded. [33] And as a result, our measure of tax rates τ is an overestimate of their effective tax rates on wealth $\tilde{\tau} = (1-e)\tau$.[34] A corollary is that our estimated elasticities may overestimate the true elasticity with respect to the effective net-of-tax rate $(1-\tilde{\tau})$:

$$\frac{dN/N}{d(1-\tilde{\tau})/(1-\tilde{\tau})} = \frac{dN/N}{d(1-\tau)/(1-\tau)} \cdot \frac{1}{(1-e)} \tag{6}$$

We can nevertheless easily explore the sensitivity of our estimates to the extent of tax evasion by using direct estimates of the fraction of wealth evaded by top wealth groups in Sweden from Alstadsæter et al. (2019) and Boas et al. (2024). In their paper, they provide an upper bound and a lower bound on the fraction of wealth e evaded by each top fractile of wealth.[35] Using these estimates, we compute a lower bound and an upper bound on the total fraction of wealth evaded by the top 2% of wealthy taxpayers, and provide in panel A of Figure 5 two bounds for our estimates of the elasticity of the stock of the population of the wealthy accounting for tax evasion. The upper bound elasticity is 1.92 and the lower bound elasticity is 1.85, indicating that accounting for the presence of tax evasion does not affect the fundamental qualitative message of our baseline results, namely that the impact of wealth taxes on the size of the population of the wealthy is small.

Comparison to Migration Elasticities in the Literature: To compare our results to the literature, we convert our estimates into an elasticity of the population stock with respect to the net-of-tax rate on *income* (rather than to the net-of-tax rate on wealth). Results are displayed in panel B of Figure 5. Our implied migration elasticity is equal to 0.05, and it accords in magnitude to cross-border migration elasticities of top incomes.[36] We also note that our elasticity is substantially smaller than estimates of intra-national mobility elasticities to capital taxation.

6 The Economic Effects of Wealthy Out-Migration Events

The previous section showed that wealth taxes affect the migration decisions of the wealthy. But does the departure of wealthy individuals create significant negative trickle-down effects? In this section, we estimate the impacts of migration events on individual-level, firm-level, and market-level outcomes.

[33] We could also underestimate the net wealth of top taxpayers if we under-value their private business assets or by omitting their minority stakes in Swedish companies.

[34] Relatedly, we could overestimate effective tax rates on wealth if we miss some ownership links in the firm ownership registry.

[35] We use Table J3 Sweden of their online appendix for the upper bound scenario, Table J3.B Sweden for their average scenario, and Table J3.C Sweden for their lower bound scenario.

[36] All details on these computations are available in Appendix VI.3. These elasticities are typically found to be quite small, around 0.1, except when focusing on specific subsegments of the labor force such as foreign nationals and expatriates.

6.1 Event Study Design

Our focus is on first-time out-migration events for individuals in the top 2% of the Swedish net wealth distribution.[37] We restrict attention to migration events occurring between 2000-2007. Event time t is indexed relative to the year of out-migration such that $t = 0$ is the year in which a given individual leaves Sweden. A control group is created from wealthy individuals who never leave Sweden, randomly assigning placebo migration dates to those individuals. We do not match on pre-event characteristics to be able to detect potential self-selection into migration based on pre-migration dynamics in individual or firm outcomes. The event study regression is specified as follows:

$$Y_{it} = \sum_j \beta_j \cdot \underbrace{M_i \cdot D_{j=t}}_{\text{Migrant x event time}} + \gamma \cdot M_i + \sum_j \delta_j \cdot D_{j=t} + \nu_{it} \tag{7}$$

where Y_{it} is an outcome for individual i (or their firm) in event year t, M_i is a dummy equal to one if the individual belongs to the migrant group, and $D_{j=t}$ is an event time dummy for time t. In the interaction term, we omit the dummy for a base year prior to migration. The coefficient β_t captures the impact of out-migration in event year t. It is estimated as a difference-in-differences comparing migrants and non-migrants between year t and the base year. This type of estimation strategy has been used to study the effects of managers' death or retirement on firm and co-worker performances (e.g., Smith et al., 2019; Jäger and Heining, 2022), except that we select our control group randomly instead of using matching.

We note that our estimation strategy does not rely on any random sampling assumption. Through the inclusion of a control group of never-treated units and the absence of matching between treated and control units based on pre-event characteristics, we allow identification of the full dynamics of treatment effects, including anticipatory effects. In other words, we do not need to assume that migration events are randomly allocated in the population. We simply require variation in the timing of events across individuals or firms.

6.2 Individual-level Effects

We first consider individual-level outcomes. To account for zeroes, we specify all outcomes in levels (rather than logs).[38] To get a clear sense of the magnitudes in relative terms, we also report estimates scaled by the average value of the outcome variable for the treated group at baseline. Our

[37] To be precise, we focus on all taxpayers who have been at least once in the top 2% of the net wealth distribution in the years that precede migration.

[38] To deal with outliers, we winsorize the top 5% and bottom 1% of these outcomes.

estimation sample includes 3,517 out-migration events for wealthy taxpayers and 255,888 placebo events.

Tax Payments: Figure 6, Panel A, presents event studies of the effects of out-migration on tax payments. We find no evidence of differential trends between migrants and non-migrants prior to out-migration. This suggests that migration events are not driven by previous increases in tax burdens. After out-migration, tax payments drop sharply for migrants relative to non-migrants. Given that event year 0—the year in which migration happens—is only partially treated, it is more informative to focus on event year 1. At this time, the impact of out-migration on total tax payments equals about -150,000 Swedish Kronor, corresponding to a drop of 66%. Breaking down the effect by type of tax payment, we find a reduction of 59% in wealth tax payments and 68% in income tax payments. Because income taxes represent almost 90% of the tax payments made by the wealthy, this implies that migration induced by wealth taxes will have significant fiscal externalities on income tax collection.

While the short-term impact of migration on tax payments is very large, the medium-term impact (i.e., 5 years after migration) is smaller. The medium-term reduction in total tax payments is about 40%. This reflects that a sizable fraction of wealthy out-migrants eventually return to Sweden. As we documented in Appendix Figure III.1, around 40% of wealthy out-migrants have returned after 5 years.[39]

Portfolio Reallocation: We then turn to the effects of migration on portfolio composition, leveraging our detailed data on asset composition and asset transactions. We first consider taxable wealth in Panel B. Out-migration is associated with a large and permanent drop in taxable wealth reported in Sweden. Wealthy out-migrants reduce their taxable wealth reported in Sweden by 94% one year after their migration event. We then focus on real estate wealth. As shown in Panel C, when wealthy taxpayers leave Sweden, they tend to sell their real estate property in the year of leaving the country. This confirms that out-migration events at the top of the wealth distribution reflect real responses rather than artificial changes in tax residence. We then look at financial wealth, excluding business assets. In Figure IV.4, we find a significant decline in the probability to report any positive financial wealth in Sweden, although the magnitude of the effect (-21%) is still far from 100%. Hence, a large fraction of wealthy out-migrants continue to hold financial assets in Sweden after departing. We also find a significant decline in financial wealth at the intensive margin. Conditional on reporting positive financial wealth in Sweden, wealthy out-migrants decrease their financial wealth in Sweden by 15% in the long run. We use our detailed data on asset

[39] In Appendix Figure IV.2, we estimate a median regression model. These event studies exhibit a clear "wheelbarrow" shape with no convergence over time, which confirms that the longer-run dynamics are driven by the extensive margin of return-migration.

transactions to verify that the decline in financial wealth in Sweden corresponds to active sales of financial assets held in Sweden: Panel D of Figure 6 shows a significant jump at $t = 0$ in the probability of selling more than 10% of one's portfolio.

Figure 6 reveals that out-migration is associated with lower housing and financial wealth in Sweden, confirming that the wealthy actively reallocate part of their assets when leaving the country. One characteristic of top wealth individuals is that they also own companies. The threat that the migration of wealthy entrepreneurs may have large economic spillovers due to reallocation of business assets looms large in the public debate about wealth taxation. We now turn to documenting how the business assets controlled by wealthy individuals are affected when they migrate out of Sweden.

6.3 Firm-Level Effects

To study the impact of migration on Swedish businesses, we use our rich data on closely-held businesses and ownership structures of Swedish firms described in section 3. This information allows us to link wealthy individuals to the firms they control either directly or indirectly. Table 1 starts by presenting descriptive statistics on firms, broken-down by the wealth level of owners. We focus on firms that are active (that have at least one employee who is not the owner). It reveals that the business assets held by the wealthiest individuals are quite granular, and account for a significant fraction of Swedish economic activity. The average business directly controlled by wealthy taxpayers has about 14 employees excluding the owner, which is almost twice as large as the average closely-held business in Sweden. Despite being larger, unlisted companies held by wealthy owners are not characterized by larger value added per employee than the average unlisted firm in the economy. However, closely-held businesses owned by the wealthy do have higher gross investments, in absolute terms and per employee, than the average firm. Consistent with their larger size, those firms also tend to have more owners than other firms: 56% of active closely-held businesses owned by an individual in the top 2% of the wealth distribution have more than one owner, compared to 46% for the full population of active CHBs.[40]

The last column of Table 1 quantifies the contribution of wealthy taxpayers' firms to the aggregate Swedish economy. Panel B focuses on closely-held businesses directly owned by individuals in the top 2% of net wealth. It shows that they account for 3.6% of total employment, 6.9% of total value added, 6.4% of total investments, and 10.7% of total tax payments.[41] Panel C shows

[40] Appendix Figure IV.10, Panel A, starts by describing unlisted companies' sectoral activity by level of wealth of their owner. Economic activities such as hospitality or construction are less represented in firms held by wealthy owners as opposed to companies held by low-wealth individuals. Overall, most closely-held businesses with wealthy owners operate in the wholesale and retail trade sector as well as in real estate, renting, and business activities.

[41] To compute employment at closely-held businesses, we exclude owners, but we include all employment in Sweden (including self-employed), in our denominator.

that these numbers increase significantly once we account for the businesses held indirectly by wealthy taxpayers through complex ownership structures. The businesses controlled by the top 2% wealthiest households represent 9% of total employment, 15% of total value added, 12% of total investments, and 19% of total tax payments.

Selection of Migration Events: Our approach is to track firm outcomes before and after one of their owners leave Sweden. We focus on first-time out-migration events among firm owners in the top 2% of the net wealth distribution.[42] We restrict attention to migration events occurring between 2001-2007, and start by considering firms directly controlled by the wealthy. When firms have multiple owners, we set the event date as the first out-migration date among all the migrant owners of the firm. We also restrict the analysis to closely-held businesses that are active in the year before the migration event.

We build a control group of firms by randomly assigning placebo migration dates to wealthy owners who never out-migrated from Sweden. In the control group, we keep only firms held by at least one owner who was in the top 2% of the wealth distribution for at least one year before the placebo event date. We also require control-group firms to be active in the year before the placebo event time, consistent with the construction of the treatment group.

Having constructed the treatment and control groups, we balance our firm-year level dataset by setting outcomes to zero in years where a firm is not active.[43] In our final event-study sample, we have 298 events of top 2% wealth holders leaving Sweden.

The results are presented in Figure 7. It shows event studies of firm outcomes based on specification (7), the same specification that we used above to study individual outcomes. Panel A of the figure considers the effect of owner out-migration on the probability of firm survival. We find a sharp decrease in the survival probability of almost 30 percentage points following out-migration.[44] Consistent with the reallocation of other assets shown in Figure IV.4, this suggests that out-migration is associated with a significant reorganization of business assets. It is important to note, however, that we define firm survival as the firm identifier being present in the administrative data. While the disappearance of a firm's identifier may be the consequence of business closure, it may alternatively be the consequence of buy-out by another firm or business restructur-

[42] Similar to the event study analysis above, we define a top 2% owner as an individual who appeared at least once in the top 2% of the net wealth distribution in the years preceding out-migration.

[43] We winsorize the top 5% and (for those not bounded by zero) the bottom 1% of our outcomes. We show the robustness of our results to different strategies for handling outliers in Appendix Table IV.3. The results are stable and consistent across all specifications. We note that when using no winsorization at all, we actually find positive but insignificant effects on value added, investments, and tax payments. These positive effects are driven by one outlier firm that continued to grow strongly after its owner migrated out of Sweden.

[44] Appendix Figure IV.6 shows the corresponding results when using instead the probability of observing the firm closing in a given year.

ing associated with a change in the identifier. The economic implications of business closures are likely very different from the implications of buy-outs and restructuring. We return to this important issue below, and show that the majority of firm "disappearances" are not definitive business closures.

In the other panels of Figure 7, we consider the effects on other firm-level outcomes. We continue to focus on the outcomes of firms directly controlled by wealthy out-migrants, not accounting for potential buy-outs or restructuring associated with changes in firm identifiers. We find large and persistent negative effects on employment, value added, net turnover, investments, and tax payments. For example, the number of employees decreases by about 33% resulting from the owner leaving Sweden. The effects on the other outcomes are similar in magnitude. Interestingly, these negative effects appear almost entirely driven by the extensive margin of firm disappearance documented in Panel A. Appendix Figure IV.5 replicates the event studies when conditioning on firm survival. We find small negative intensive margin effects on employment and small positive (but statistically insignificant) effects on value added and investment.

These average effects could mask significant heterogeneity. For instance, older entrepreneurs may be more likely than younger entrepreneurs to shut down their firms when out-migrating. Similarly, having children may affect owners' incentives to close their firms when leaving the country. In Appendix Figure IV.7, we investigate if the firm-level effects of out-migration vary by owner and firm characteristics. Overall, we find limited heterogeneity in the effects by age and the presence of children. We find some heterogeneity by firm size, as measured by the number of employees. The effects of owner out-migration tend to be smaller when the firm is larger. This is particularly true when focusing on extremely large businesses. Figure IV.7 shows estimates for the top ten moves in terms of the value of business assets controlled by the owner. These are businesses worth several billions of Swedish Kronor. Interestingly, the effect of out-migration by the owners of these large businesses is considerably smaller than the average effect in the data.

6.3.1 In-Migration Events

We have seen that wealthy individuals reorganize their business assets when they migrate out of Sweden, but what happens when they migrate into Sweden? To get a full picture of the economic implications of migration by the wealthy, we now consider the effects of in-migration events. The analysis is done in the same way as the analysis of out-migration, but focusing on firms owned by wealthy entrepreneurs who arrived in Sweden between 1998 and 2006. The estimation is based on specification (7), where the dummy M_i is equal to one if the firm owner has migrated into Sweden. The event study graphs are presented in Appendix Figure IV.9: they feature parallel pre-trends and clear positive effects on firm-level outcomes following in-migration. That is, the effects of in-migration on firms are similar to the effects of out-migration, but with opposite signs. Panel A

of Figure 8 summarizes our results by plotting the effects on different outcomes at event time 5, comparing the positive effects of in-migration (red dots) to the negative effects of out-migration (blue dots).[45] We report the estimated effects in percentage terms, rescaling the coefficient $\hat{\beta}_5$ for in-migration events by the average value of the outcome in the out-migration sample the year prior to out-migration. We find that the effects on the probability of having an active business and the number of employees are similar in magnitude for in- and out-migration (but with opposite signs). The effects on other outcomes (value added, turnover, investments, and tax payments) are quantitatively smaller for in-migration.

6.3.2 Firms Held Indirectly by the Wealthy

The preceding analysis ignores the downstream effects through indirect ownership. This is potentially important because indirect ownership is common among wealthy entrepreneurs, and because the firms they own indirectly tend to be large. As shown in Table 1, when accounting for the subsidiaries held by individuals in the top 2% of the wealth distribution, their average number of employees increases from 14.1 to 22.6. Value added and net turnover increase by about 60%.

To get a more comprehensive picture of the economic implications of migration, we estimate the effects of out-migration events on *all* firms owned by the wealthy, including those held through other companies. We use the same empirical specification, but consider outcomes for all firms (e.g, parent company and subsidiaries) owned by the wealthy. The results are shown in Panel B of Figure 8, depicted by the red dots. We find very similar effects when accounting for the economic activity at firms held indirectly. Five years after the departure of their wealthy owner (whether direct or indirect owner), firms experience a decrease in employment by 19%, a decrease in value added by 33%, a decrease in turnover by 28%, a decrease in investments by 19%, and a decrease in tax payments by 45%. Therefore, the out-migration of wealthy entrepreneurs has sizable effects on business activity in their firms, even in those that they control indirectly.

6.4 Reallocation and Spillovers

6.4.1 Firm Acquisitions

As mentioned, companies that disappear upon owner out-migration may have been bought or merged with existing firms. To examine the importance of such mergers and acquisitions, we use administrative data from the Swedish Companies Registration Office (*Bolagsverket*) on mergers and reasons for firms closures. Among all closure events triggered by the out-migration of a wealthy owner (as showed in Figure 7, Panel A), we find that 45% are subsequently linked to a merger event. The remaining 55% are recorded as liquidations or bankruptcies. For each closure

[45] In Appendix Table IV.1, we provide formal tests of equality between the effects of in-migration and out-migration, i.e. $\beta_5^{in} = -\beta_5^{out}$.

associated with a merger, our data identify the administrative ID of the firm into which the original entity was absorbed.

To account for this reallocation of economic activity across firms within Sweden, we implement a version of our analysis that accounts for buy-outs. Specifically, because we observe the identifiers of firms that absorb closed businesses previously held by wealthy out-migrants, we may consider total economic activity in the old and new firms as our outcome variable throughout the entire period of analysis. We then re-estimate our baseline specification using this updated definition of firm boundaries and outcomes for all firms undergoing a merger, in both the treatment and control groups. We plot the resulting estimates in Panel B of Figure 8. As we can see, accounting for buy-outs reduces the estimated effects of out-migration by approximately 40%, though the estimates remain negative and statistically significant.

6.4.2 Worker-Level Analysis

The preceding estimates accounting for reallocation related to mergers and acquisitions may still overstate the implications of firm closures following out-migration. The reason is that, even without a merger or acquisition, workers in the closed firm may be able to find employment at other firms. If workers find new jobs that pay similar wages shortly after their previous firm closes, the effects of wealthy out-migration on aggregate economic activity would be minimal.

Our data enable us to study such reallocation mechanisms too. Considering the same event study specification as above, we replace the firm-level outcomes with either gross earnings or an unemployment indicator measured for each worker employed at treated and control firms in the year before out-migration (real or placebo). As shown in Figure 9, we find that the out-migration of wealthy entrepreneurs is associated with a decrease in their employees' earnings (Panel A) and a slight increase in their unemployment probability (Panel B). However, the effects are relatively small in magnitude: labor earnings decrease by 4.3% and the unemployment probability increases by 0.6pp following owner migration.

6.4.3 Local and Sectoral Spillovers

A final way to investigate the economic implications of migration is to look at market-level effects. In principle, this type of analysis has the potential to capture all reallocation, general equilibrium, and externality effects. For evaluating how important such market-level effects could be, it is important to note that, on average, firms owned by wealthy out-migrants tend to be small relative to their relevant local or sectoral markets. To see this, we define markets at the level of municipality$\times$sector cells (measuring sector at the one-digit level) and plot the distribution of market shares for businesses owned by wealthy out-migrants. The results are provided in Appendix Figure IV.11 for employment shares (Panel A) and value-added shares (Panel B). The vast

majority of firms controlled by wealthy out-migrants represent less than 1% of employment and value-added in their municipality×sector cell. This suggests that market-level spillovers are likely to be limited. Still, a small fraction of out-migration events involve businesses that are quite granular in their local and sectoral market. In Appendix Figure IV.12, we provide event studies of out-migration by owners of businesses that represent at least 10% of employment in their market cell. We use specification (7) with outcomes measured at the market-cell level. The results suggest that, even for these granular businesses, the market-level effects of out-migration are small and statistically insignificant. This is true for employment and value added (shown in the figure), and for other aggregate outcomes that are not shown. The absence of significant spillovers replicates across alternative definitions of the relevant market (varying the size of geography and sector used to define markets). The reason is that almost all granular businesses remain open or get bought out upon their owner's out-migration, implying limited aggregate effects on economic activity.

Summarizing: Our analysis provides the first systematic evidence on the economic implications of migration decisions by wealthy entrepreneurs, a salient topic in the public debate. We show that out-migration negatively impacts economic activity in the firms controlled by the wealthy, mainly through firm closures. The effects of in-migration are similar, but with the opposite sign. Our data offer the opportunity to go beyond those firm-level effects and explore the reallocation of economic activity within Sweden following the out-migration of wealthy entrepreneurs. We find substantial reallocation: many firms closed by their wealthy owners are absorbed by other companies through mergers and acquisitions, and employees at the closed-down firms experience only small losses in employment and labor earnings. Overall, our analysis indicates that the impact of wealthy out-migration on aggregate economic activity is in part mitigated by reallocation forces in the Swedish economy.

It is useful to compare our findings with those from the nascent literature on the effects of managers and CEOs on firm performance, relying on variation from retirements, family successions, and deaths. This literature finds large negative effects of CEO death or retirement on firm and co-worker outcomes (Smith et al., 2019; Jäger and Heining, 2022). We study a different type of owner-specific event, international migration. While we find that out-migration by wealthy entrepreneurs has negative consequences for the firms they control, the effects are smaller in magnitude than in studies of owner death and retirement. For example, Smith et al. (2019) estimate a 26pp decrease in the probability of firm survival following owner retirement and a 82% decrease in profits per worker (accounting for firms' restructuring). Even after conditioning on firm survival, they estimate a 45% decrease in profits per worker. By contrast, we find almost no effects of owner out-migration after conditioning on firm survival.[46] This suggests that owner migration is much

[46] We note that our analysis excludes "human capital vehicles" such as doctor practices and other LLCs. We focus

less disruptive for firms than owner death or retirement. Owners often retain control of their firms upon migration, or their firms get absorbed by other entities through mergers and acquisitions.

7 Aggregate Implications of Wealth Tax-Induced Migration

In the previous section, we provided evidence on the effects of migration on individual-level, firm-level, and market-level outcomes. To quantify the aggregate economic effects of wealth tax-induced migration and draw policy conclusions, we now combine these estimates with migration elasticities.

7.1 Aggregate External Effects on Employment and Economic Activity

The public debate about wealth taxes focuses on the negative externalities or out-migration by entrepreneurs on employment, investments, and business dynamism. We therefore start by quantifying these "trickle-down" effects. For this, we follow equation (1), and combine (i) the elasticity of the stock of wealthy entrepreneurs with respect to the net-of-tax rate on wealth estimated in Section 5, and (ii) the event study estimates of the impact of migration on economic outcomes estimated in Section 6. The product of these two impacts (migration elasticity$\times$migration impact) then needs to be multiplied by the share of the aggregate outcome Y generated by the wealthy entrepreneurs (as shown in Table 1).

7.1.1 Are Treatment Effects of Migration Heterogeneous?

As explained in section 4, our two-step procedure requires that the LATE for migration impacts are based on the same population as the LATE for migration elasticities. For this reason, we now probe into the presence of heterogeneous treatment effects.[47]

Selection Into Tax-Induced Migration We apply standard methods used in the literature on selection in insurance markets (e.g. Hendren et al., 2021). We define marginal individuals as the group of compliers in the wealth tax reform, i.e. individuals who would have moved before the reform, and stopped moving after reform.

We can first look at the observable characteristics of these compliers. To do so, in Table VI.1, we start by comparing the average characteristics of wealthy migrants before (column (2)) vs after the reform (column (3)). Results reveal that these two groups of migrants are extremely similar (panel A) and that the firms they own are also remarkably comparable (panel B). In column (4) we

on firms that generate value added over and above the labor returns of their owners.

[47] As shown in equation (2), in the presence of treatment effect heterogeneity, the calibration of the migration impact in equation (1) requires that we use the treatment effect of migration on individuals at the margin of migration because of the wealth tax, weighted by the average characteristics of these marginals relative to the average population of the wealthy.

formally test for the null of equality of characteristics between pre- and post-abolition migrants. t-statistics suggest that pre-abolition migrants are a bit younger, a bit wealthier and slightly more likely to own a closely-held business. But apart from that, we cannot reject that they are the same as post-abolition migrants, neither can we reject that the characteristics of their firms are the same.

From these comparisons, we can then formally retrieve the average characteristics Y^m of compliers. The methodology is simple. The average characteristics of pre-abolition wealthy migrants is a weighted average of the characteristics of the compliers and of infra-marginal migrants: $\omega Y^m + (1-\omega)Y^i$. While the average characteristics of inframarginal migrants Y^i corresponds to the post-abolition average. By plugging the fraction of compliers $\omega = .3$ in pre-abolition migration flows (estimated in section 5), we can then easily estimate Y^m, which we report in column (5) along with standard errors computed with the Delta-method.[48]

The average migrant induced to move because of the wealth tax is 46 years old, born in Sweden, has tertiary education, and is around the 65-th to 70-th percentile of the distribution of cognitive and non-cognitive skills. To gauge the extent of selection into tax-induced migration, it is useful to compare compliers to the average characteristics of wealthy individuals Y^w from column (1). We find compliers to be a bit younger, a bit more likely to be entrepreneurs, and their net wealth to be slightly larger.[49] But as far as firms are concerned, we find no evidence of selection into migration. In other words, firms owned by compliers are not different from firms owned by all wealthy individuals.

Treatment Effects of Migration for Tax Reform Compliers The analysis of compliers suggests that movers are very similar before vs after the repeal of the wealth tax. While this should alleviate concerns that treatment effects of migration are strongly different among compliers, we now test for treatment effect heterogeneity more formally.

In Appendix Table VI.2, we compare migration impacts estimated on events happening before and after the repeal of the wealth tax. The table shows that the event study estimates of out-migration are similar before and after the repeal of the wealth tax. For almost all outcomes, the effects of out-migration are slightly larger, but not statistically different. This implies that the out-migration effects for those who move because of wealth taxes are not statistically different from the effects of other moves. Following the same strategy as in Table VI.2, we then estimate the treatment effect of migration for compliers $\frac{\partial Y}{\partial N}\big|_m \cdot \frac{1}{Y^m}$, which we report in column (4) of Appendix Table VI.2. We find slightly larger LATE for our population of compliers than for the whole population of migrants pre-2007, although we cannot rule out homogenous treatment effects. In column (5),

[48] For firm outcomes we use the fraction of compliers among entrepreneurs, 0.51.

[49] One implication is that the elasticity of the wealth tax base (i.e. the elasticity of migration flows weighted by wealth level) is -.30 (.07), a bit larger than the flow migration elasticity.

we further investigate whether the modest decrease in point estimates before vs after the abolition of the wealth tax could be driven by other trends unrelated to changes in tax-induced migration. We exploit our difference-in-difference framework and compare migration event study estimates before vs after 2007 for treated individuals (belonging to the top 2% of the wealth distribution) vs untreated individuals (in the top 20 to top 10% of the wealth distribution). This approach leads to very similar LATE estimates for compliers, which we will use as our preferred estimates of migration impacts in our calibrations below.

7.1.2 Estimated Aggregate Effects

Putting together these estimated impacts of migration for the tax reform compliers with our estimate of the migration elasticity, we can now calibrate formula (1) to compute the aggregate economic effects of tax-induced migration. The results are presented in Figure 10, with computational details provided in Appendix VI.[50] The main take-away from the figure is that the effects of tax-induced migration on aggregate economic outcomes are modest. A one percentage point increase in the average wealth tax rate on the top 2% of wealthiest taxpayers reduces aggregate employment by 0.022%, aggregate investment by 0.065%, and aggregate value added by 0.103% in the long run. The effects are small despite the fact that, as we have shown, wealthy entrepreneurs account for a substantial share of economic activity through the firms that they control directly and indirectly. The main reason for the small effects is the small elasticity of the stock of wealthy individuals, which itself is explained by the fact that migration flows at the top of the wealth distribution are very small.

Figure 10 also presents results from alternative calibrations to assess the robustness of our findings. We explore the implications of different assumptions regarding the migration elasticity and the migration effect. The alternative assumptions imply smaller effects on aggregate outcomes. This suggests that, if anything, our baseline estimates can be interpreted as a conservative upper bound. In particular, accounting for buyouts when measuring migration impacts on firms (which we did not do in the baseline) significantly reduces the aggregate effects.

Finally, it should be stressed that, by reporting the effects of a 1pp change in the average tax rate on wealth, we are considering a large wealth tax experiment. The variation is about twice as large as the actual variation created by the Swedish wealth tax repeal for the top 2% of wealthiest taxpayers. Hence, the aggregate effects of the Swedish wealth tax reform are roughly half the size of the effects shown in Figure 10.[51]

[50] Because τ is small, $d(1-\tau)/(1-\tau) \approx -d\tau$. We therefore interpret our estimates of $\frac{dY/Y}{d(1-\tau)/(1-\tau)}$ as the percentage effect on Y of a 1 percentage point increase in the effective average tax rate on wealth.

[51] Another way to present these effects is to compute the implied fiscal cost per job "created" by migration responses to the abolition of the wealth tax in Sweden. We find that each job created costs about 3,257,000 Swedish Kronor (or about 340,000 US Dollars) of tax revenue, corresponding to about 15 years of average salary in Sweden at the time.

7.2 Aggregate Tax Revenue Implications

Having shown that the aggregate external effects of tax-induced migration on economic activity are modest, we now turn to the effects on tax revenue. The fiscal externalities from behavioral responses to wealth taxes are important for evaluating welfare effects and optimal taxation. To fully evaluate these effects, we account for fiscal externalities operating through both the extensive margin (migration responses) and the intensive margin (taxable wealth responses among stayers). For intensive margin responses, we use the estimates in Jakobsen et al. (2020).

When calculating the aggregate revenue effects of increasing the wealth tax rate, we account for the fiscal externalities on all tax bases, not just the wealth tax base. The total tax revenue collected from wealthy individuals equals $N \cdot \mathbb{T}$, where N is the size of the wealthy population and $\mathbb{T} = \mathbf{t} + \tau W$ are total taxes per wealthy individual. The latter includes wealth taxes (τW) and all non-wealth taxes paid by the wealthy ($\mathbf{t}$). The revenue effect of changing the wealth tax rate can be written as the sum of a mechanical effect $dM = N \cdot W \cdot d\tau$ and a behavioral effect $dB = dB_E + dB_I$, including the extensive margin effect from migrants (dB_E) and the intensive margin effect from non-migrants (dB_I).

The key statistic is the ratio of behavioral to mechanical revenue effects, dB/dM. Absent non-fiscal externalties (we have shown these to be small), this ratio is sufficient for measuring the marginal efficiency effect of taxation (see e.g., Kleven, 2021). We have:

$$dB/dM = \underbrace{\frac{dN/N}{d(1-\tau)/(1-\tau)} \cdot \frac{\partial \mathbb{T}}{\partial N} \cdot \frac{1}{W}}_{\text{Migration Effect}} + \underbrace{\frac{dW/W}{d(1-\tau)/(1-\tau)} \cdot (\tau + t^K r)}_{\text{Intensive Margin Effect}}$$

It is straightforward to compute the migration effect using our estimates. As we have seen, the elasticity of the stock of wealthy individuals equals $\frac{dN/N}{d(1-\tau)/(1-\tau)} = 1.77$. To calibrate the term $\partial \mathbb{T}/\partial N \cdot 1/W$, we use our estimates of the effects of migration on tax payments from section 6. We account for all non-wealth taxes on wealthy people from Figure 6 and for business taxes on firms directly or indirectly owned by wealthy people from Figure 7. We also account for the fact that wealthy outmigrants are slightly wealthier, and that the migration impact of compliers on tax payments are a bit larger, as we just documented above. We find that the fiscal externality dB/dM from migration responses alone equals 0.22. That is, for each additional dollar of revenue raised mechanically by wealth taxes, 0.22 dollars are lost due to migration responses.

Two points are worth noting. First, the fiscal externality from migration responses is driven mostly by tax bases other than the wealth tax. This reason is that wealth taxes account for a relatively small part of total taxes $\mathbb{T}$ paid by the wealthy. Second, the fiscal externality from migration responses

is relatively small: on their own, these responses are much too small to suggest that the Swedish wealth tax was anywhere close to the Laffer bound (i.e., where $dB/dM = 1$).

Finally, we compare the revenue effects of migration responses to the revenue effects of intensive margin responses, which can be computed using the estimates in Jakobsen et al. (2020).[52] Our calculations imply a fiscal externality from intensive margin responses equal to 0.54, about 2.5 times larger than for migration responses. The main take-away is that the distortions of wealth taxation are driven mostly by taxable wealth responses at the intensive margin. Migration responses to wealth taxes, while being salient in the public debate, impose much smaller revenue and efficiency costs.

7.3 External Validity and Policy Implications

To what extent can the evidence presented here generalize to other contexts? Did, for instance, the Swedish tax system create unusually strong incentives to relocate economic activity abroad upon migration, since non-residents remained liable for wealth taxes on domestic assets. It appears that this feature is not unique to Sweden. Most countries have residence-based taxation systems that adhere to OECD international taxation principles and impose taxes on non-residents for income and wealth generated within their jurisdiction. Therefore, the Swedish environment does not create specific incentives to relocate assets upon migration, and is representative of most wealth taxes that have been implemented in developed countries.

A second question relates to the exemption of business assets from the Swedish wealth tax. Such exemptions are in fact common: 10 of the 13 European wealth taxes surveyed by Scheuer and Slemrod (2021) include full or partial exemptions for business assets. Interestingly, when replicating our analysis in Denmark–a setting where business assets were taxed–we found both the level and responsiveness of top-wealth migration to be comparable. This suggests that our estimates of the semi-elasticity of migration are potentially portable to countries with different tax regime of their business assets.

We note that our migration elasticities are computed out of multiple reforms, which, despite being of different intensities, systematically decreased, rather than increased, wealth tax rates. As a consequence, we cannot rule out the possibility of asymmetric responses to tax increases vs tax cuts. Furthermore, while identification of the migration elasticity comes in our case from variation in tax rates in the residence country, the elasticity itself may be a function of the level of taxes in all potential destination countries. When describing migration patterns, we found that destination

[52] These estimates are based on wealth tax reform in Denmark. To calibrate the long-run effect of the wealth tax on log wealth, we use $\frac{dW/W}{d(1-\tau)/(1-\tau)}$ = 37.8 from Table III (Panel A, Column 5) in Jakobsen et al. (2020). Note that our computation also accounts for the externalities generated by intensive margin responses to future capital income tax payments, $t^K r$.

countries of wealthy Swedish taxpayers are predominantly lower tax countries (UK for its *non-dom* tax regime, Switzerland, Austria for its advantageous bilateral treaty with Sweden exempting all capital gains from taxation, etc.). Around 2007, most European countries had much lower level of taxes on wealth than Sweden. One may hypothesize that migration responses would have been more muted in a context where all neighboring countries had much higher taxes on the wealthy.

A rare feature of our study lies in the ability to replicate the analysis in two different countries, Sweden and Denmark, whose wealth tax systems were structured differently. At the same time, these two countries are broadly similar in terms of their economic and political institutions. They are both small open economies with a relatively low degree of wealth inequality.[53] All else equal, small open economies tend to have *greater* migration elasticities than larger, more closed economies like the United States (see Kleven, Landais, Muñoz and Stantcheva, 2020). Moreover, US entrepreneurs are likely affected by stronger agglomeration forces than Scandinavian entrepreneurs. By themselves, these arguments suggest that our estimates provide *upper bounds* on migration elasticities and trickle down. While this is somewhat speculative, the main point is that there is nothing obvious about our empirical setting that would lead us to expect smaller effects than elsewhere.

8 Conclusion

In this paper, we provide some of the first evidence on international migration responses to wealth taxation and investigate their aggregate economic implications ("trickle down"). The analysis is based on rich administrative data from Sweden and Denmark and exploits three large wealth tax experiments in these countries. We find clear and precisely estimated effects of wealth taxes on the migration flows of the wealthy. The semi-elasticity of migration with respect to the net-of-tax rate on wealth equals -0.17 for out-migration and +0.05 for in-migration. Based on a simple theoretical framework, we show that these flow effects translate into a modest effect on the steady-state stock of wealthy people: a one percentage point increase in the top wealth tax rate reduces the stock of wealthy taxpayers by about 2 percent in steady state.

We develop a transparent approach to map these migration responses into aggregate economic effects. The approach combines the quasi-experimental estimates of migration responses to wealth tax reform with event studies of out-migration, leveraging our rich data on individual-level, firm-level, and market-level outcomes. We show that trickle-down effects do exist, but that they are quantitatively small. A one percentage point increase in the top wealth tax rate decreases aggregate

[53] However, it is worth noting that Sweden has more billionaires per capita than the United States. According to *The World's Billionaires* by Forbes Magazine, Sweden has 4.1 billionaires per million people while the United States has only 2.4.

employment by 0.02%, aggregate investment by 0.07%, and aggregate value-added by 0.10% in the long run. Importantly, these effects are modest despite the fact that the wealthy—many of which are entrepreneurs—account for a large share of economic activity in Scandinavia through the businesses they control. Our approach to estimating trickle-down effects is arguably the most innovative part of our paper. It is based on clear identification assumptions and is statistically precise.

These modest economic effects of tax-induced migration do not necessarily imply that wealth taxation is an optimal policy. To evaluate wealth taxes, we also have to account for their effects along the intensive margin, operating through changes in savings, investments, avoidance, and evasion. Jakobsen, Jakobsen, Kleven and Zucman (2020) find sizable intensive margin effects of wealth tax reform in Denmark. Combining the migration estimates presented here with their intensive margin estimates, we show that the Scandinavian wealth taxes were below the Laffer point and that their Marginal Cost of Public Funds (MCPF) was about 4.2.[54] Ignoring equity arguments, a strong requirement for taxing top wealth is that the revenue raised is spent on projects with a Marginal Value of Public Funds (MVPF) greater than 4.2. Comparing MVPFs across a range of policies, Hendren and Sprung-Keyser (2020) argue that programs targeted to low-income children have the highest MVPFs, often greater than 5. This suggests that funding projects for low-income children via progressive wealth taxation holds the potential to increase social welfare.

[54] In other words, our results imply that 0.7 dollars were lost for each additional dollar raised mechanically through wealth taxes.

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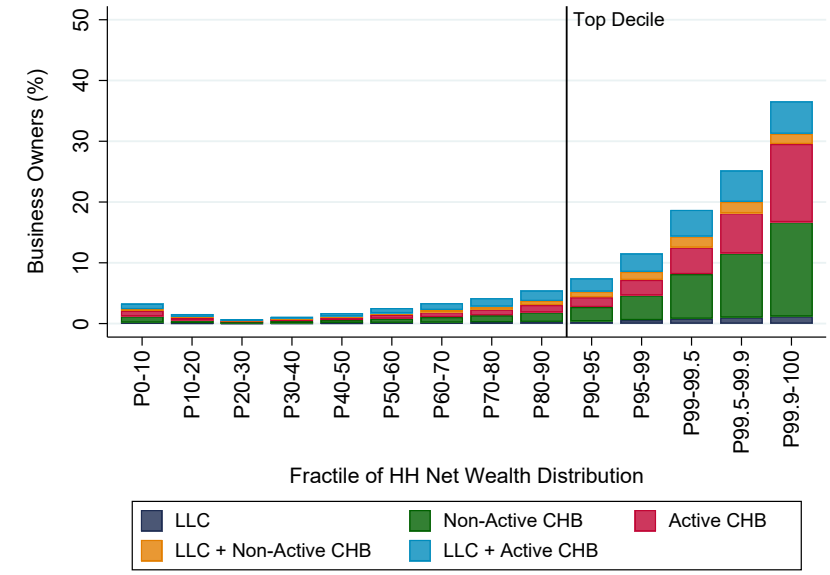
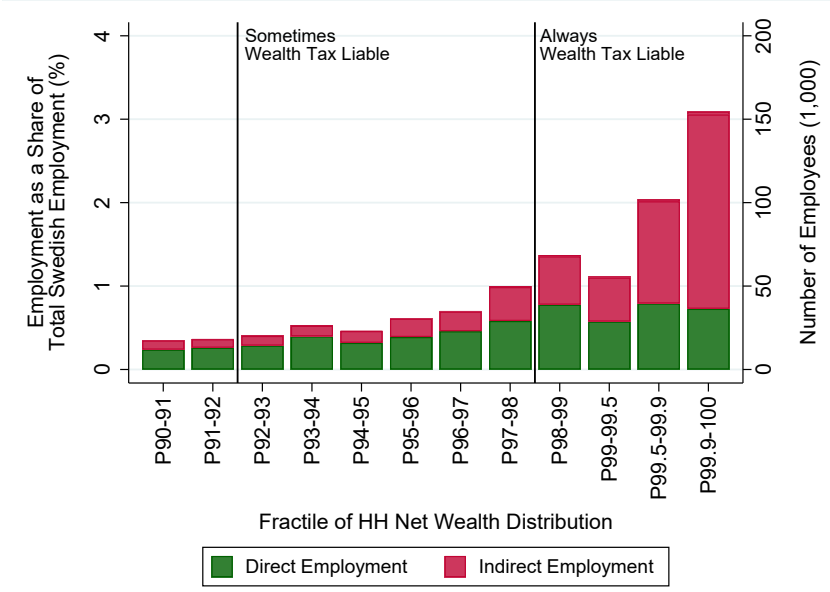
Figures and Tables

Figure 1: **Wealth and Entrepreneurship in Sweden**

A. Business Owners by Level of Net Worth in Sweden

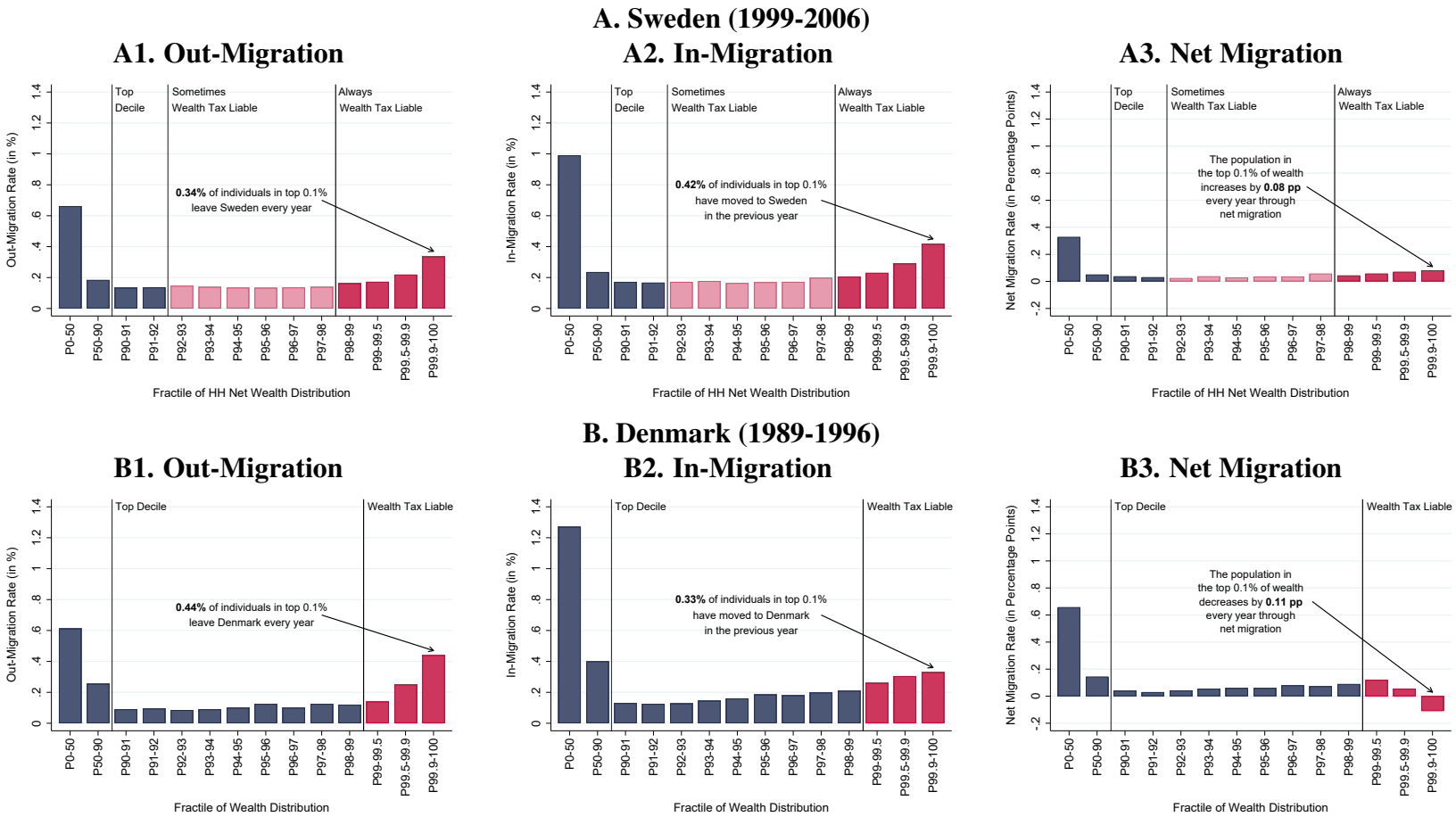


B. Employment at Firms Owned by the Wealthy



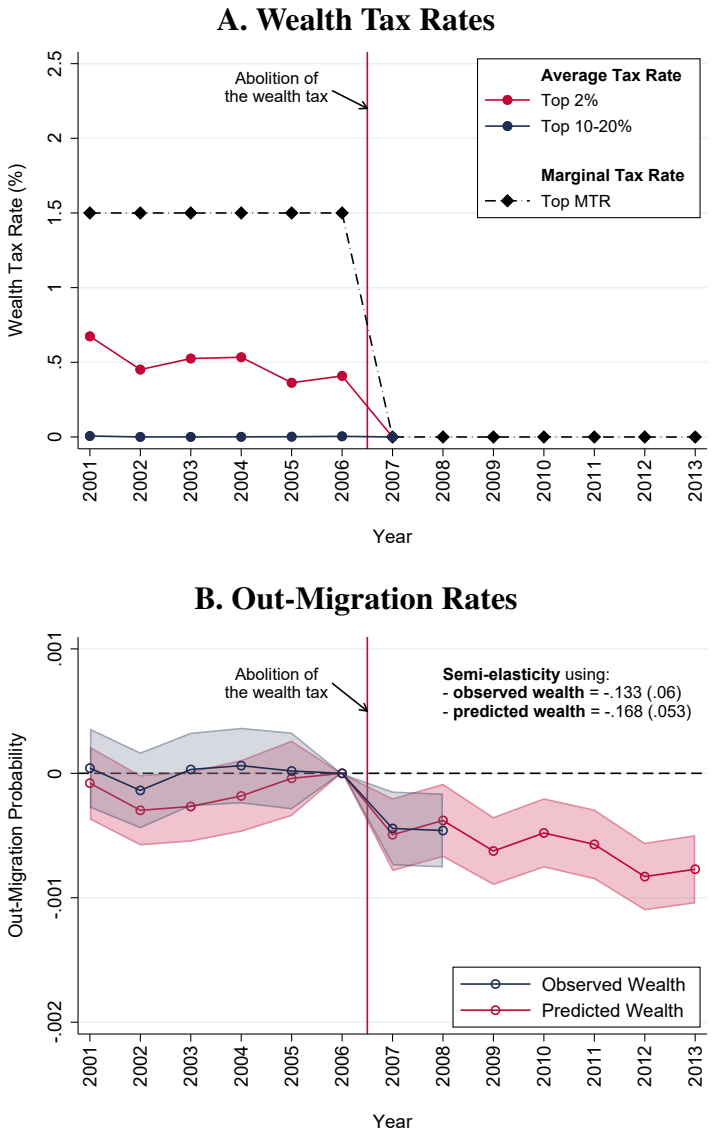
Notes: This figure describes firm ownership in Sweden during the period 2000-2006, when a wealth tax was still in place in Sweden. Each year, we rank households by their net wealth level and assign this rank to the individuals in the household. Panel A shows the share of individuals owning at least one firm in Sweden by level of their household net wealth. For each wealth fractile, we compute the percentage of individuals who own firms in each category. Active closely-held businesses ("CHB") are CHBs employing at least one person beyond the owners of the firm. In Panel B, we show the share of all Swedish workers employed at firms controlled directly (green bars) or indirectly (pink bars) by individuals in the top wealth decile. We allocate employment at subsidiaries to their ultimate owners by using the registry of ownership links across all Swedish firms. We exclude from our measure of employment the employment of owners within the firms they control directly.

Figure 2: **Migration Flows by Percentile of Net Wealth in Scandinavia**



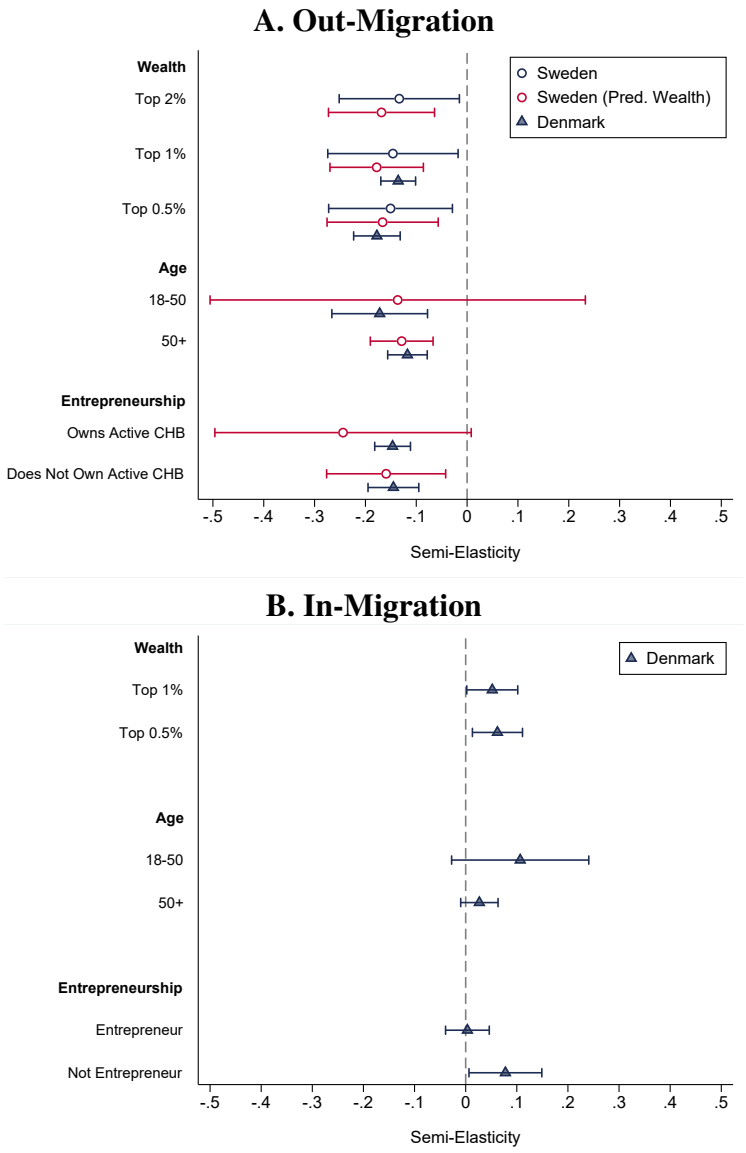
Notes: This figure shows in-migration, out-migration and net-migration rates by level of total household net wealth in Sweden (Panel A) and total household net wealth in Denmark (Panel B). We compute those statistics during the years in which the wealth tax was still in place in each country, which corresponds to the period 1999-2006 in Sweden and the period 1989-1996 in Denmark. For out-migration rates, we rank individuals by level of household net wealth in year t, and compute the fraction of individuals who out-migrate in $t+1$. For in-migration rates, we rank individuals by level of household net wealth in year t, and compute the fraction of individuals who in-migrated in $t-1$. The black vertical lines denote the wealth tax exemption threshold. In Sweden, the exemption threshold varied over the period 1999-2006. Therefore, we show the lowest and highest exemption thresholds in Sweden during the period 1999-2006.

Figure 3: **Swedish Wealth Tax Repeal and Out-Migration Flows**



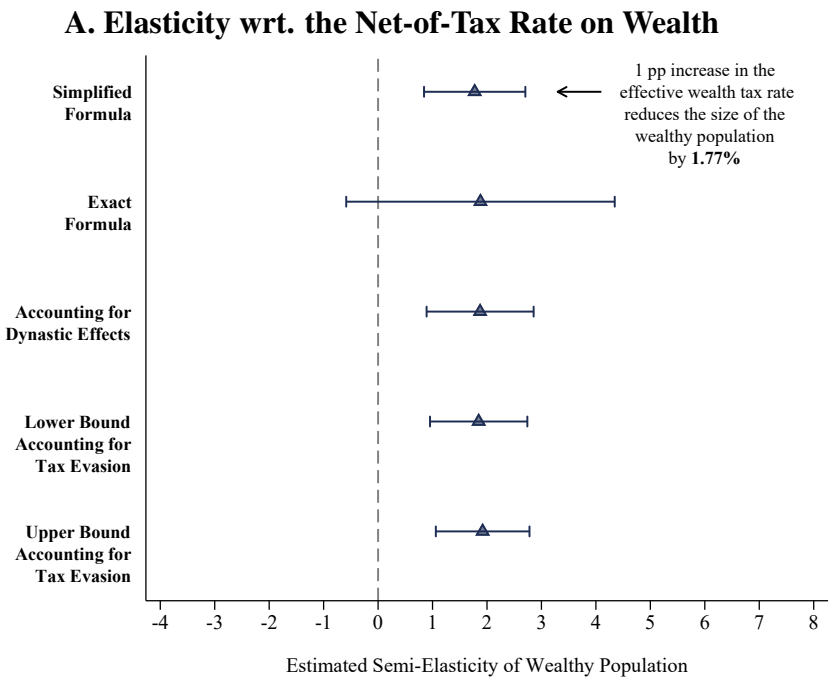
Notes: Panel A reports the evolution of wealth tax rates before and after the repeal of the wealth tax in Sweden. The dotted black line displays the evolution of the statutory marginal tax rate on wealth above the exemption threshold between 2001 and 2013. We show the corresponding evolution of the average effective tax rate (defined as total wealth tax payments over total household net wealth) for wealthy taxpayers in the top 2% of the household net wealth distribution (treated group, red series) and for the wealthy tax payers in the top 10-20% of the distribution (control group, blue series). Panel B reports the differential effects of the repeal of the wealth tax on the out-migration probability of treated (top 2% of the household net wealth distribution, subject to the wealth tax) and control (top 10-20%, not subject to the wealth tax) individuals. We regress the yearly probability to leave Sweden on an interaction between year fixed effects and a dummy variable equal to one if the individual is subject to the wealth tax. We define exposure to the reform using observed level of wealth (blue series) or predicted level of wealth based on pre-reform assets and income flows (red series). We omit year 2006 to interpret the effects relative to the year before the reform. We plot the estimated coefficients β_j from Equation 3 and their 95 percent confidence intervals. The semi-elasticities correspond to ε estimated from Equation 4 using 2001-2008 data for observed wealth, and 2001-2013 for predicted wealth.

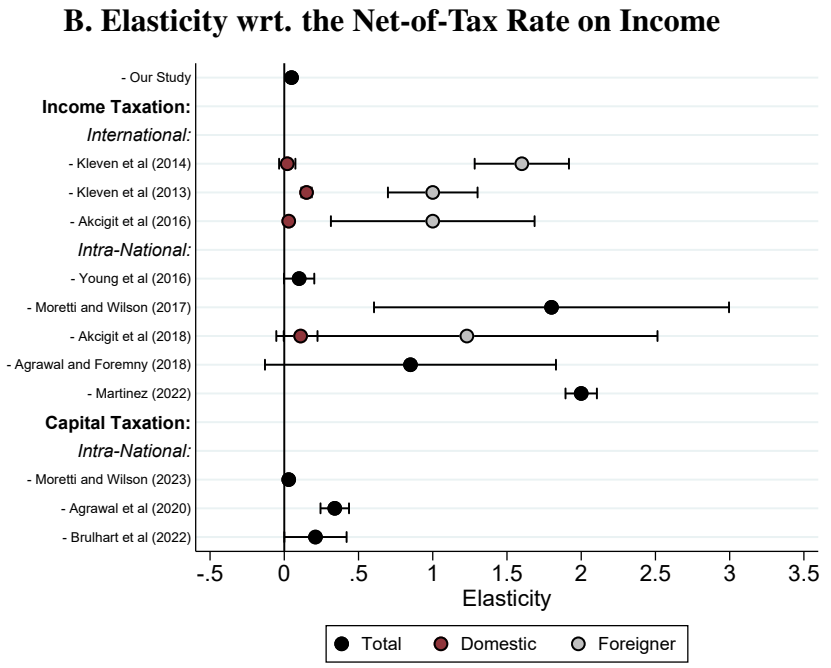
Figure 4: **Semi-Elasticities of Migration Flows**



Notes: This figure plots the semi-elasticities of the out-migration rate (Panel A) and the in-migration rate (Panel B) with respect to the net-of-tax rate on wealth. The coefficients showed in the figure correspond to ε estimated from Equation 4 and their 95 percent confidence intervals. Each coefficient and its confidence intervals refer to one separate regression. In Panel A, we estimate semi-elasticities of out-migration flows exploiting the repeal of the wealth tax in Sweden (blue and red circles) and two large wealth tax reforms in Denmark (blue triangles). For Sweden, we defined exposure to the reform using observed level of wealth (blue circles) or predicted level of wealth based on pre-reform assets and income flows (red circles). In Panel B, we estimate semi-elasticities of in-migration flows exploiting two large wealth tax reforms in Denmark (blue triangles).

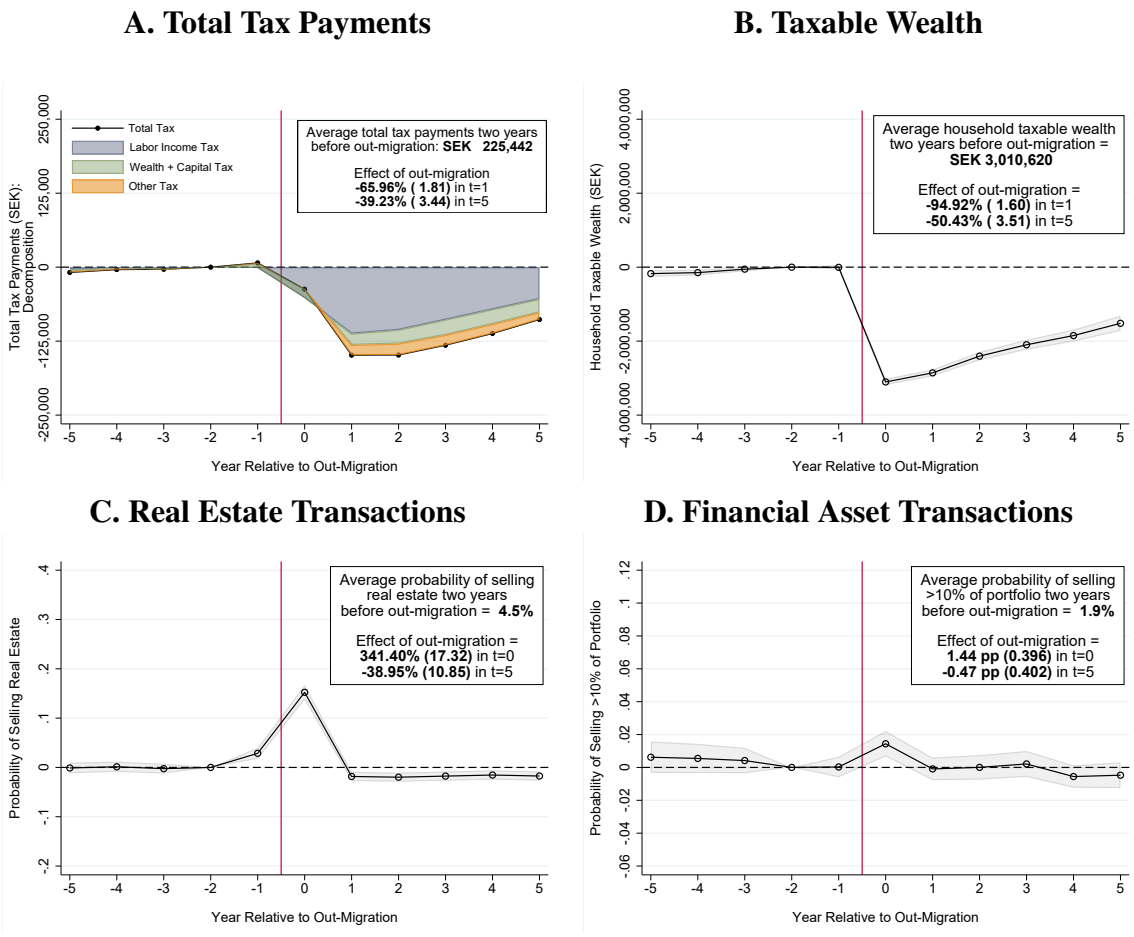
Figure 5: **Elasticities of the Stock of Wealthy Individuals**





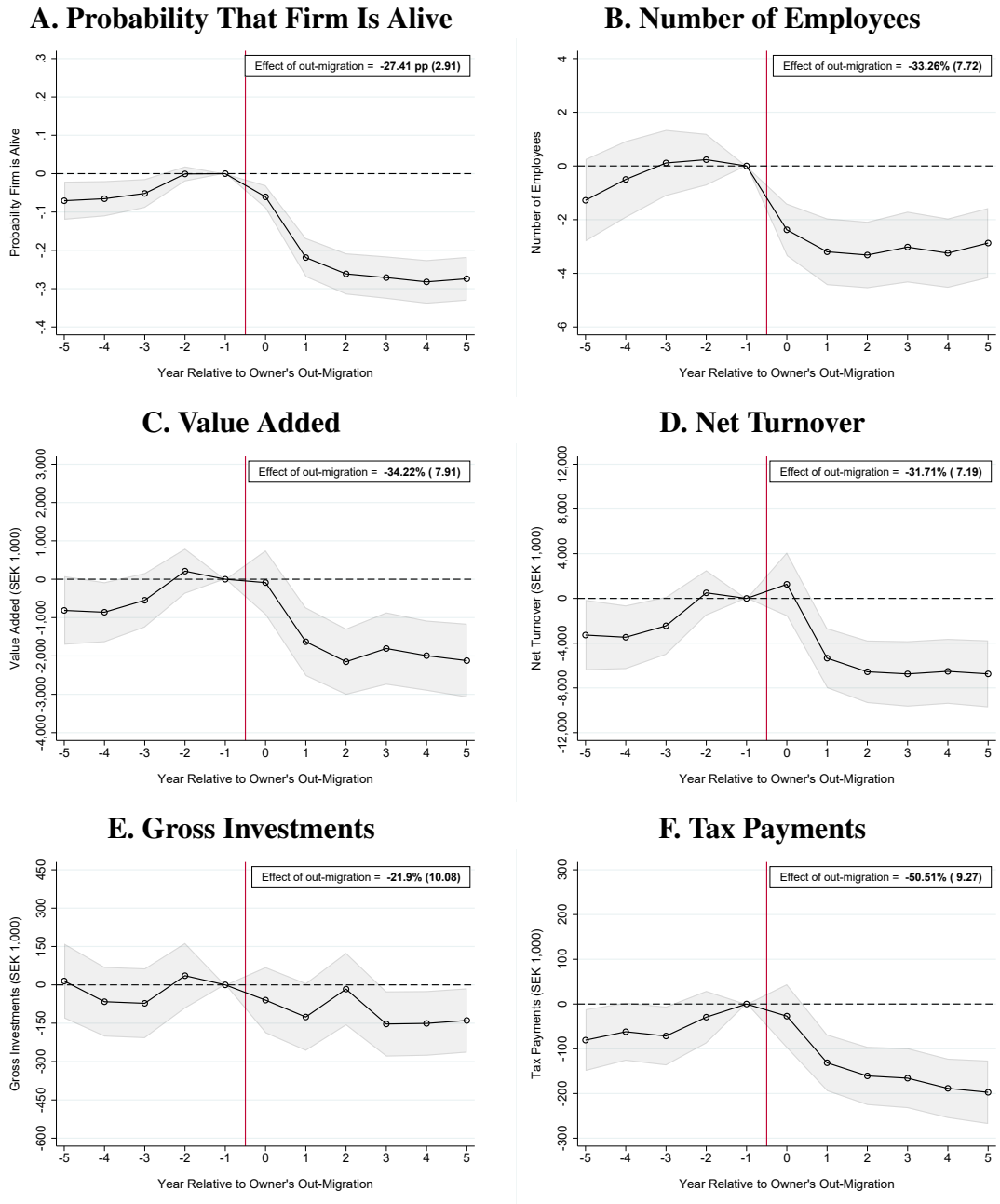
Notes: Panel A displays the effects of a one percentage point increase in the net-of-tax rate on wealth on the steady-state *stock* of the population of wealthy individuals. We cumulate the estimates of migration flows semi-elasticities showed in Figure 4 over time building on a model detailed in Appendix VI. In Panel B, we convert our estimate into an elasticity of the population of wealthy individuals with respect to the net-of-tax rate on *capital income* implied by the wealth tax and compare it to estimates in the literature, which are always expressed in terms of migration elasticities with respect to the net-of-tax rate on income. Our implied migration elasticity with respect to the net-of-tax rate on capital income is equal to .05.

Figure 6: **Effects of Wealthy Out-Migration on Tax Payments and Portfolio Composition**



Notes: This figure describes the evolution of wealthy individuals' outcomes before and after they leave Sweden, compared to control wealthy individuals who do not move that same year. The sample includes individuals who were in the top 2% of the household net worth distribution in Sweden for at least one year before their true or placebo out-migration date. We focus on out-migration events occurring between 2000 and 2007, with wealth ranks drawn from 1999-2006, when the wealth tax was in place. We winsorized the bottom 1% and top 5% of all outcomes. We plot the estimates β_j from Equation 7 and their 95 percent confidence intervals. The estimates displayed in the text boxes are computed as the estimate of β_j when $t = 0$, $t = 1$ or $t = 5$ divided by the average outcome in the treatment group in $t = -2$, multiplied by 100. The standard errors are rescaled using the same approach.

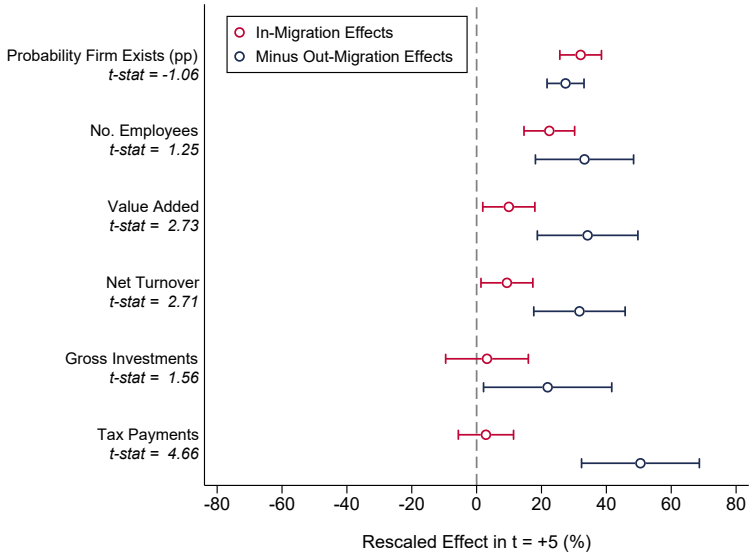
Figure 7: **Effects of Wealthy Out-Migration on Closely-Held Businesses**



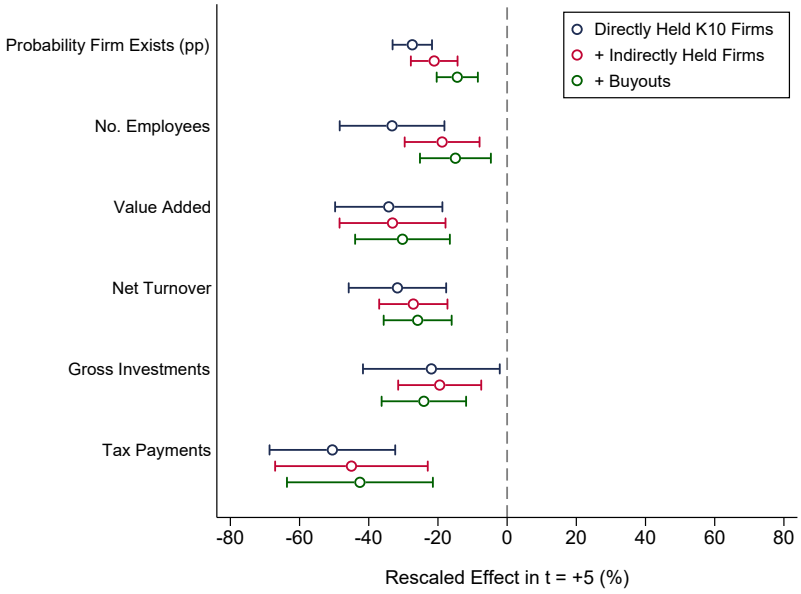
Notes: This figure shows the effects of wealthy owners' out-migration events on firm-level outcomes. We focus on out-migration events occurring between 2001 and 2007, with wealth ranks drawn from 2000-2006, when the wealth tax was still in place in Sweden. The sample includes active closely-held businesses controlled by wealthy individuals in the year $t-1$, with (real or placebo) out-migration events occurring in the subsequent year t. We winsorized the bottom 1% and top 5% of all outcomes except for the number of employees, for which we winsorized only the top 5%. We plot the estimates of β_j and their 95 percent confidence intervals from Equation 7. The effect displayed in the text boxes is computed as the estimate of β_5 divided by the average outcome in the treatment group in $t=-1$, multiplied by 100. The standard errors are rescaled using the same approach.

Figure 8: **Accounting for In-Migration, Indirectly Held Firms, and Buy-outs**

A. In-Migration vs Out-Migration Effects

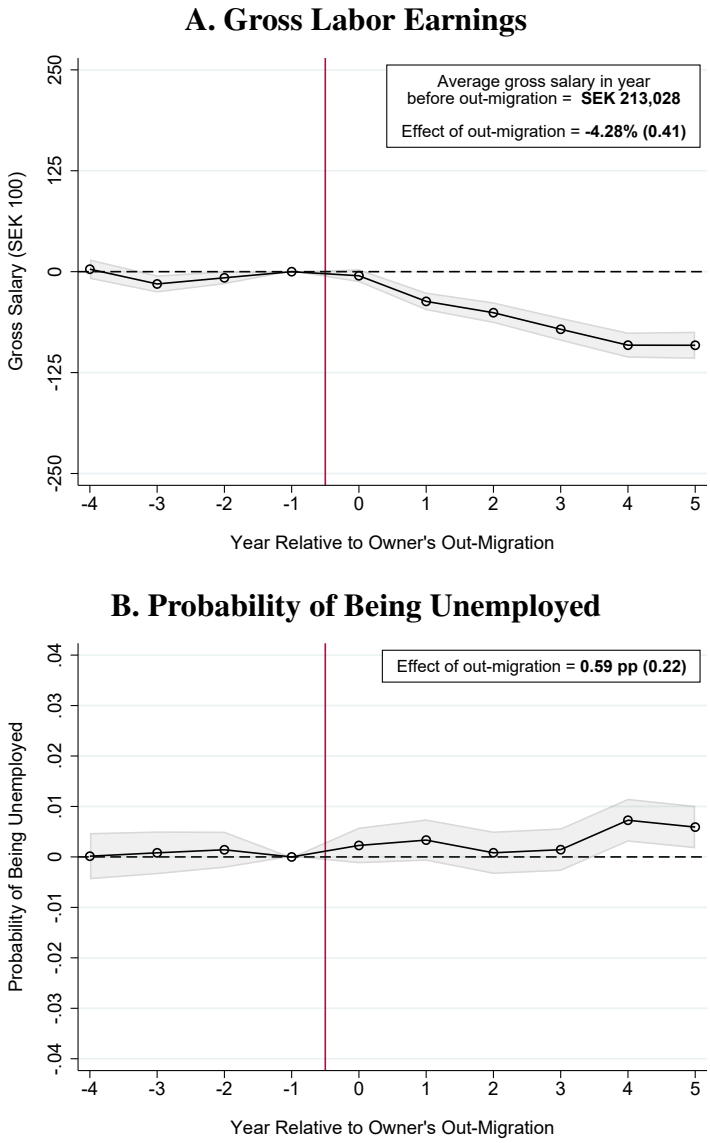


B. Accounting for Indirectly Held Firms and Buy-Outs



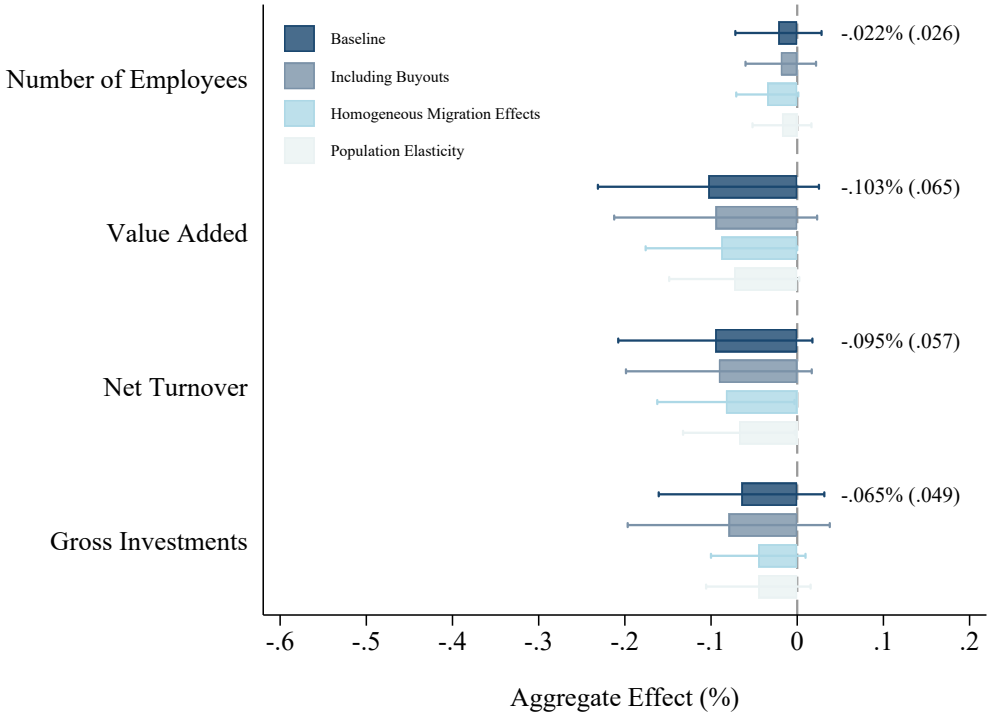
Notes: In Panel A, we study the effects of wealthy owners' in-migration and out-migration events on firm-level outcomes. We study in-migration events during the period 1999-2006, when a wealth tax was still in place in Sweden. The sample includes active closely-held businesses directly owned by individuals whose real or placebo in-migration happened in year t and who were in the top 2% of the household net wealth distribution in Sweden for at least one year after t. We rescaled our coefficients β_5 from Equation 7 estimated separately for out-migration events (blue coefficients) and in-migration events (red dots) by the average outcome in the treated group of the out-migration event-study sample in $t-1$. Additionally, we multiply the out-migration effects by -1 to ease comparison. In Panel B, we augment the baseline estimates of out-migration effects presented in Figure 7 adding firms held indirectly by the wealthy in our estimation sample (pink dots) and accounting for firms' mergers after their closure (green dots).

Figure 9: **Effects of Wealthy Owners' Out-Migration on Worker-Level Outcomes**



Notes: This figure shows the effects of wealthy owners' out-migration events on worker-level labor market trajectories. We focus on out-migration events occurring between 2001 and 2007, with wealth ranks drawn from 2000-2006, when the wealth tax was still in place in Sweden. We focus on workers employed at firms controlled directly or indirectly by wealthy entrepreneurs in the year before the (real or placebo) out-migration event. Panel A uses gross labor earnings at the main outcome, while Panel B focuses on the probability to be unemployed. Each regression controls for pre-existing trends in the outcome. We plot the estimates of β_j from Equation 7 with worker-level outcomes as the outcome variable and we report β_5 rescaled by the average outcome for the treatment group (workers employed at firms controlled by wealthy out-migrants) in year $t-1$ in the text boxes.

Figure 10: **Aggregate Economic Effects of Migration Responses to a 1pp Increase in the Wealth Tax Rate**



Notes: This figure presents the outcomes of our quantification exercise, which evaluates the aggregate economic effects of migration responses following formula (1). This exercise is described in details in Appendix VI and in the main text. Because τ is small, $d(1-\tau)/(1-\tau) \approx -d\tau$. We therefore interpret our estimates of $\frac{dY/Y}{d(1-\tau)/(1-\tau)}$ as the percentage effect on Y of a 1 percentage point increase in the effective average tax rate on wealth. In the baseline calibration, we use the elasticity of the population stock estimated for active entrepreneurs. This elasticity is computed using the semi-elasticity of migration flows for entrepreneurs in Figure 4 and then translating it into an elasticity of the population stock using formula 5, with $T = 15.1$. For the impact of migration, we use estimated out-migration effects for treated entrepreneurs coming from the LATE estimates in Appendix Table VI.2, Column (5). Out-migration and in-migration effects are then weighted to give an average migration effect, where weights are the relative size of the respective in and out-migration elasticities. Estimates for the top wealth share come from Table 1, Panel C. Standard errors are computed using the delta method. In the first alternative calibration, we account for firms' reorganizations after closure, using estimates shown in Figure 8. In the second alternative calibration, we estimate the average migration impacts over the whole sample of treated entrepreneurs, rather than retrieving the LATE for marginal migrants. In the third alternative calibration, we use the out-migration elasticity estimated over the whole population, which is more precisely estimated than the elasticity for entrepreneurs only.

Table 1: **Descriptive Statistics on Firms Controlled by the Wealthy**

Variable	Mean	Median	Std. Dev.	Obs.	% of Swedish Aggregates
Panel A. All Active CHBs					
No. of Owners	1.78	1	7.05	589,788	
No. of Employees	8.03	3	40.49	589,788	13.53%
Value Added	3,398	1,518	30,859	541,097	21.84%
Net Turnover	10,610	3,878	61,029	541,097	17.68%
Gross Investments	534	55	4,661	541,097	17.88%
Tax Payments	138	21	4,708	541,097	27.64%
Panel B. Active CHBs with at least one owner in the top 2% of net worth					
No. of Owners	2.44	2	17.91	89,485	
No. of Employees	14.08	4	82.30	89,485	3.56%
Value Added	7,098	2,238	54,677	82,473	6.90%
Net Turnover	23,598	6,034	126,880	82,473	6.13%
Gross Investments	1,271	100	10,940	82,473	6.41%
Tax Payments	386	56	3,653	82,473	10.68%
Panel C. Active firms with at least one direct or indirect owner in the top 2% of net worth					
No. of Owners	5.61	2	72.38	138,067	
No. of Employees	22.57	6	116.97	138,067	9.18%
Value Added	10,341	2,912	58,351	128,602	15.43%
Net Turnover	38,691	8,386	255,191	128,602	15.63%
Gross Investments	1,646	118	16,418	128,602	12.22%
Tax Payments	502	58	5,157	128,602	18.98%

Notes: This table reports descriptive statistics for closely-held businesses in Sweden. We study active closely-held businesses ("CHBs") in Sweden during the period 2000-2007, that have at least one employee that is not the owners. The unit of measure for value added, net turnover, gross investments, and tax payments is SEK 1,000. Value added, net turnover, gross investments, tax payments as percentages of Swedish aggregates (last column) are obtained by dividing total value added, net turnover, gross investments, tax payments from active closely-held businesses in 2003 by the total of the same variables for all Swedish firms (including LLC, foreign firms, and listed firms) in 2003. For employment, the total number of individuals employed in active closely-held businesses in 2003 (excluding owners) is divided by the total number of individuals reporting as being employed in Sweden in the same year (including self-employed and employees in the public sector). In Panel C, we allocate employment at subsidiaries to their ultimate owners by using the registry of ownership links across all Swedish firms.