NBER WORKING PAPER SERIES

TAXING TOP WEALTH:
MIGRATION RESPONSES AND THEIR AGGREGATE ECONOMIC IMPLICATIONS

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Working Paper 32153
http://www.nber.org/papers/w32153

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2024, Revised January 2026

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). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 32153
February 2024, Revised January 2026
JEL No. D31, E21, H20, H31, L26

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%.

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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 fiscal externalities on other tax bases, the revenue implications of migration responses are insufficient 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 through migration responses. By comparison, 0.54 dollars are lost due to intensive margin responses—through changes in savings, investment, avoidance, and evasion—using the estimates in Jakobsen, Jakobsen, Kleven and Zucman (2020). Our findings indicate that migration threats, while prominent in the public debate, are less important for welfare and policy design than intensive margin responses. Nevertheless, because welfare effects are highly nonlinear, migration responses substantially increase the Marginal Cost of Public Funds (MCPF) associated with wealth taxes, from about 2.2 to 4.2.

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

[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.

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. 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

[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.

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 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.

[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.

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", individuals 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. Expressed as a percentile of the household wealth distribution, the threshold was at its lowest in 1999, at which point all households above the 92nd percentile of the wealth distribution were taxable. In 2006, the threshold had been raised significantly so that only couples belonging to the top 2% of the tdistribution were liable to the tax. For singles, the lower threshold meant that in 2006 they remained taxable if their wealth was above the 92nd percentile of the distribution. In what follows, we consider households with total net wealth in the top 2% of 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 (see Appendix Figure I.1 for details).

[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.

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 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

[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.

[14] The main argument used by the coalition to justify the wealth tax abolition pertained to its purported effects on

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. 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. See Appendix I.2 for further details on the Danish wealth tax and a comparison with the Swedish system.[15]

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 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

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] We refer to Jakobsen et al. (2020) for an empirical analysis of the Danish wealth tax, focusing on effects along the intensive margin.

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 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

[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).

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 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

[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.

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.[21]

Table 1 presents additional 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). 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.[22]

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

[22] In terms of sectors, 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

The last column of Table 1 quantifies the contribution of wealthy taxpayers' firms to the aggregate Swedish economy. It confirms that business assets held by the wealthiest individuals are quite granular, and account for a significant fraction of Swedish economic activity, especially once we account for the businesses held indirectly by wealthy taxpayers through complex ownership structures (panel C). 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.

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.[23] Conversely, an in-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.[24]

wealthy owners operate in the wholesale and retail trade sector as well as in real estate, renting, and business activities. See Appendix Figure IV.10, Panel A.

[23] 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.

[24] 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.

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.[25] 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 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.[26]

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

[25] In Appendix Figure III.3, we show out-migration rates for each decile of the distribution separately.
[26] See Appendix Figure III.4 for year by year out-migration and in-migration figures.

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 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:

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

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 6.5, 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, 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. But we show 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 (Appendix Figure I.2 panel A). It is only when wealth tax liability is taken into account that tax rates on capital evolve differently across treatments and controls (Appendix Figure I.3). We also find that the effective labor income tax rates evolved similarly in our treatment and control groups over time (Appendix Figure I.2 panel B). Hence, confounding effects from non-wealth tax changes do not seem to pose concerns in this context.[27]

We start by providing direct difference-in-differences evidence (Appendix Figure V.1). 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.

[27] 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.

Predicting Post-2007 Wealth to Estimate Long-Term Effects: The migration series from the difference-in-differences evidence discussed above 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.[28]

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.[29] Specifically, we consider the following linear probability model:

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

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.[30] 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.

[28] 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).

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

[30] 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.

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.

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. We confirm 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.[31] 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.

Our estimates prove remarkably robust 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%

[31] 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%.

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 (see Appendix Figure V.5 for details).

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 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.[32] 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.

[32] 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.

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 show a significant but small positive effect of the reform on return migration, building up over time (see Appendix Figure V.4). While these results demonstrate that the in-migration margin does also respond to wealth tax rates, the approach fails to capture the full 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? The effect on the steady-state stock of wealthy individuals ultimately 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. In the context of a simple OLG framework, it is possible to derive formulas for quantifying the stock elasticity, where the formulas depend solely on our estimated semi-elasticities of net-migration flow rates and on moments that can be observed in the data. Appendix VI provides all the details.

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 regenerates 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.[33]

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

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

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

[33] 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. $B = 1/T$.

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.[34]

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 is evaded.[35] And as a result, our measure of tax rates τ is an overestimate of their effective tax rates on wealth $\tilde{\tau} = (1-e)\tau$.[36] 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.[37] 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.

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

[35] 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.

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

[37] 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.

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.[38] 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 use an event-study design described in Section 6.1 to estimate the impacts of migration events on individual-level (Section 6.2), firm-level (Section 6.3), and market-level (Section 6.4) outcomes. We further investigate the presence of heterogeneous treatment effects of migration, and estimate the impacts of migration for individuals who moved in response to the 2007 wealth tax reform (Section 6.5).

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.[39] 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.

[38] 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.

[39] 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.

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).[40] 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 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 zero—the year in which migration happens—is only partially treated, it is more informative to focus on event year one. 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 the effect down 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 is smaller. Five years after migration, the reduction in total tax payments is about 40%. This reflects that a sizeable fraction of wealthy out-migrants eventually return to Sweden. As documented in

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

Appendix Figure III.1, around 40% of wealthy out-migrants have returned after 5 years.[41]

Portfolio Reallocation: We now 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 taxable wealth in Sweden by 94% one year after migration. 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. Looking at financial wealth, excluding business assets, we find a significant decline at both the extensive (-21%) and intensive margin (-15%), although it should be noted that a large fraction of wealthy out-migrants continue to hold financial assets in Sweden after departing (these results are reported in Appendix Figure IV.4). We use our detailed data on asset 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. A key feature of top-wealth holders is that they often own companies. Concerns that the migration of wealthy entrepreneurs 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 firms, we use our rich data on closely-held businesses and ownership structures described in section 3. This information allows us to link wealthy individuals to the firms they control either directly or indirectly.

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 equal to the first out-migration date among all the

[41] 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.

[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.

migrant owners of the firm. We 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 migration events among business owners in the top 2% of the wealth distribution.

The results are presented in Figure 7. It shows event studies of firm outcomes based on the specification in equation (7). 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 the previous subsection, 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 restructuring 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. When replicating the event studies conditional on firm survival, we find small

[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.

negative intensive margin effects on employment and small positive (but statistically insignificant) effects on value added and investment.[45]

Interestingly, we do not find that these average effects mask significant heterogeneity. As far as owner characteristics are concerned, we find limited heterogeneity by age or 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. If we take for instance the top-10 moves in terms of the value of business assets controlled by the owner – these are businesses worth several billions of Swedish Kronor – the effect of out-migration is considerably smaller than the average effect in the data. All these results are presented in detail in Appendix Figure IV.7.

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 – presented in Appendix Figure IV.9 – 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).[46] 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

[45] These results can be found in Appendix Figure IV.5.

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

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 sizeable 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 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, 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 aggregate outcomes in the old and new firms combined through the entire period of analysis. We then re-estimate our baseline specification using this alternative definition of firm boundaries and outcomes for all firms undergoing a merger, in both the treatment and control groups. We plot the resulting estimates with green dots in Panel B of Figure 8. As can be seen, accounting for buy-outs reduces the estimated effects of out-migration on the probability of firm existence as well as on employment by approximately 40%, although estimates remain negative and statistically significant. The impacts on turnover, value-added, and tax payments are also reduced significantly when accounting for buy-outs, but the effect is a bit smaller for these outcomes.

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 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. If we define markets at the level of municipality$\times$sector cells (measuring sector at the one-digit level), more than 80% of firms controlled by wealthy out-migrants have employment and value-added market shares that are smaller than 1%.[47] 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. If we focus on out-migration events by owners of businesses that represent at least 10% of employment in their market cell, we find that even for these granular businesses, the market-level effects of out-migration are small and statistically insignificant (see Appendix Figure IV.12). 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

[47] For further details, see Appendix Figure IV.11, which plots the distribution of market shares for businesses owned by wealthy out-migrants.

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.[48] This suggests that owner migration is much 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.

6.5 Are Treatment Effects of Migration Heterogeneous?

As explained in section 4, our two-step procedure requires that the LATE for the effects of migration are based on the same population as the LATE for migration elasticities. For this reason, we now investigate the presence of heterogeneity in the treatment effects of migration.[49]

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.

[48] We note that our analysis excludes "human capital vehicles" such as doctor practices and other LLCs. We focus on firms that generate value added over and above the labor returns of their owners.

[49] 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.

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: $\omega Y^m + (1-\omega)Y^i$. The average characteristics of inframarginal migrants Y^i corresponds to the post-abolition average. By inserting the fraction of compliers in pre-abolition migration flows ($\omega = .3$, estimated in section 5), we can estimate Y^m along with standard errors computed using the Delta-method.[50]

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. We find compliers to be a bit younger, a bit more likely to be entrepreneurs, and their net wealth to be slightly larger.[51] 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. See Appendix Table VI.1 for more detail.

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 Table 2, we compare migration impacts estimated on events happening before and after the repeal of the wealth tax. Note that for firm-level outcomes in panel B, we use event study estimates among firms that are directly or indirectly owned by a migrant owner, but not accounting buyouts and restructuring. The estimates in the first column (pre-abolition) are therefore similar to the estimates reported in panel B of Figure 8 (red dots). 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 above, 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 Table 2. These estimates are obtained from comparing the migration event study estimates for the periods pre vs post abolition, and scaling this "simple difference" by the fraction of individuals who moved because of the wealth tax. 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. Note

[50] For firm outcomes we use the fraction of compliers among entrepreneurs, which is equal to $\omega_{\text{entrepreneurs}} = .51$.

[51] 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.

that the *simple difference* estimates in column (4) may capture any change in time trends in migration effects over time, even absent the tax reform. In column (5), 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 can 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).[52] It turns out that the effects of migration are quite stable over time in the pre-abolition period, and evolve similarly for the treated and control group of migrants. After the abolition, we observe a small decline in the magnitude of the effects of migration among the treated group, while the estimates remain unchanged for control migrants. The *double difference* gives therefore very comparable results to the simple difference approach. The LATE estimates for compliers derived from the double difference approach are reported in column (5) of Table 2, and are very similar to the estimates from column (4). In the next section, we use column (5) as our preferred estimates of migration impacts among the wealth tax compliers.

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).

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 eco-

[52] Appendix Figure VI.2 provides a simple graphical illustration of this approach. For each year, we plot the coefficient estimates of the event study for all migrations happening in that year. We do this separately for individuals belonging to the treatment group (i.e. in to the top 2% of wealth, in red) and for individuals belonging to the control group (i.e. the top 20 to top 10% of wealth, in blue).

nomic effects of tax-induced migration. The results are presented in Figure 10, with computational details provided in Appendix VI.[53] 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.[54]

7.2 Effects on Tax Revenue and Economic Efficiency

Having shown that the aggregate external effects of tax-induced migration on economic activity are modest, we now turn to the effects on revenue and efficiency. The efficiency implications of wealth tax reform can be measured by the fiscal externalities from behavioral responses to the reform. To fully evaluate these effects, we need to 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

[53] 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.

[54] 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.

from wealthy individuals equals $N \cdot \mathbb{T}$, where N denotes the size of the wealthy population and $\mathbb{T} = \mathbf{t} + \tau W$ equals 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 externalities (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 calculate 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 personal wealth and 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 facts that wealthy out-migrants are slightly wealthier and that the migration impact of compliers on tax payments are a bit larger, as we 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. The reason is that wealth taxes account for a relatively small share 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 $dB/dM = 1$).

We may 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).[55] Our calculations imply a fiscal externality from intensive margin responses equal to 0.54, about 2.5 times larger than for migration responses. Hence, the efficiency costs of wealth taxation are driven

[55] These estimates are based on a 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, $t^K r$.

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.

Adding the intensive and extensive margin effects, our results imply that 0.76 dollars of revenue are lost for each additional dollar raised mechanically through wealth taxes. While this effect may seem large, the corresponding Marginal Cost of Public Funds (MCPF) of 4.17 ($= 1/(1 - dB/dM) = 1/(1 - 0.76)$) is not unusually large compared to the MCPFs typically found for top income taxes (Kleven and Kreiner, 2006; Hendren and Sprung-Keyser, 2020).

Three points are worth highlighting. First, because the MCPF is highly nonlinear, even modest migration responses can have large consequences for this welfare measure, particularly in the presence of substantial intensive margin effects. Using the intensive margin estimates in Jakobsen, Jakobsen, Kleven and Zucman (2020), our migration estimates nearly double the MCPF associated with top wealth taxes, from about 2.2 to 4.2. For the same reason, statistical uncertainty in either intensive-margin or migration responses to wealth taxes translates into substantial uncertainty in the magnitude of the MCPF.

Second, interpreting behavioral revenue effects as capturing the total efficiency effect of wealth taxes relies on the assumption that there are no pre-existing, non-government distortions associated with top-wealth accumulation (Kleven, 2021). Finally, these computations are done in the context of the Scandinavian wealth taxes of the 1990s and early 2000s, a time period where evasion and avoidance—especially through wealth offshoring—have been shown to be important (Alstadsæter et al. (2019)). Other policy instruments such as the choice of tax base, enforcement policies, and exit taxes may significantly affect the magnitude of both intensive and extensive margin elasticities, thereby influencing the efficiency implications of wealth taxation.

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 replicat-

ing 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 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.[56] 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

[56] 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.

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 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 top wealth holders—many of whom 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.

The modest economic effects of tax-induced migration do not necessarily imply that wealth taxation is an optimal policy. To evaluate wealth taxation, 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.[57] Leaving aside equity arguments, taxing top wealth would be welfare-improving if 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 has the potential to increase social welfare.

[57] The MCPF follows from our estimate that, for each additional dollar raised mechanically through wealth taxes, 0.76 dollars were lost due to migration responses and intensive margin responses combined.

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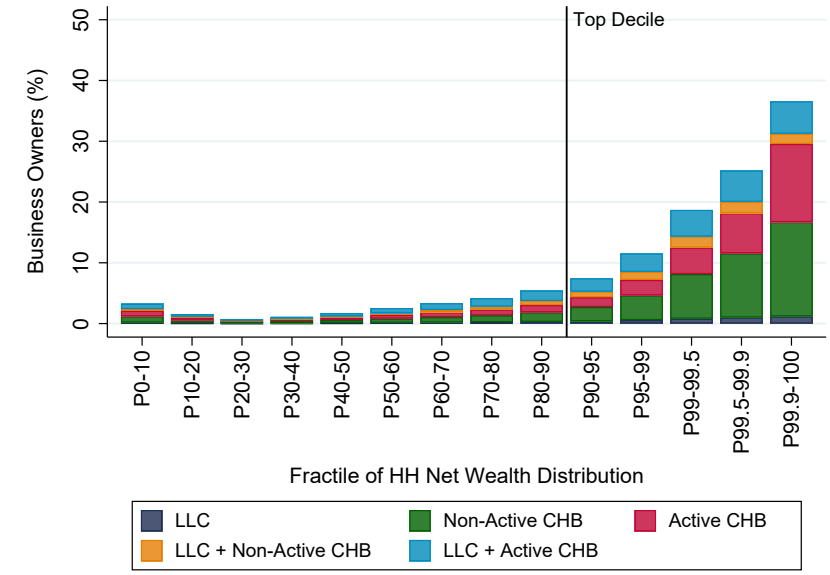
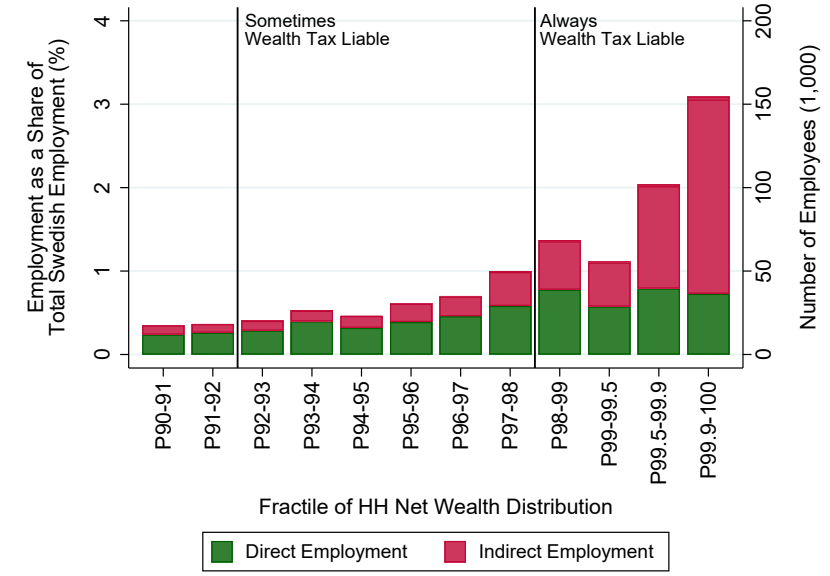
Figures and Tables

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

A. Business Owners by Level of Net Worth in Sweden

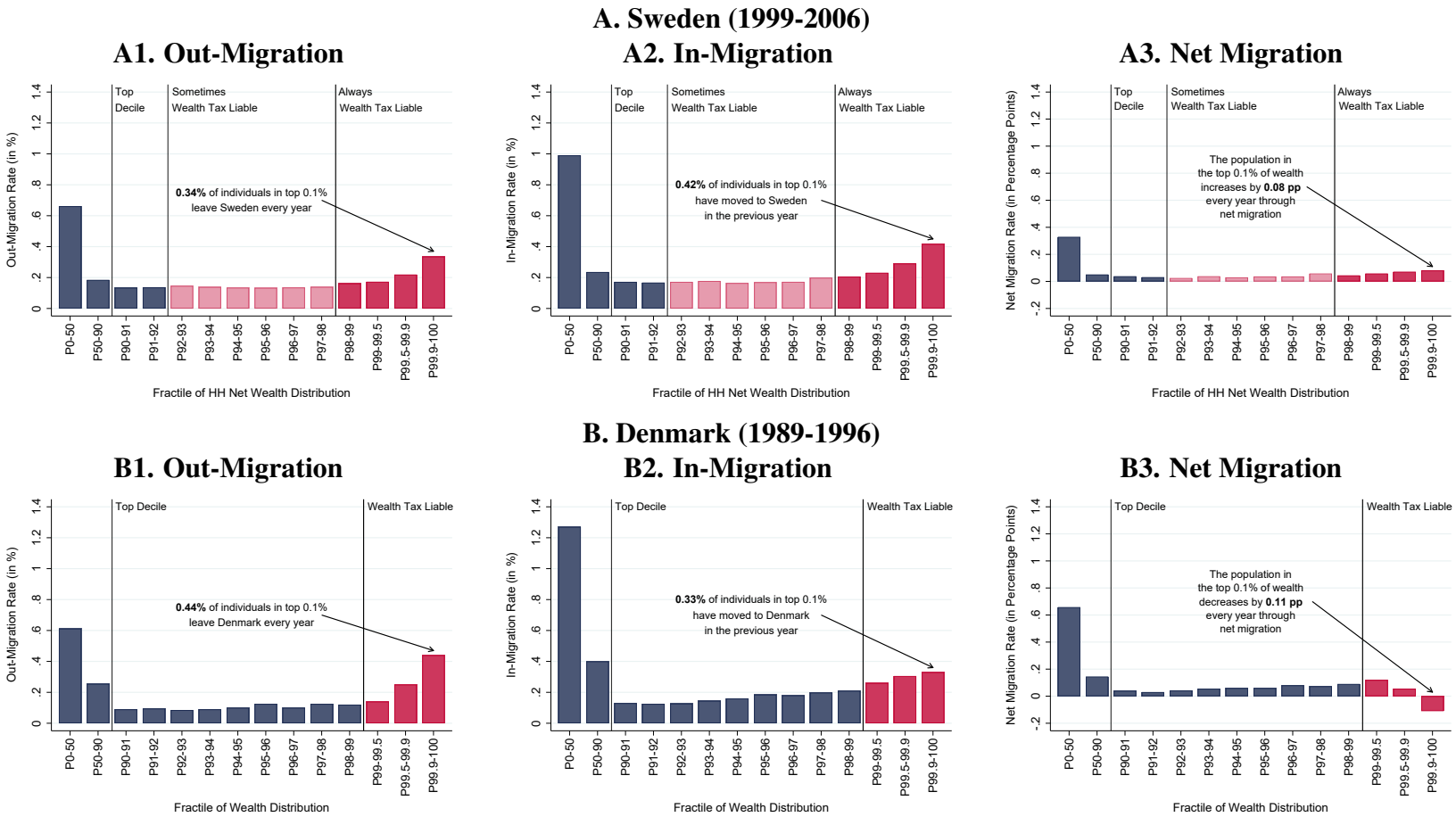


B. Employment at Firms Owned by the Wealthy



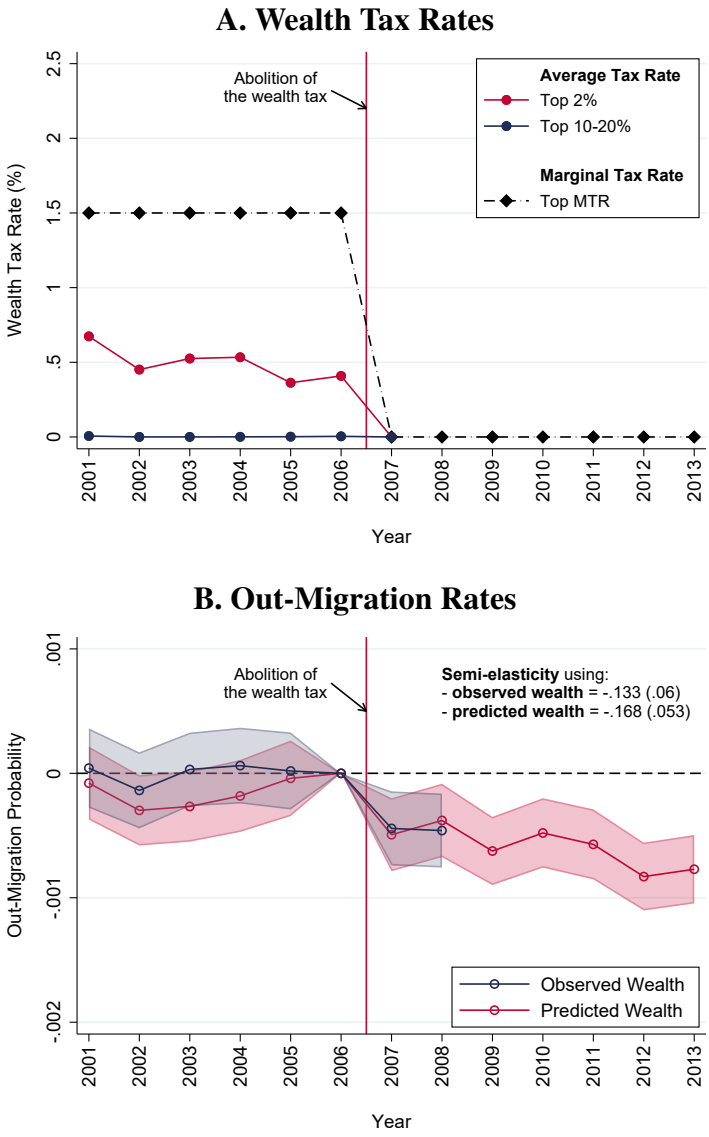
Notes: This figure describes firm ownership in Sweden during the period 2000-2006, when a wealth tax was still in place in Sweden. Each year, we rank households by their net wealth level and assign this rank to the individuals in the household. Panel A shows the share of individuals owning at least one 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**



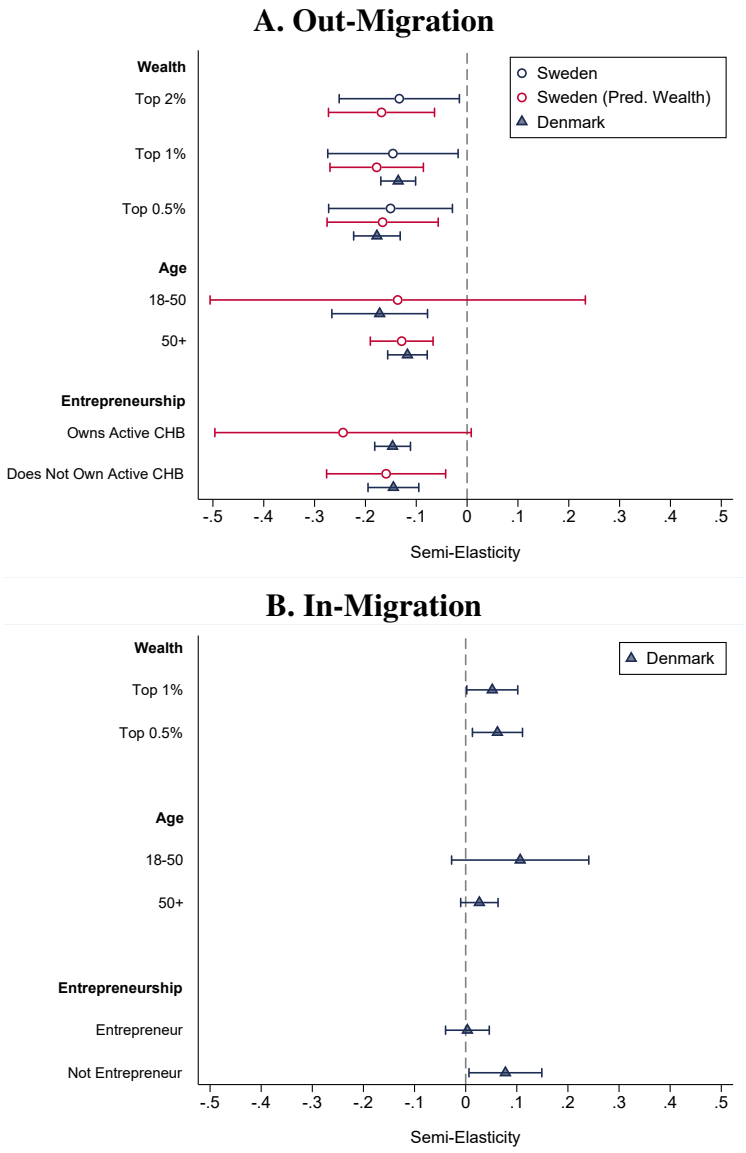
Notes: This figure shows in-migration, out-migration and net-migration rates by level of total household net wealth in Sweden (Panel A) and total household net wealth in Denmark (Panel B). We compute those statistics during the years in which the wealth tax was still in place in each country, which corresponds 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**



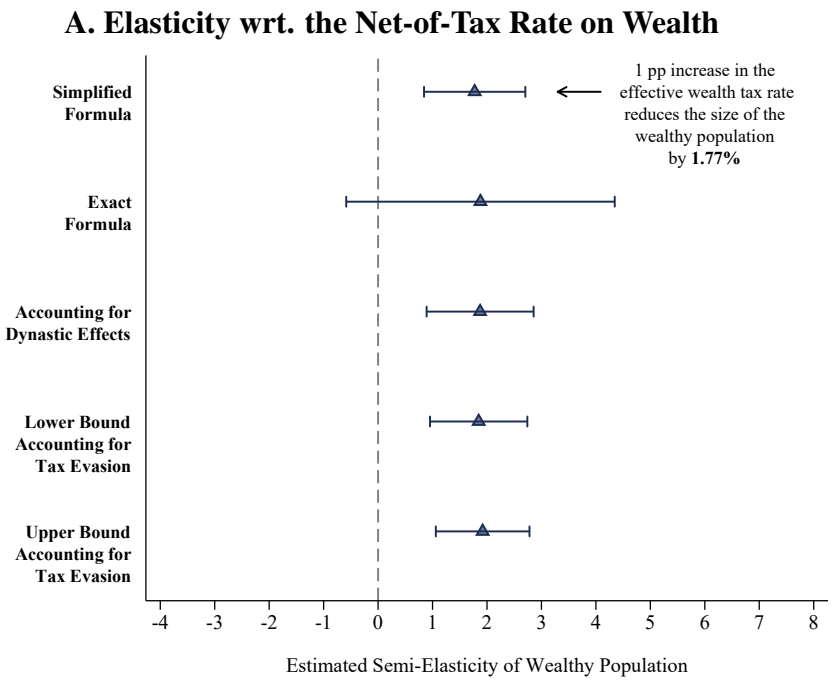
Notes: Panel A reports the evolution of wealth tax rates before and after the repeal of the wealth tax in Sweden. The dotted black line displays the evolution of the statutory marginal tax rate on wealth above the exemption threshold between 2001 and 2013. We show the corresponding evolution of the average 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.

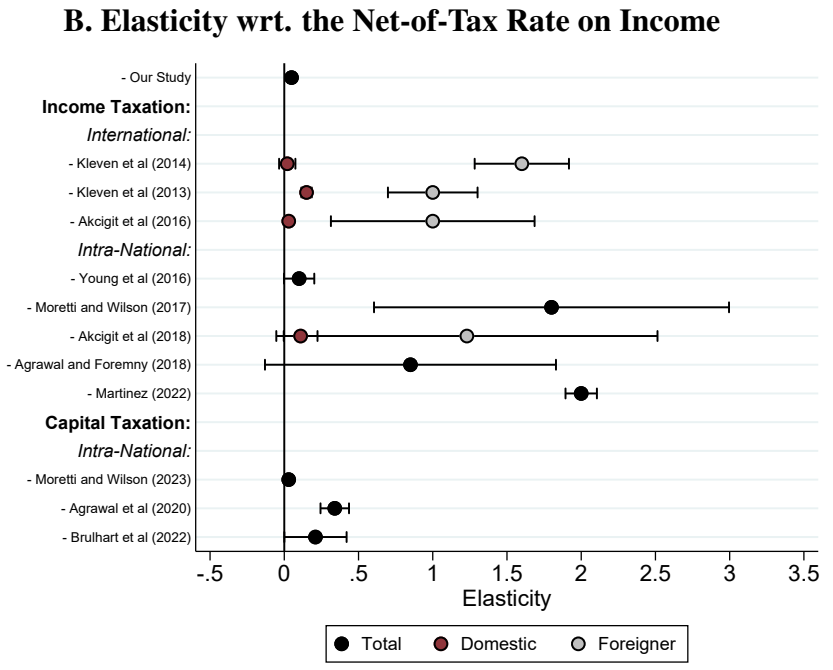
Figure 4: **Semi-Elasticities of Migration Flows**



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).

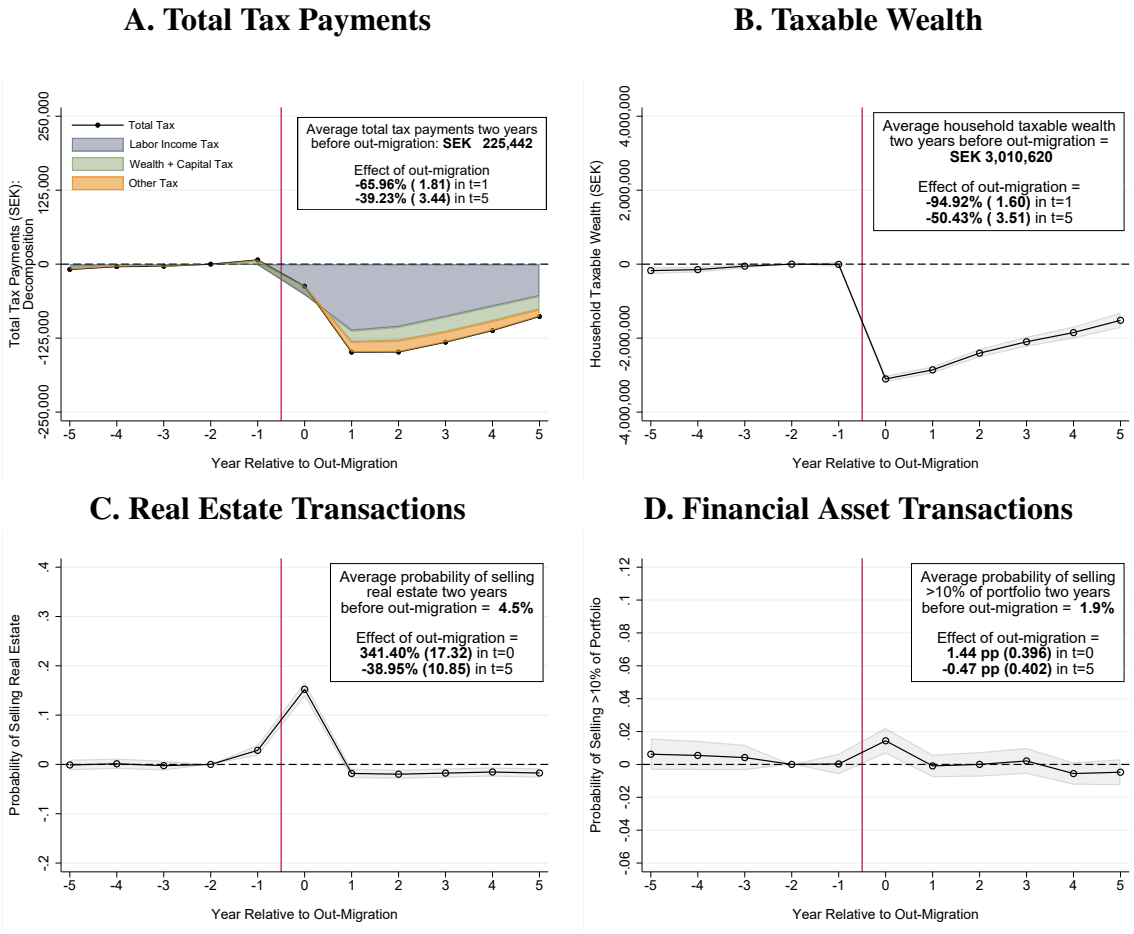
Figure 5: **Elasticities of the Stock of Wealthy Individuals**





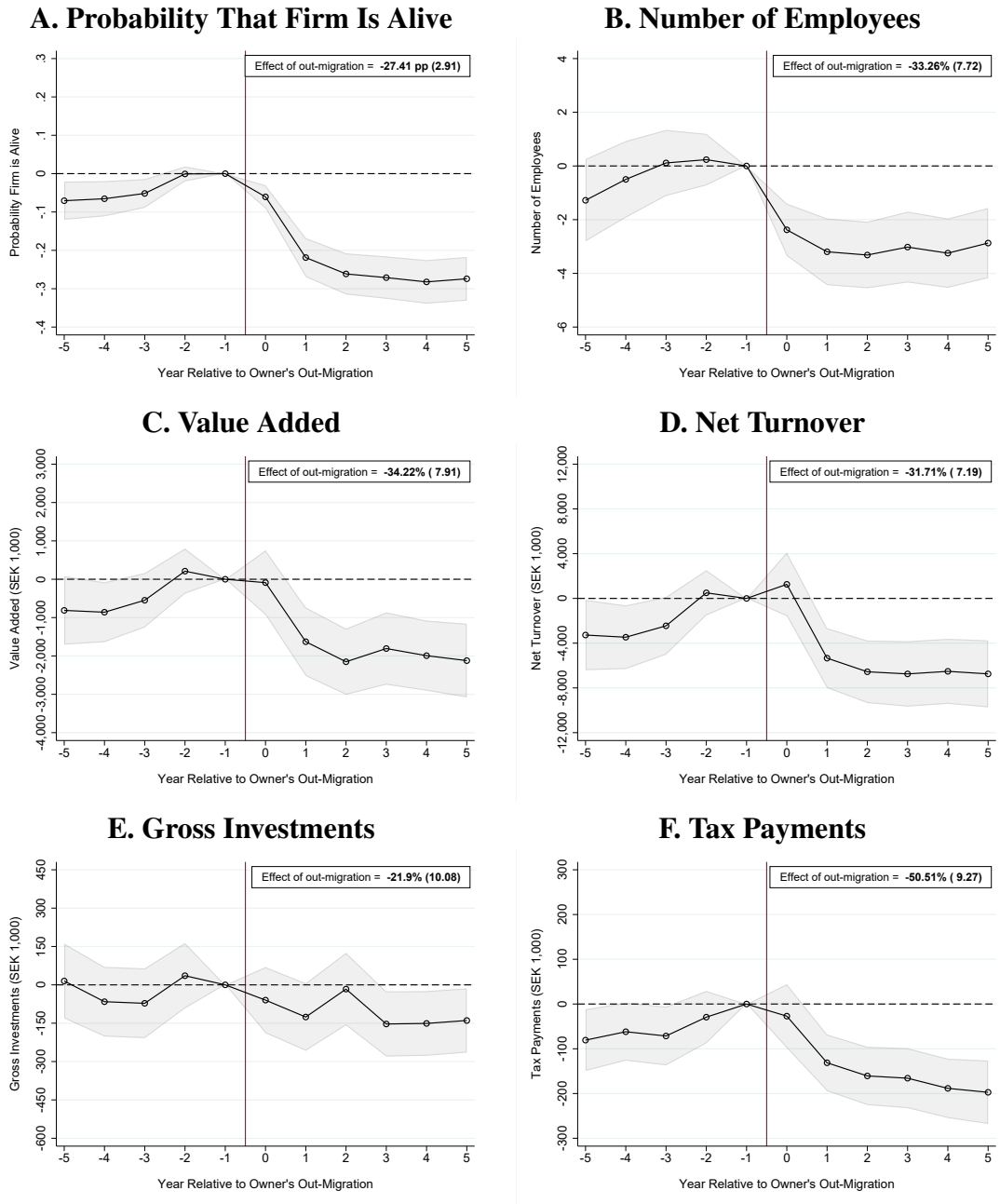
Notes: Panel A displays the effects of a one percentage point increase in the net-of-tax rate on wealth on the steady-state *stock* of the population of wealthy individuals. We cumulate the estimates of migration flows semi-elasticities showed in Figure 4 over time building on a model detailed in Appendix 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.

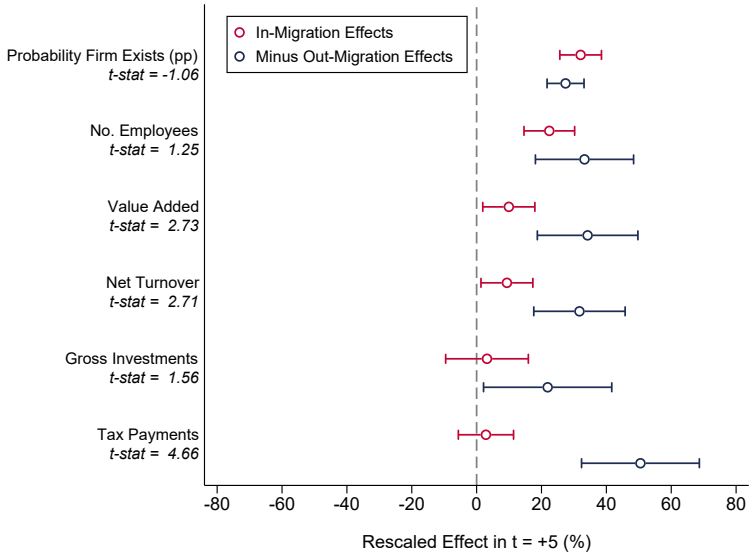
Figure 7: **Effects of Wealthy Out-Migration on Closely-Held Businesses**



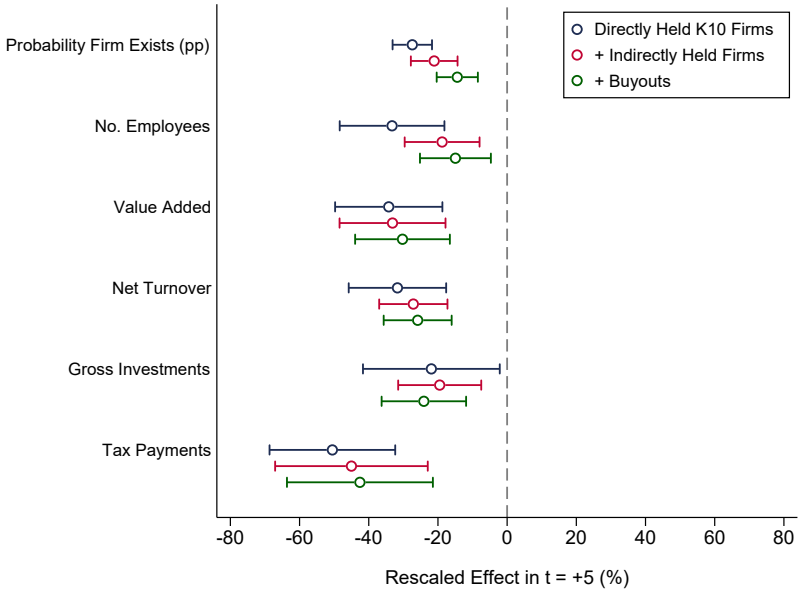
Notes: This figure shows the effects of wealthy owners' out-migration events on firm-level outcomes. We focus on out-migration events occurring between 2001 and 2007, with wealth ranks drawn from 2000-2006, when the wealth tax was still in place in Sweden. The sample includes active closely-held businesses 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

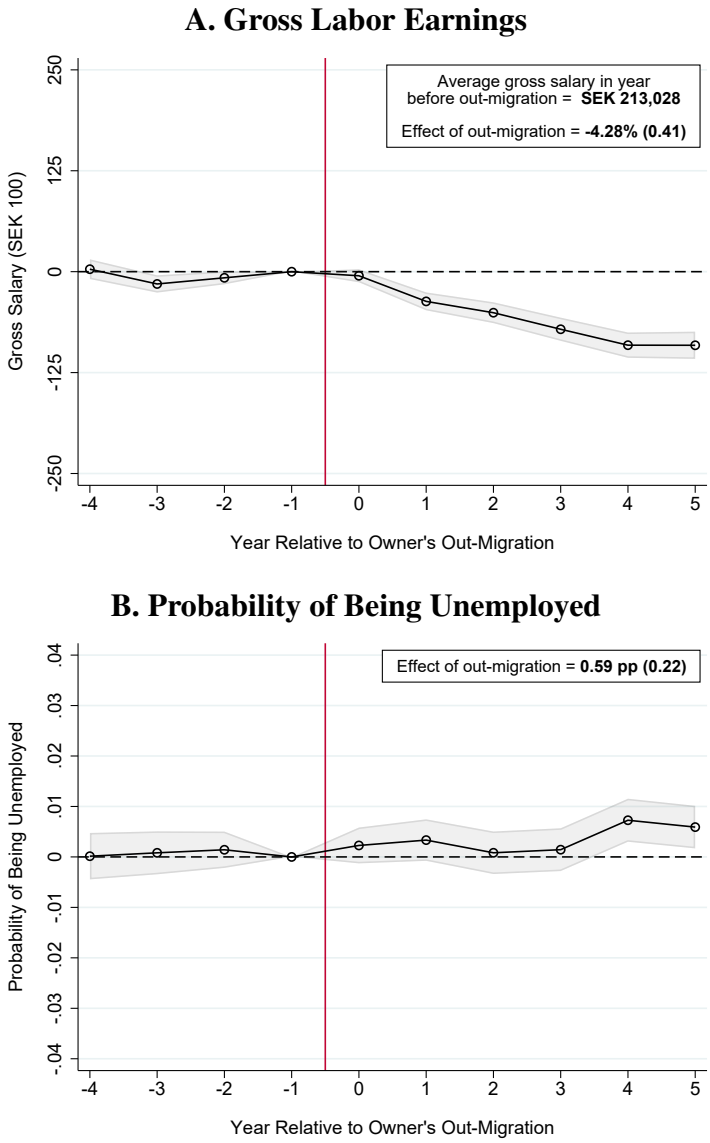


B. Accounting for Indirectly Held Firms and Buy-Outs



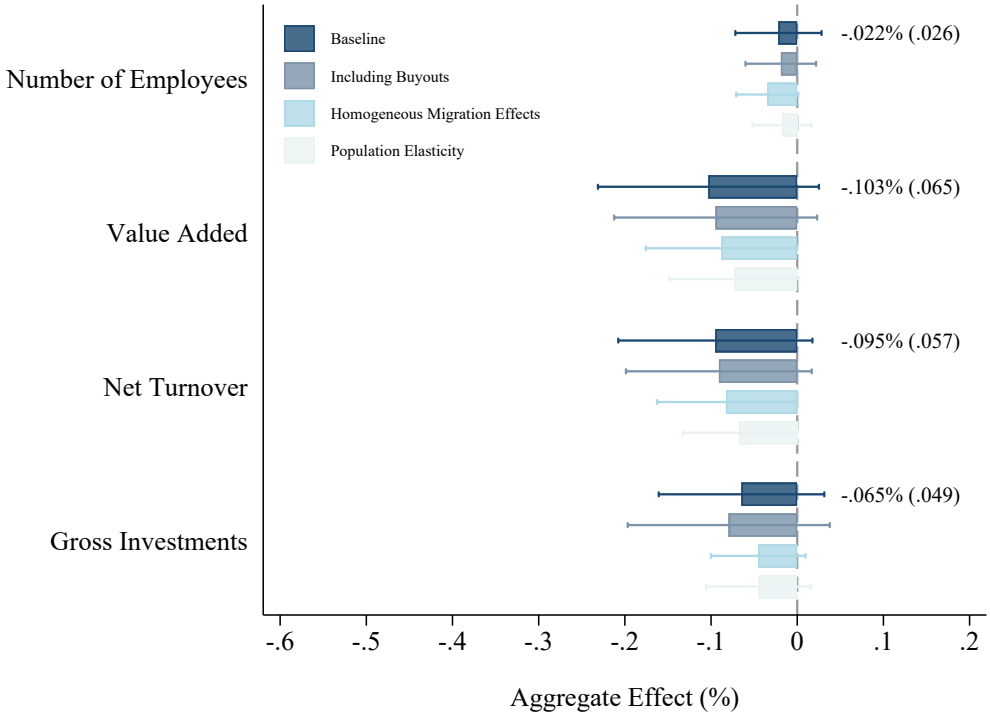
Notes: In Panel A, we study the effects of wealthy owners' in-migration and out-migration events on firm-level outcomes. We study in-migration events during the period 1999-2006, when a wealth tax was still in place in Sweden. The sample includes active closely-held businesses directly owned by individuals 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 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.

Table 2: **Test of Equality of Out-Migration Effects in Event Studies Estimated Before and After the Repeal of the Wealth Tax**

Outcome	Percentage Effects				
	Out-Migration: *Pre-Abolition*	Out-Migration: *Post-Abolition*	T-Statistic	LATE for Compliers: *Simple Diff.*	LATE for Compliers: *Double Diff.*
	(1)	(2)	(3)	(4)	(5)
A. Individual-level Outcomes					
Sample of individuals in the top 2% of net worth					
Total Tax Payments	**-46.43%**	**-40.22%**	**−2.26****	**-60.92%**	**-58.37%**
	(1.87)	**(2.00)**		**(7.80)**	**(7.98)**
Income Tax Payments	**-50.57%**	**-41.19%**	**−3.27*****	**-72.45%**	**-63.50%**
	(2.02)	**(2.04)**		**(8.25)**	**(8.62)**
Labor Income Tax Payments	**-49.85%**	**-40.55%**	**−3.33*****	**-71.54%**	**-64.90%**
	(1.96)	**(1.99)**		**(8.02)**	**(8.38)**
B. Firm-level Outcomes					
Sample of firms with owner in the top 2% of net worth					
Prob. Firm Is Alive (pp)	-20.10%	-17.63%	-0.51	-22.44%	-16.51%
	(3.37)	(3.53)		(7.36)	(9.16)
Number of Employees	-14.51%	-13.59%	-0.10	-15.38%	-7.31%
	(4.70)	(7.43)		(11.52)	(12.25)
Value Added (SEK 1,000)	-26.91%	-17.03%	-1.06	-36.25%	-31.88%
	(6.45)	(6.71)		(14.05)	(14.59)
Net Turnover (SEK 1,000)	-24.98%	-15.68%	-1.13	-33.77%	-28.97%
	(4.81)	(6.67)		(11.26)	(11.87)
Gross Investments (SEK 1,000)	-18.16%	-10.85%	-0.83	-25.07%	-26.25%
	(6.75)	(5.68)		(14.19)	(15.19)
Tax Payments (SEK 1,000)	-35.98%	-22.77%	-0.93	-48.47%	-47.55%
	(9.86)	(10.25)		(21.48)	(21.79)

Notes: This table compares the effects of out-migration events of wealthy entrepreneurs (in the top 2% of household net wealth distribution) in Sweden, before and after the repeal of the wealth tax, for firm and individual-level outcomes. For each outcome, we present parameter β_5 estimated from Equation 7 in percentage terms, focusing separately on out-migration events pre-abolition, from 2001-2006 (Column (1)), and post-abolition, from 2007-2012 (Column (2)). Column (3) reports the t-statistic of a test of equality for those rescaled estimates. Outcomes whose equality can be rejected at the 0.05 significance level are shown in bold. For the firm outcomes, we focus on active firms which are either directly or indirectly held by wealthy out-migrants. We winsorized the top 5% and bottom 1% of all outcomes (except for number of employees, which we winsorized only the top 5% of). As we need to estimate out-migration impacts post-abolition, we use predicted wealth to define exposure to the reform here. In Columns (4) and (5), we compute the LATE for the marginal out-migrants: the treatment effects for the wealthy who would have moved before the reform, but not after it, using both strategies discussed in Appendix VI.4. Column (4) shows the computed LATE using the pre vs post abolition difference in treatment effects. We call this estimate the simple difference estimate (β^m from Appendix Equation 17). In Column (5), we account for the fact that the pre vs post comparison may capture aggregate dynamics in treatment effects that may not be related to the wealth tax reform. We therefore use individuals who are not affected by the wealth tax reform as a control group, to estimate in *double difference* the LATE for the wealth tax compliers ($\beta^{\tilde{m}}$ from Appendix Equation 19).

APPENDIX

for "Taxing Top Wealth: Migration Responses and their Aggregate Economic Implications"

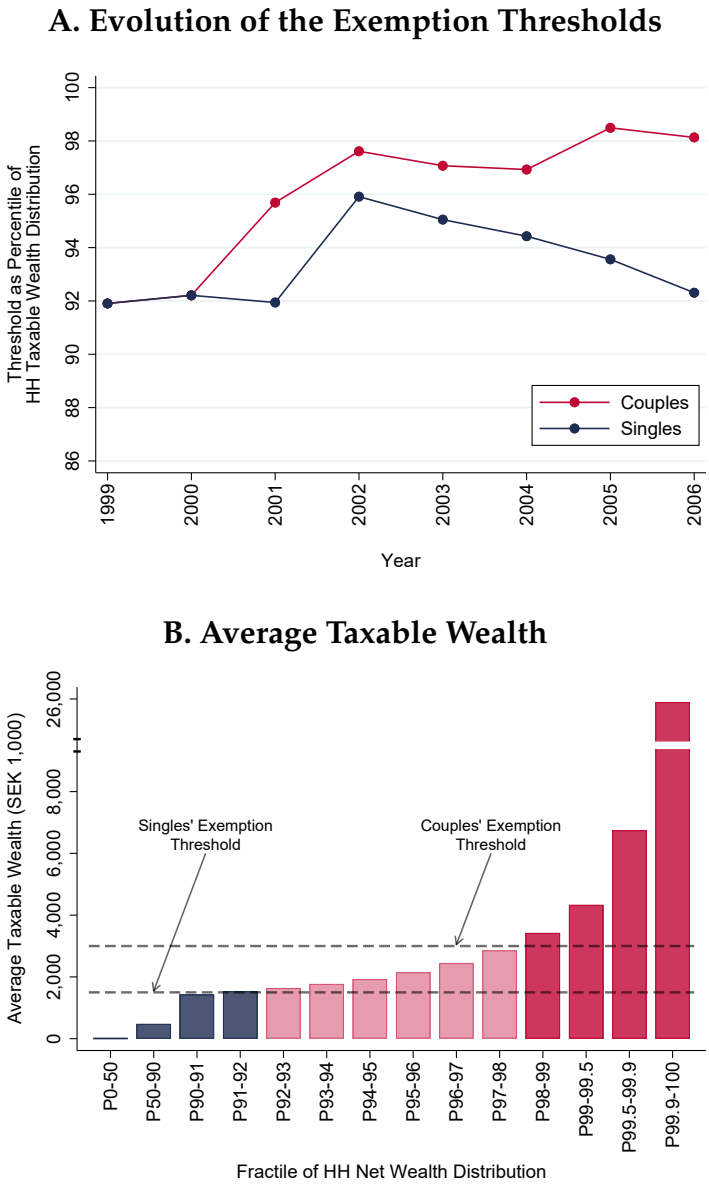
by Katrine JAKOBSEN, Henrik KLEVEN, Jonas KOLSRUD, Camille LANDAIS and Mathilde MUNOZ

Contents

I. Additional Institutional Details

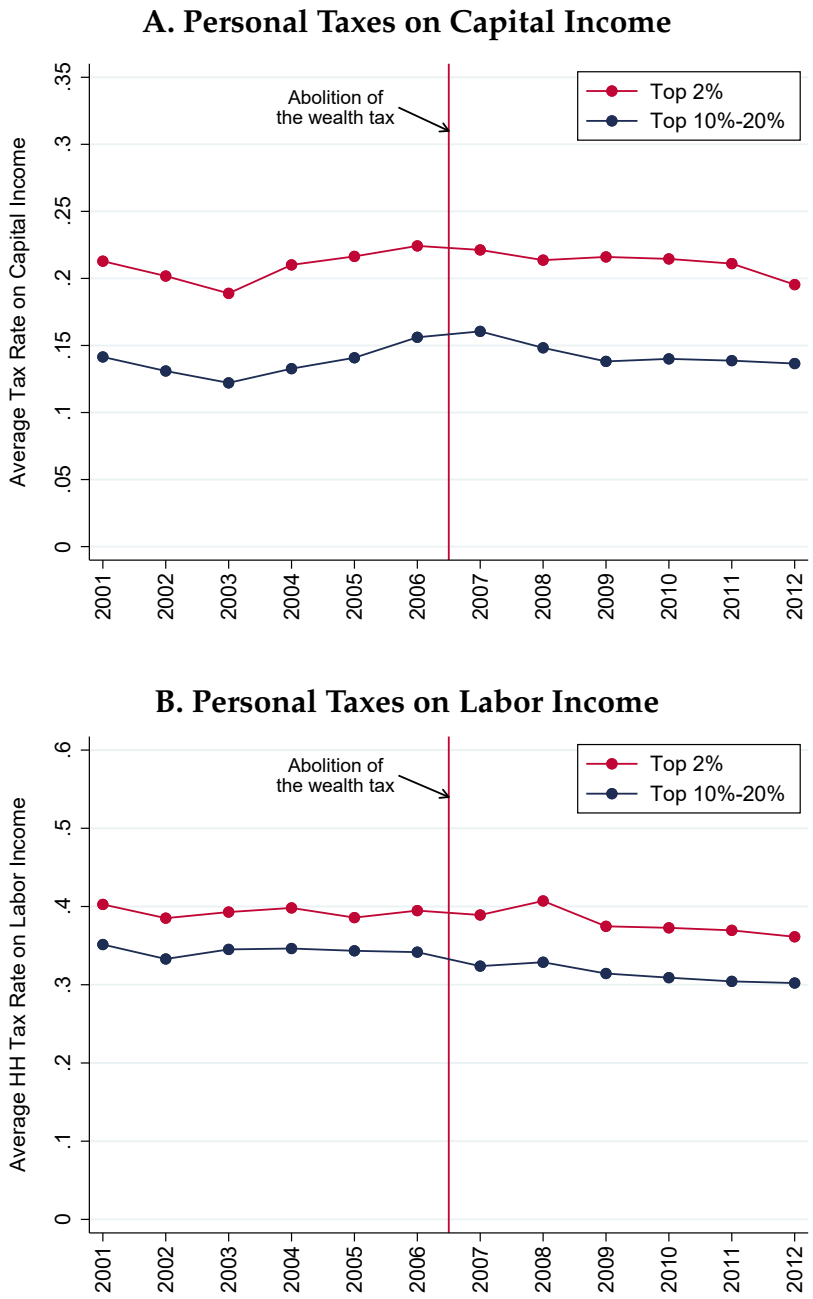
I.1. Additional Institutional Details: Sweden

Figure I.1: **Exemption Threshold for the Wealth Tax in Sweden**



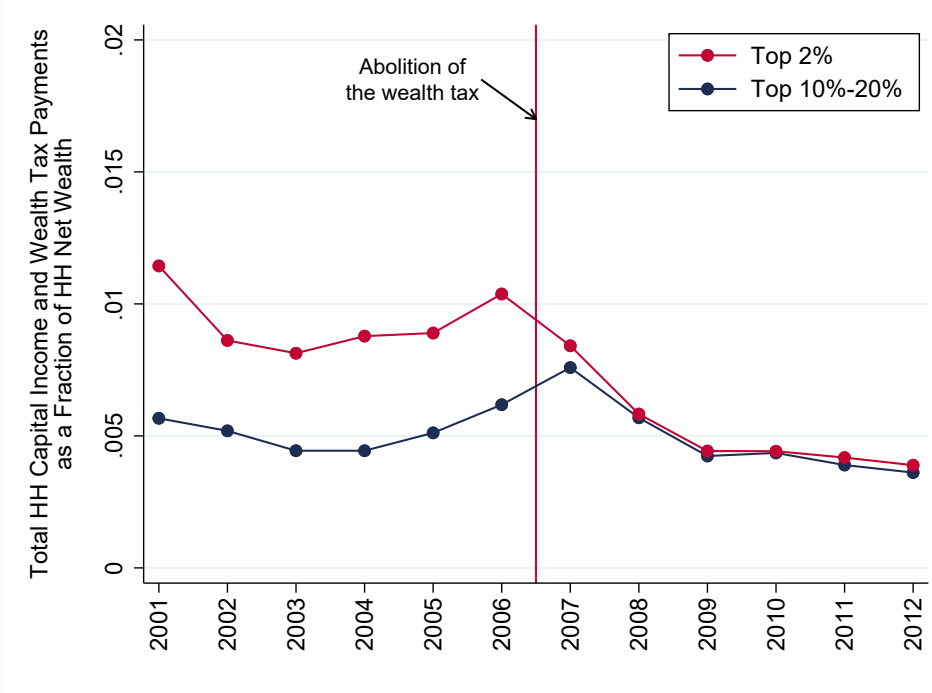
Notes: Panel A shows the evolution of the exemption threshold in the Swedish wealth tax as a function of the household taxable wealth distribution. The red line refers to the wealth tax exemption threshold set for couples, while the dark blue line refers to the wealth tax exemption threshold set for single individuals. Panel B shows the average household taxable wealth in 2006. We rank households based on their total household net wealth in 2006, and then plot the average taxable wealth by fractile of household net wealth. The dashed black lines refer to the wealth tax exemption thresholds for singles and couples that same year.

Figure I.2: **Effective Tax Rates on Capital and Labor Income**



Notes: This figure plots the average effective tax rate on capital income (Panel A) and on labor income (Panel B) for households in the top 2% of net wealth (wealthy households subject to the wealth tax, red line) and for households in the top 20-10% of net wealth (wealthy households not subject to the wealth tax, blue line). The tax rate on capital income (Panel A) is computed as the ratio of household capital income tax payments over household capital income each year. The top 1% of capital income tax payments among individuals in the top 20% of household net wealth was winsorized. The tax rate on labor income (Panel B) is computed as the ratio of household labor income tax payments over household taxable income. The vertical red line denotes the year where the wealth tax was repealed in Sweden.

Figure I.3: **Effective Tax Rate on Capital Income and Wealth**



Notes: This figure plots the average effective tax rate on the sum of capital income and wealth for households in the top 2% of net wealth (wealthy households subject to the wealth tax, red line) and for households in the top 20-10% of net wealth (wealthy households not subject to the wealth tax, blue line). We divide the sum of household tax payments on capital income and wealth by total household net wealth. The top 1% of capital income tax payments among individuals in the top 20% of household net wealth was winsorized. The vertical red line denotes the year where the wealth tax was repealed in Sweden.

I.2. Wealth Taxation in Denmark

An annual progressive wealth tax was implemented in Denmark from 1903 to 1997 and applied to Danish tax residents with net taxable wealth above an exemption threshold. This exemption threshold was always above the 98th percentile of the wealth distribution during the period we study.

Tax Base Taxable wealth was defined as the total net wealth of households, excluding pension wealth. Taxable wealth components included cash, deposits, bonds, equities, housing, large durables and business assets, net of any debts. Only taxable wealth above a given exemption threshold, whose evolution is shown in Panel B of Figure I.1, was included in the wealth tax base.

The definition of the geographic scope of the wealth tax base was the same in Denmark and Sweden. Danish residents were taxed on their worldwide assets, including financial and non-financial assets held abroad, while non-residents were only taxed on their assets held in Denmark.

From 1980 onwards, the value of certain assets was reduced when calculating taxable wealth. Specifically, the value of physical business assets was reduced by 20%, while the value of large forests was reduced by 30%.[1] The relief for physical business assets was increased to 25% in 1982 and 30% in 1983. From 1986 onwards, the relief for both business assets and forests was increased to 40%.

Finally, there was also a tax credit for the wealth tax through a tax ceiling mechanism capping the amount of wealth tax owed by taxpayers as a fraction of their taxable income. This mechanism varied for couples and single individuals and is described in Jakobsen et al. (2020).

Tax Rates Like in Sweden, the Danish wealth tax had a simple two-bracket structure. Wealth in Denmark was taxed at a flat rate of 2.2% above the exemption threshold until two reforms in the late 1980s and 1990s. After 1989, the tax rate was reduced to 1%, while in 1997 the wealth tax was repealed.

Reporting and Enforcement Most of the assets and liabilities were reported by third-parties to the Danish government. The value of bank deposits was reported by banks and the value of listed stocks and bonds was reported by financial institutions. The government used land and real estate registries to record non-financial assets. All other wealth

[1] Since forests were also considered a physical business assets, their taxable value was reduced by 50%.

components had to be self-reported by taxpayers.

II. Data

II.1. Prediction Model for Wealth in Sweden

After the repeal of the wealth tax, reporting requirements changed, and we do no longer observe after 2007 the same comprehensive components of household wealth. Certain elements such as liquid bank accounts or listed stocks are missing, but we still observe many wealth components, such as real estate (through real estate registers) and closely-held business assets. To construct a consistent measure of wealth before and after 2007, we build a prediction model of household total net wealth that we train for the period pre-2007, and then use to predict net wealth after 2007. In this section, we provide all the details regarding our prediction model.

Overview of the Approach Our approach leverages two important features of the data. First, we continue to observe many components of household net wealth post-2007. Second, for the elements that we do not observe any more, we have precise information on the past value of assets, and on all income flows, which are both related in an accounting sense to the current value of the assets through iterating the law of motion of household wealth.

To understand the approach, we start by splitting assets between wealth W^o from asset classes o that are observable throughout the period, and wealth W^u from asset classes u that are only observed up to 2007.

$$\underbrace{W_t}_{\text{Net Wealth}} = \underbrace{W_t^o}_{\text{Assets observed pre and post 2007}} + \underbrace{W_t^u}_{\text{Assets not observed after 2007}} \tag{1}$$

We can then use the law of motion of wealth to break down W_t^u between capitalized past wealth W_{t-1}^u and net active savings/dissavings in assets of class u:

$$W_t = W_t^o + \underbrace{(1 + r_t^u) \cdot W_{t-1}^u}_{\text{Capitalized Past Wealth}} + \underbrace{p_t^u \cdot \Delta q_t^u}_{\text{Active Savings in Assets u}} \tag{2}$$

We then use the accounting identity of the household budget constraint, which imposes that the sum of net active savings in all classes of assets must be equal to the sum of all earnings and inheritances flows, net of all taxes and transfers received, minus household consumption.

$$p_t^u \cdot \Delta q_t^u + p_t^o \cdot \Delta q_t^o = \underbrace{E_t}_{\text{Earnings}} + \underbrace{I_t}_{\text{Inheritance}} + \underbrace{T_t}_{\text{Taxes \& Transfers}} - \underbrace{C_t}_{\text{Consumption}} \tag{3}$$

Combining (2) and (3), we get:

$$W_t \;=\; W_t^o + (1 + r_t^u) \cdot W_{t-1}^u \;\; + E_t + I_t + T_t - C_t - p_t^o \cdot \Delta q_t^o \tag{4}$$

Iterating the above identity, we get after X iterations:

$$W_t \;=\; W_t^o + W_{t-X}^u \prod_{j=t-X}^{t} (1 + r_j^u) \;\; + \sum_{k=t-X}^{t} (E_k + I_k + T_k - C_k - p_k^o \cdot \Delta q_k^o) \tag{5}$$

Note that equation 5 is an accounting identity. Which means that if all the elements of the right-hand side of the identity are observed, the exact value of wealth can be computed. In practice, we observe, thanks to the rich administrative information available in Sweden, many of the elements from the right-hand side:

- Observable wealth W_t^o:
  We observe continuously the following elements of wealth: real estate assets via the real estate register; closely-held business assets directly owned, through the K10 registry data, and indirectly owned through the Serrano database.

- Past wealth W_{t-X}^u:
  We can observe all asset classes until 2007. After this date, the following financial assets are no longer observed: mutual funds, stocks in listed firms, bonds, bank holdings and balance in other liquid accounts. We use the fact that we can observe these assets up until 2007, and that we also observe average rate of returns r_j^u on these assets.

- Cumulated flow of past earnings $\sum_{k=t-X}^{t} E_k$:
  We measure past disposable earnings recorded in the LISA dataset from 1990 to 2017. Importantly LISA accounts for all potential sources of labor income.

- Cumulated flow of past inheritances $\sum_{k=t-X}^{t} I_k$:
  We observe all inheritance flows for the period 2001 to 2005 via the inheritance registry BELINDA.

- Cumulated flow of past taxes and transfers $\sum_{k=t-X}^{t} T_k$:
  We observe all tax payments in the IoT tax registry, and all transfer payments received from LISA.

- Cumulated active savings in asset classes o $\sum_{k=t-X}^{t} p_k^o \cdot \Delta q_k^o$:
  For these assets, the registers record both prices and quantities, and all related transactions, which means that we can compute active savings/dissavings in these asset classes.

Implementation While we observe many of the elements of equation (5), we cannot simply implement this accounting identity to measure wealth post 2007 because: (i) we do not observe inheritances flows for the period pre-2001, and post-2005 and (ii) most importantly, we do not observe the cumulated flow of consumption $\sum_{k=t-X}^{t} C_k$. However, we have access to rich additional information that can serve as useful predictors of these unobserved inheritance and consumption flows.

- For inheritance flows: we observe parental wealth as well as the age of parents and the number and age of siblings, which allows us to predict the likelihood of receiving an inheritance using the observed inheritance flows over the period 2001-2005.

- For consumption flows: we observe some durable consumptions, such as cars (through the FORDON car register), and a rich set of demographics that correlate with consumption patterns (age, household structure, place of residence, etc).

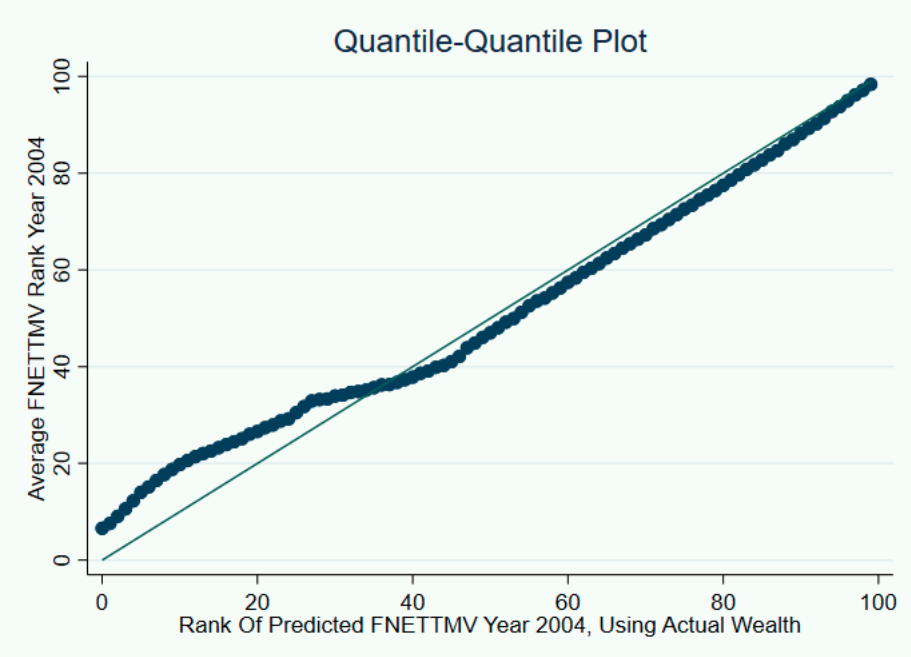
- Furthermore, from equation (3), we know that consumption can be retrieved as residual from all flows of earnings, taxes/transfers and inheritances and active savings and dissavings. In practice we measure throughout the period all active savings and dissavings in asset classes o and we also measure active dissavings in asset classes u (e.g. dividends perceived, realized capital gains/losses, etc), but not active savings in asset classes u.

As a consequence, our methodology relies on building an ensemble classification model that exploits identity (5). For our model, we use all elements of the identity that we observe. And for the elements that we do not strictly observe in identity (5) (i.e. some inheritance flows and consumption flows), we complement the set of covariates of the model with all the various proxies and potential predictors described above: (i) parental wealth, age of parents and number and age of siblings, (ii) demographic characteristics of the household, (iii) observed durable consumption in cars, (iv) cumulated flows of active dissavings in asset classes u (e.g. dividends perceived, realized capital gains/losses, etc).

More specifically, we train random-forests on 10% of a random sample of the Swedish population and we then classify the population in groups of predicted net wealth. We

choose to fix $X = 7$ for the model. This means that we can then use our model to predict wealth from 2000 to 2014.

Figure II.1: **Predicting Wealth: Accuracy of our Model**



Notes: This figure plots the average rank of taxable wealth in 2004 based on the rank of predicted taxable wealth the same year using our prediction model.

Validation and Model Accuracy Importantly, our prediction model performs exceptionally well, as shown in Appendix Figure II.1, and much better than capitalization methods often used to proxy wealth in the absence of proper administrative registers on wealth (Saez and Zucman, 2016).

Note that because the prediction model relies on past data, it is a good model to predict wealth for people about to move (out-migration effects) but does not work for individuals who just moved in (in-migration rates) for whom we do not have info on past wealth, parental wealth, past income, etc.

II.2. Computation of Effective Tax Rates for Entrepreneurs

We compute average effective tax rates on wealth for entrepreneurs. To do so, we need to obtain a valuation of private business assets. We value private business assets by us-

ing the balance-sheet measure of profits before taxes (EBIT) from our firm registry data. We apply a liquidity discount factor of 10%, meaning that we capitalize 0.9 × EBIT for the wealthy who control unlisted firms. For smaller businesses with less than 50M in profits, we account for the human capital contribution of profits estimated in Smith et al. (2019): we only capitalize 0.9 × 0.25 × EBIT . We then capitalize firms' profits. We use Smith et al. (2022) estimates for entrepreneurs' returns (10.5%) to infer the value of private business assets controlled by the rich in Sweden. The implicit assumption is that private business assets of the wealthy had the same returns in the U.S. and Sweden during the period we study. We thus obtain an estimate of private business assets wealth for each wealthy entrepreneur, each year. We then compute the implied effective tax rate on wealth, by dividing effective wealth tax payments by the total of net wealth and valued private business assets.

II.3. Wealth and Migration in Denmark

In Denmark, wealth and income registries are based on tax return data from the Danish Tax Agency (SKAT). Wealth levels are stated by the end of the year, and most of the components measuring wealth are reported by third-parties. For instance, cash value of real estate, deposits, bonds, and shares are third-reported. Until 1997, we also observe self-reported components that are subject to auditing by the tax authorities, such as the self-reported value of stocks (listed and non-listed), self-reported value of durables (cars, boats and caravans) and the self-reported value of share certificates for housing cooperatives, premium bonds and cash-holdings. Liabilities include third-party reported values of debt in financial institutions, mortgage credit debt, credit and debit card debt, student debt, and all other liabilities such as unpaid taxes which are not deposited. After 1997, the asset statement is based solely on third-party reporting. Unlike in Sweden, we do not observe closely-held businesses or unlisted shares held by taxpayers neither before or after the wealth tax repeal. More information of the Danish wealth tax data can be found in Jakobsen et al. (2020).

These data are linked with a longitudinal dataset containing rich information on all earnings, transfers and demographics from 1989 to 2006, which include information on age, family situation or occupation.

Like in Sweden, we have access to detailed citizenship and migration information such as daily dates of entry and exit in the country. Individuals working in Denmark must obtain a personal identification number (CPR) to pay taxes, rent an apartment, or register with health insurance. The application for a CPR contains detailed questions about citizenship, country of origin, and date of entry in Denmark. Individuals must also register their move to the CPR office to stop paying taxes in Denmark.

A summary of the different variables and datasets in Denmark and Sweden is presented in Table II.1. In Sweden, we have better information on unlisted companies, and will therefore focus on this country for our analysis of the effects of out-migration on firm-level outcomes.

Table II.1: **Sweden and Denmark**

	Sweden	Denmark
Panel A. Wealth Taxation		
Period of implementation	1910-2007	1903-1997
Exemption Threshold (percentile of wealth distribution)	96th-98th	98th
Tax ceiling	Y	Y
Business Assets	Exempted	Taxed
Number of brackets	2	2
Maximum Top MTR in the period	1.5%	2.2%
Revenue	0.16% of GDP in 2006	0.06% of GDP in 1996
Panel B. Data Availability		
Migration (in or out) date	1990-2019	Y
Duration of stay abroad (days)	1986-2019	Y
Closely-Held Businesses	2000-2017	N
Income and transfers	1990-2017	Y
Inheritance	2001-2005	Y

Notes: This table summarizes the data and institutional environments for the wealth tax in Sweden and Denmark.

II.4. Identifying Ownership Links in Serrano Dataset

Serrano is a firm-year level dataset, whose population comprises all the firms registered in Sweden between 1998 and 2021. It includes general company information retrieved from Statistics Sweden, data on financial statements, bankruptcy and mergers compiled from the Swedish Companies Registration Office (Bolagsverket), and group data from the Bisnodes group registry. For our analysis, we focus on the group data.

The group data are at the parent-subsidiary-year level. For each company ("subsidiary firm") registered in Sweden and whose ownership shares are held at least partially by other firms ("parent firms"), the group data list its parent companies and the share of the subsidiary they own in every year. We can also see links between associated companies and parent companies, where the ownership share is between 20 and 50%. The reason for the cutoff is that as a parent firm owning a minority stake at 20% or more gives certain rights vis-à-vis the majority owner.[2] We thus see all ownership links between domestic firms in the SERRANO dataset, except for firms that own less than 20% of another firm. We merge this information on parent companies and subsidiaries links with our firm ownership registries. To do so, we proceed in two steps.

The first step consists in reconstructing the entire paths of firm links using the group data in Serrano. For example, assume the following group structure: firm A owns 50% of firm B, firm B owns 100% of firm C and firm C owns 40% of firm D. In the Serrano data, this structure is decomposed into the links A-B, B-C and C-D. By merging these links sequentially, we are able to reconstruct the path A-B-C-D. When we implement this sequential merge, we also merge the ownership shares of parent firms in their subsidiaries. Once we reconstruct the entire path of links for each group of firms, we can multiply these ownership shares to compute the integrated ownership share. In the example, the integrated ownership share of firm A in firm D is 20%. In the dataset of firm links that we build, we do not only keep the final link paths (e.g., A-B-C-D) but also each intermediate link path (e.g., A-B-C, B-C-D, A-B, B-C, C-D) and the corresponding integrated ownership shares.[3] For each link path we reconstruct, we keep the first and the last firm and we define these as parent-subsidiary pairs. Two firms forming a parent-subsidiary pair

[2]For instance, the minority owner can (i) ask for a special "minority accountant" at the firm's expense to make sure the accounting is made properly, (ii) call for an extra general meeting to discuss firm issues (likely so that other minority owners can be alerted of potential wrongdoings by the majority owner), (iii) make the firm sue the CEO or board members if they have caused damage to the firm, and (iv) call on the court to make a compulsory liquidation of the firm.

[3]Since ownership cycles (e.g., firm A owns a share of firm B, which in turn owns a share of firm A) are very few in the data, we deal with them by stopping the sequential merge (e.g., we would keep links A-B and B-A but not A-B-A or B-A-B).

may be linked via multiple paths. Therefore, we sum the integrated ownership shares computed for each parent-subsidiary pair across all the paths that link them and use this as our final measure of integrated ownership of the parent firm in the subsidiary firm.

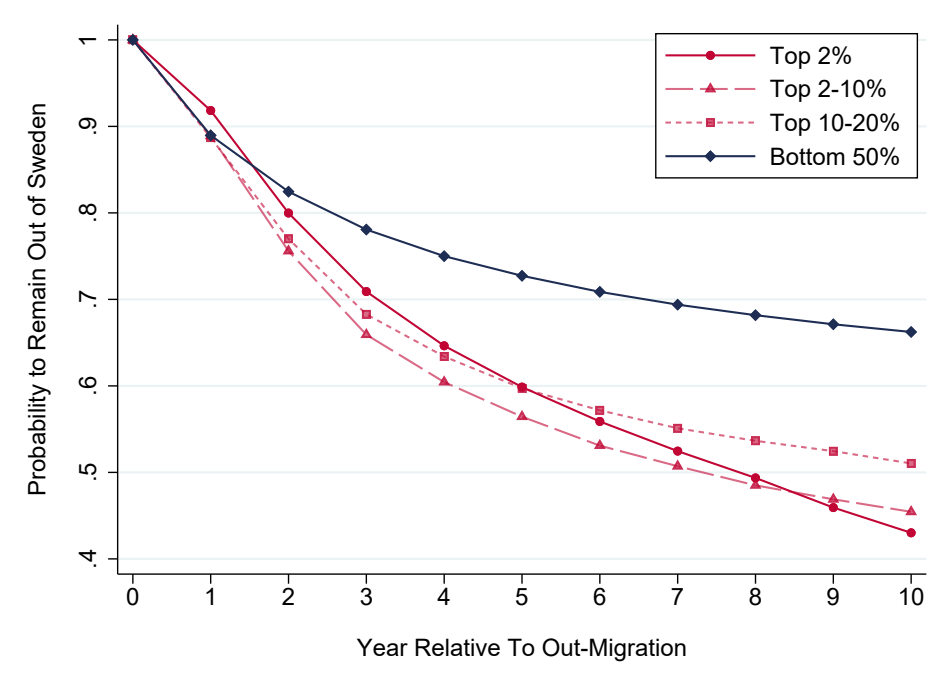
In a second step, we match the parent companies to the registry of closely-held businesses. When a parent company appears in the registry of closely-held businesses, we observe the individuals who own it. We define these individuals as the indirect owners of the subsidiary firm held by their closely-held business. The ownership share of these individuals in the subsidiary is defined by the integrated ownership share defined previously. Continuing with the example and assuming that firm A is a closely-held business, the owners of firm A are also indirect owners of firm B (with a 100% share), firm C (with a 50% share) and firm D (with a 20% share). By implementing this approach, although we can only observe the direct owners of closely-held businesses, we can observe the indirect owners of both closely-held and not closely-held businesses, provided that their parent companies are closely-held businesses. We use the integrated ownership shares to allocate employment and other economic outcomes of Swedish firms to their final owners in Sweden.

III. Additional Descriptive Evidence on Migration Patterns in Scandinavia

III.1. Duration analysis

Figure III.1 documents the duration pattern of migration events at the top of the wealth distribution. We observe the number of days spent in Sweden by each individual each year in the administrative migration register. Thus we can check that migration events reflect real mobility responses, rather than bogus changes in tax residency. In Figure III.1, we show the probability to remain outside Sweden following an out-migration event for individuals with different level of wealth. For all groups, mobility is a real response and is quite persistent over time. One year after out-migrating, the probablity to remain outside Sweden is around 90% for both wealthy and non-wealthy individuals. However, migration also appears to be transitory for a large fraction of migrants: for instance, roughly 40% of the wealthy are back after 5 years. Migration events are also less persistent among the very wealthy than for rest of the population.

Figure III.1: **Probability to Remain Out of Sweden After an Out-Migration Event**

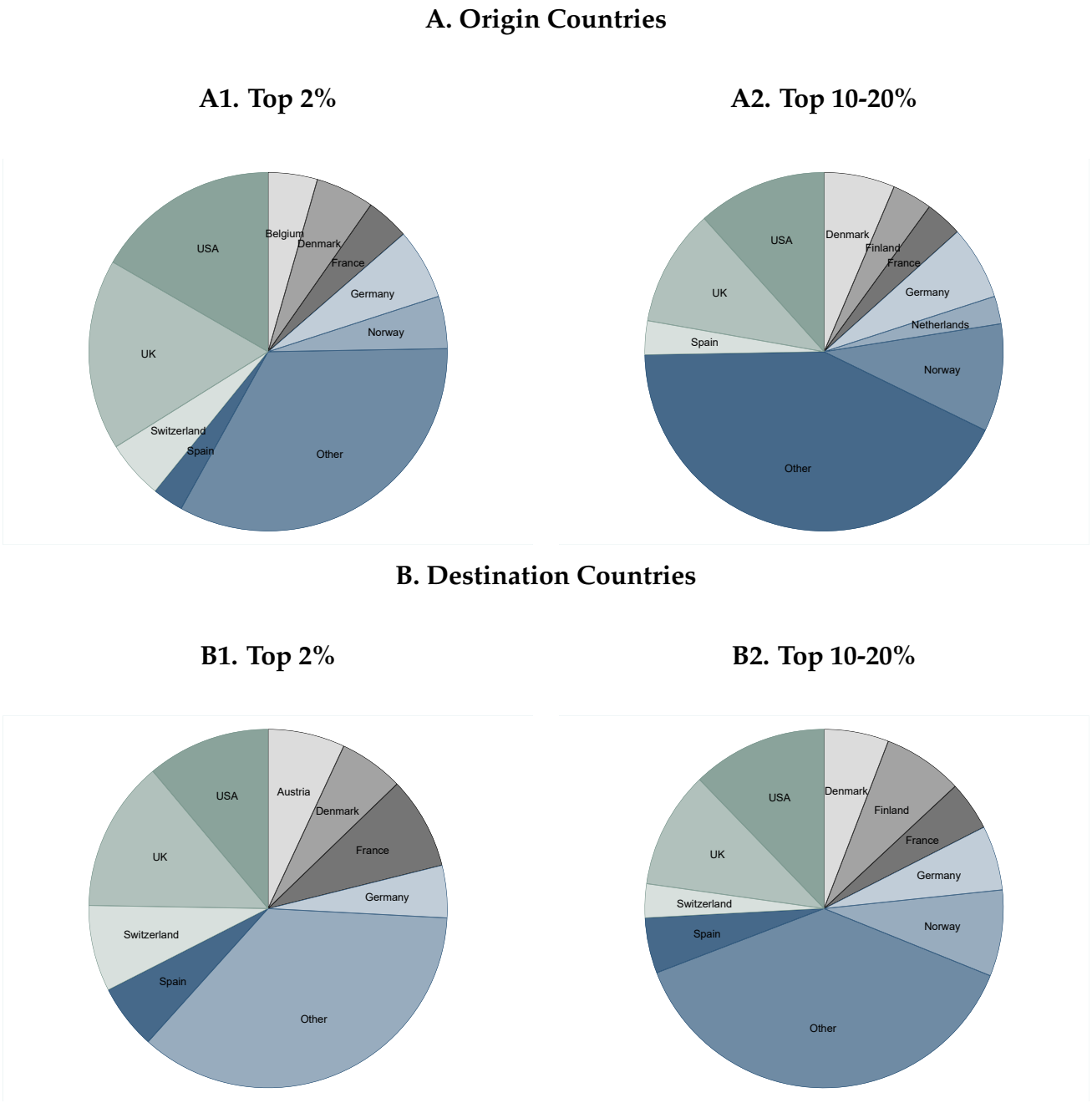


Notes: This figure shows the probability to remain out of Sweden after an out-migration event. The out-migration events considered for this plot cover the years 2000-2007. The wealth groups are based on the net wealth of individuals' household in the year before their out-migration date. Each year that follows an out-migration event, we measure the probability that the individual still lives outside Sweden, or has migrated back to Sweden.

III.2. Destination and origin countries

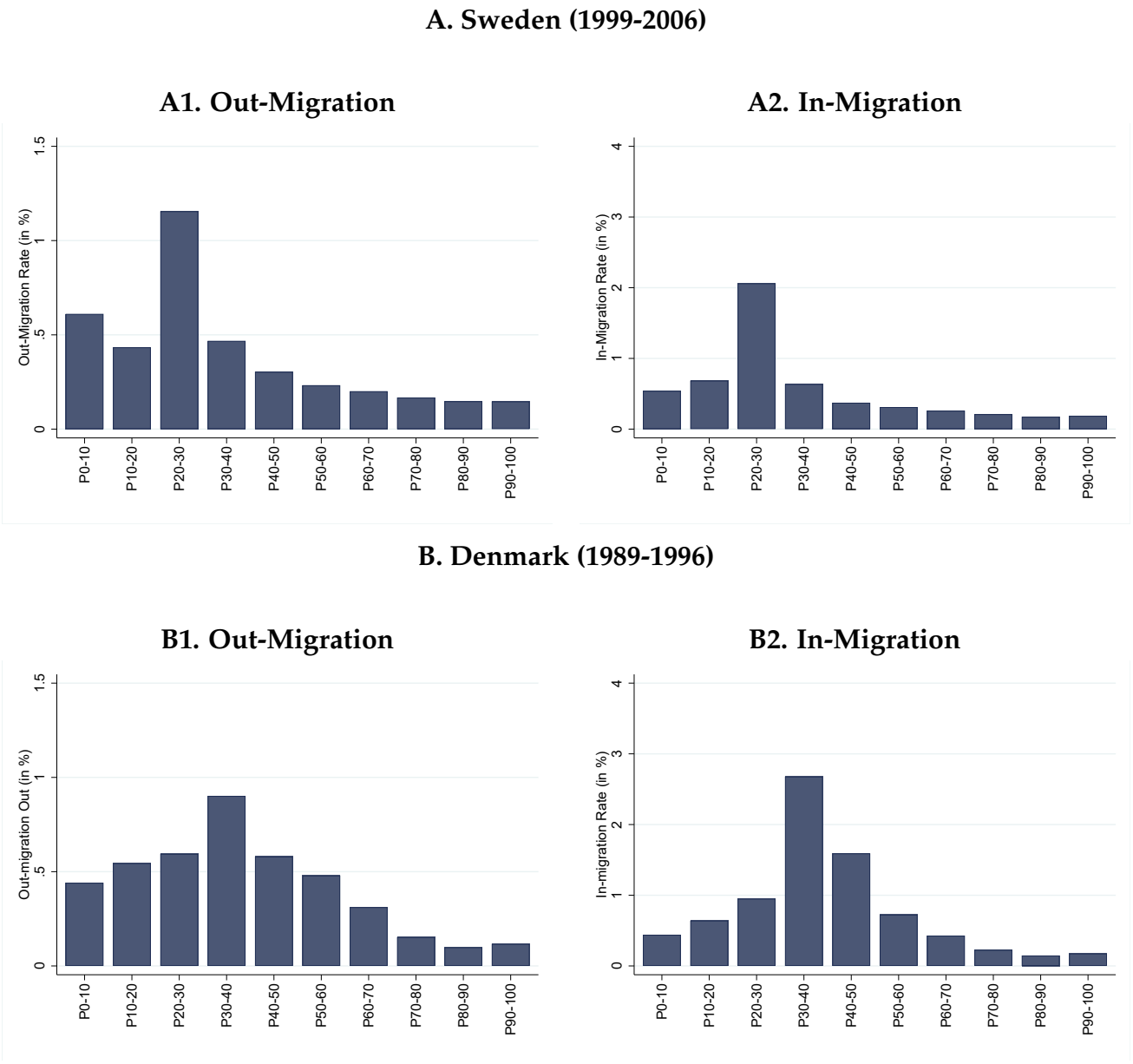
We also describe the geographic patterns of migration at the top of the wealth distribution. Figure III.2 decomposes destination and origin countries for Swedish citizens with different level of wealth before (respectively after) their migration event. The top destination for wealthy Swedes is the UK. Capital taxes are lower in the UK compared to Sweden and there has been no tax on wealth. Furthermore, Swedish individuals can benefit from the preferential "non-dom" tax status in the UK, which exempts any of their non-UK income from taxation, to the extent that it is not repatriated in the UK. The second top destination is the US. In 2005 Sweden renegotiated the 1995 tax treaty with the US. The alteration was removing the tax on dividends paid from a US subsidiary to a Swedish parent company. Sweden had already unilaterally removed this tax. Two other noteworthy destination are Switzerland, and Austria. The latter has a beneficial bilateral tax treaty exempting taxation of capital gains for Swedish expatriates. Overall, destination countries of wealthy Swedish taxpayers are predominantly lower tax countries, suggesting that geographic migration patterns are in large part driven by tax concerns.

Figure III.2: **Migration at the Top in Sweden: Main Origin and Destination Countries**



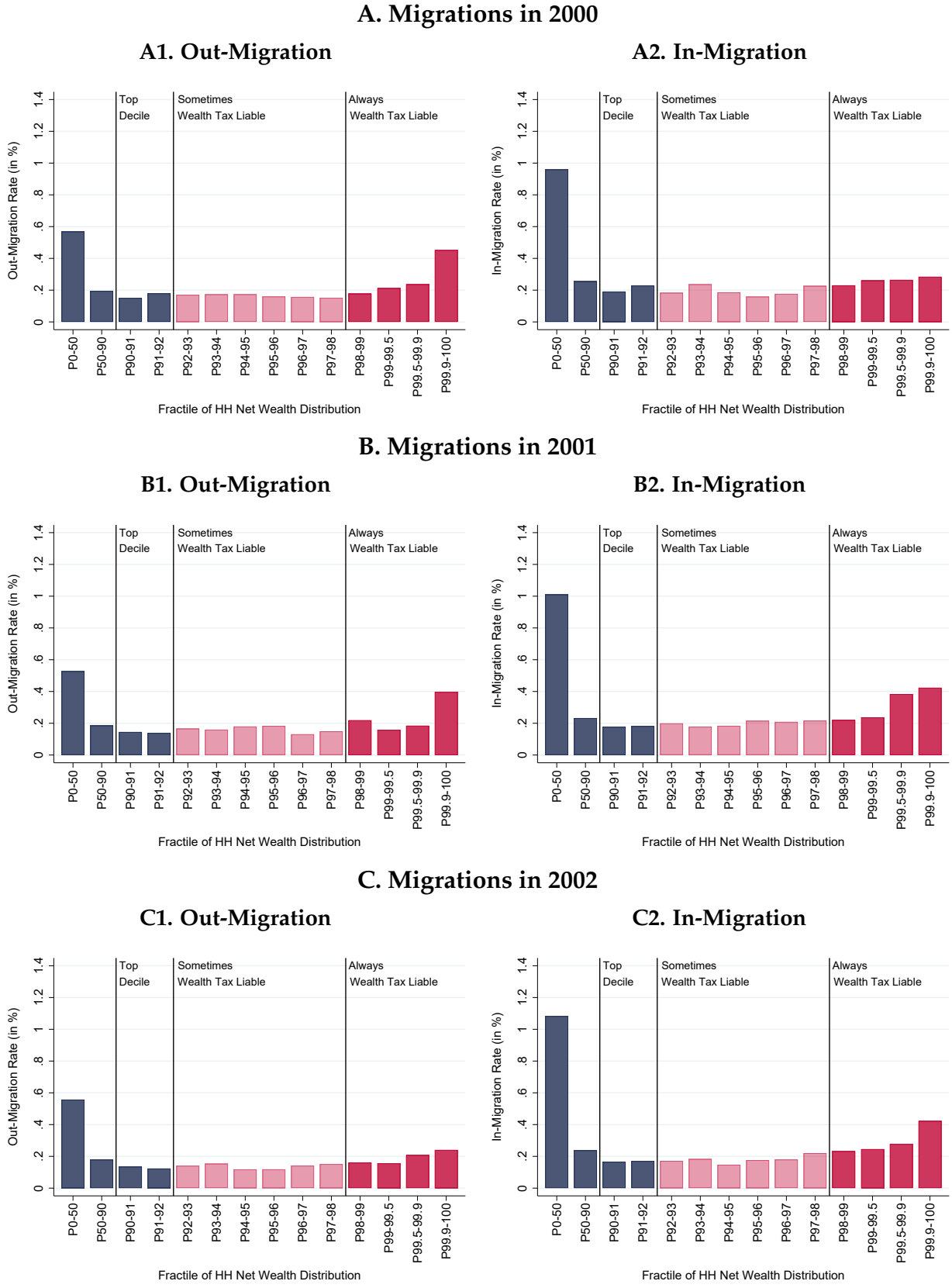
Notes: This figure describes the main origin countries of individuals in-migrating into Sweden (Panel A) and the main destination countries of individuals out-migrating from Sweden (Panel B) by wealth group (individuals whose household net wealth is either in the top 2% or in the top 10-20%).

Figure III.3: **Wealth and International Migration Patterns in Scandinavia**



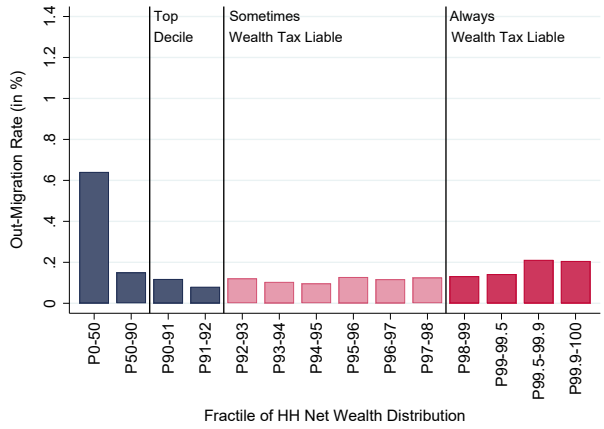
Notes: This figure shows in-migration and out-migration rates by decile of total net worth, in Sweden (Panel A) and Denmark (Panel B). We compute those statistics during the last years when the wealth tax was still in place in each country, which corresponds to the period 1999-2006 in Sweden and the period 1989-1996 in Denmark.

Figure III.4: **Wealth and International Migration Patterns in Sweden Between 2000 and 2004**

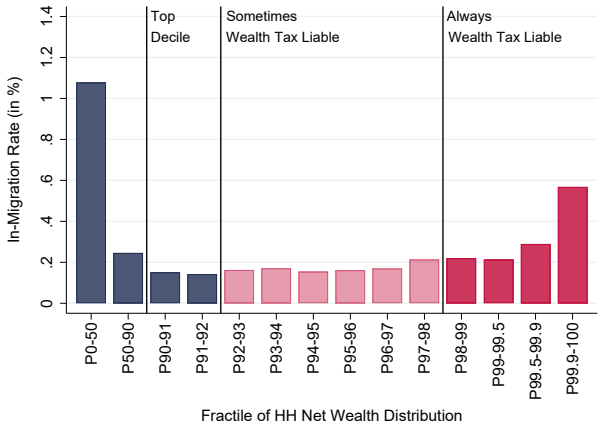


D. Migrations in 2003

D1. Out-Migration

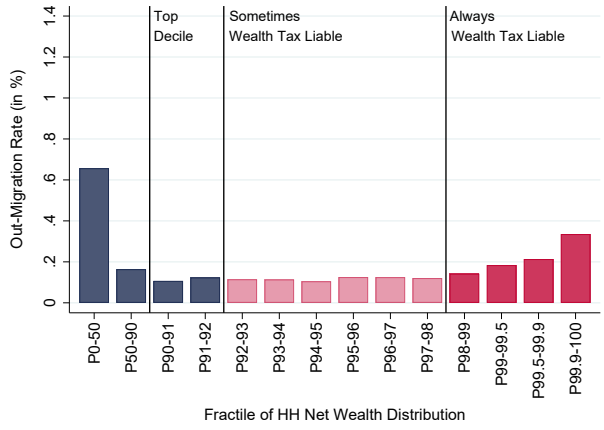


D2. In-Migration

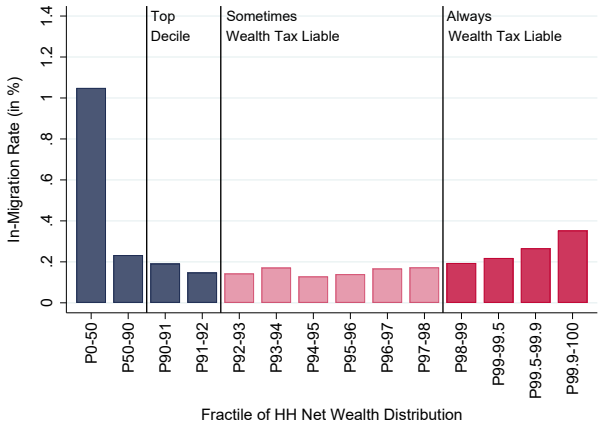


E. Migrations in 2004

E1. Out-Migration



E2. In-Migration



Notes: This figure shows in-migration and out-migration rates by level of total household net worth in Sweden for each year between 2000 and 2004. Each year, we rank individuals based on their household net worth, and show the out-migration and in-migration rates for each fractile of the household net wealth distribution.

III.3. Selection: Who Migrates In and Out?

We study selection into migration out of and into Sweden. We perform the analysis during the period 2000-2006 when the wealth tax was in place. We start by asking what individual characteristics can predict out-migration (respectively in-migration) in Sweden. We run a first regression model:

$$\mathbb{P}\{Y = 1\} = \beta' X_0 \tag{6}$$

where X_0 is vector of individual characteristics. The set of characteristics includes age bins, education bins, a dummy for being a closely-held active business owner, a dummy for being an independent contractor, a dummy for being foreign born, and cognitive skills quartiles.[4] To ease computation, we run this specification on the full population of the wealthy (top 2% of wealth distribution) and on a 10% random sample of the rest of the population.[5]

The blue coefficients in Figure III.5 correspond to the estimated coefficients on each of those demographics estimated on the all Swedish individuals, regardless of their wealth. We rescale the coefficients by the average predicted probability of out-migration, in order to interpret our coefficients in relative terms. In the general population, we find that younger individuals are much more mobile than others. Education is also a strong predictor of mobility, with the more educated more likely to move out of, or move into Sweden, although PhDs are much more likely to out-migrate than in-migrate. The gradient of self-selection into mobility follows the same pattern for cognitive ability, with higher skills leading to more mobility out of and into the country. On the contrary, in the full population, entrepreneurs and business owners are less likely to migrate than others. This suggests that owning a firm could be associated with large mobility costs and frictions that make it harder to leave the country.

We then focus on self-selection into migration at the top of the wealth distribution. To understand if selection patterns into migration are different for the very wealthy, we estimate:

$$\mathbb{P}\{Y = 1\} = \mathbb{1}\{W < P_W\} \cdot \beta' X_0 + \mathbb{1}\{W \geq P_W\} \cdot \beta'_w X_0 \tag{7}$$

Where we use X_0 to denote vectors and we set $P_W = \text{P98}$, i.e. the 98th percentile of the

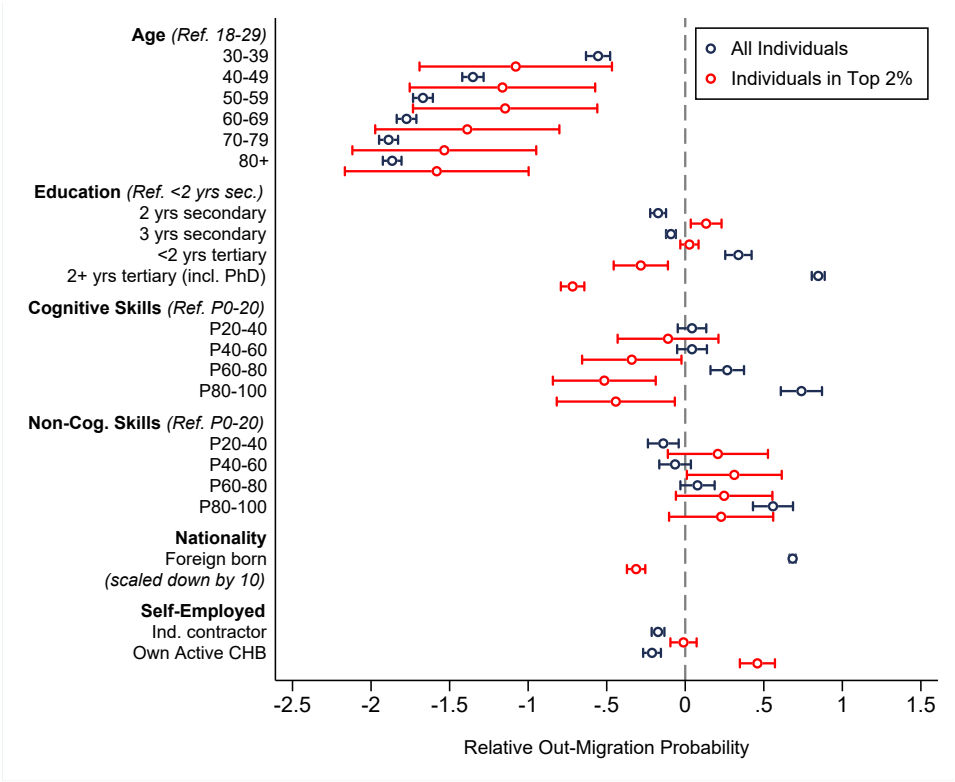
[4] Active closely-held businesses are firms that have at least one employee beyond the owner. For cognitive skills, we proceed by running a separate regression because cognitive skills come from enlistment data and are therefore available for a limited subset of people.

[5] When using a 10% random sample of the entire population of the wealthy, we found similar results. The baseline approach instead allows to exploit all variations in our migration events at the top, that we will study later.

wealth distribution. The red coefficients in Figure III.5 plot the estimated vector of coefficients β_w representing the effects of the various components of X_0 on the out-migration (respectively in-migration) probability for individuals in the top 2% of the wealth distribution.

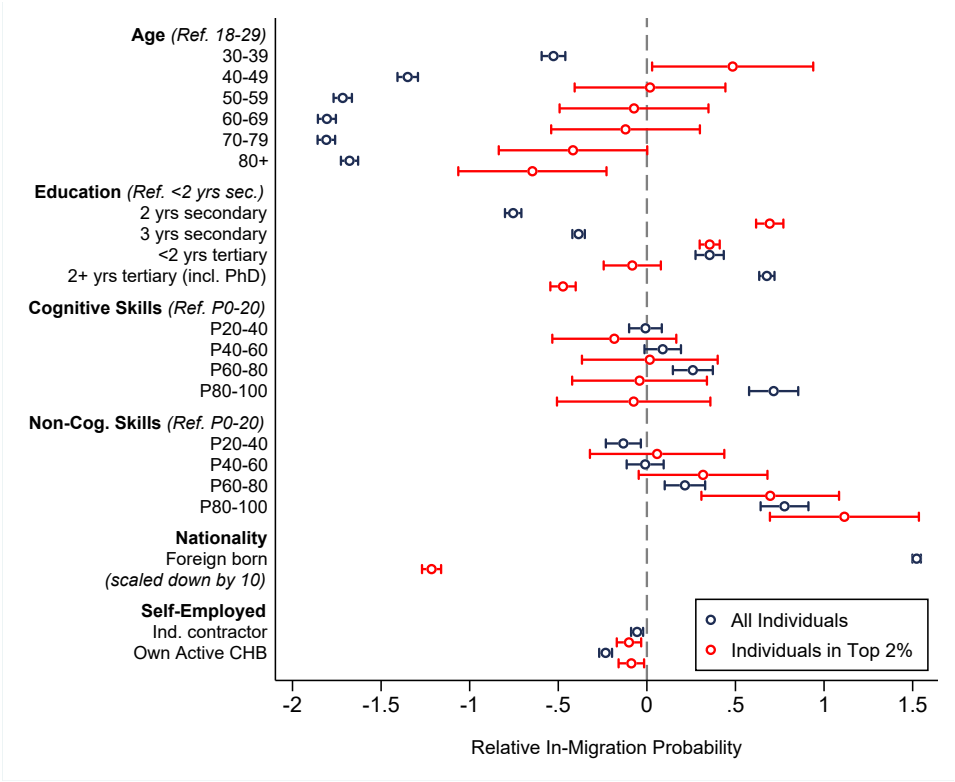
The results show that selection patterns differ among the very wealthy. For individuals in the top 2% of the wealth distribution, the education and cognitive ability gradient are inverted. In other words, while there is positive selection ("brain drain") into migration for the population at large, the reverse is true at the top of the wealth distribution. We also find that wealthy business owners do appear to be more likely to out-migrate, but not to in-migrate. The estimated (rescaled) coefficient of 0.41 in the top panel means that all else equal, individuals in the top 2% who control an active firm in Sweden are 40% more likely than non-wealthy entrepreneurs to outmigrate. The effect was of the opposite sign when focusing on the entire population. On the contrary, being an entrepreneur is not associated with higher propensity to become a Swedish resident for wealthy individuals during that period, as shown in the bottom panel. In Appendix Table III.1, we also investigate whether owners of fast-growing firms are more likely to leave Sweden than others. We find that wealthy owners controlling firms with high value added growth rates are between 48% and 71% more likely to out-migrate.

Figure III.5: **Selection into Out-Migration from Sweden**



Notes: This figure describes selection into out-migration from Sweden during the period 2000-2007. We estimate Equation 6 and plot the estimated vector of coefficients β and their confidence intervals in blue. We estimate Equation 7 and plot the estimated vector of coefficients β_w and their confidence intervals in red. All coefficients are rescaled by the average predicted probability of out-migration. The estimation sample includes all taxpayers with household net wealth in the top 2%, and a 10% random sample of all other Swedish individuals. The information on cognitive and non-cognitive skills is available only for a subsample of Swedes who passed enlistment tests.

Figure III.6: **Selection into In-Migration to Sweden**



Notes: This figure describes selection into in-migration in Sweden. We focus on all individuals moving to Sweden over the period 1999-2005, when the wealth tax was still in place. Blue coefficients come from estimates of Equation 6 detailed in the text, while red coefficients are estimated on the interaction term of Equation 7. All coefficients are rescaled by the average predicted probability of in-migration. The estimation sample includes all taxpayers with household net wealth in the top 2% and a 10% random sample of all other Swedish individuals.

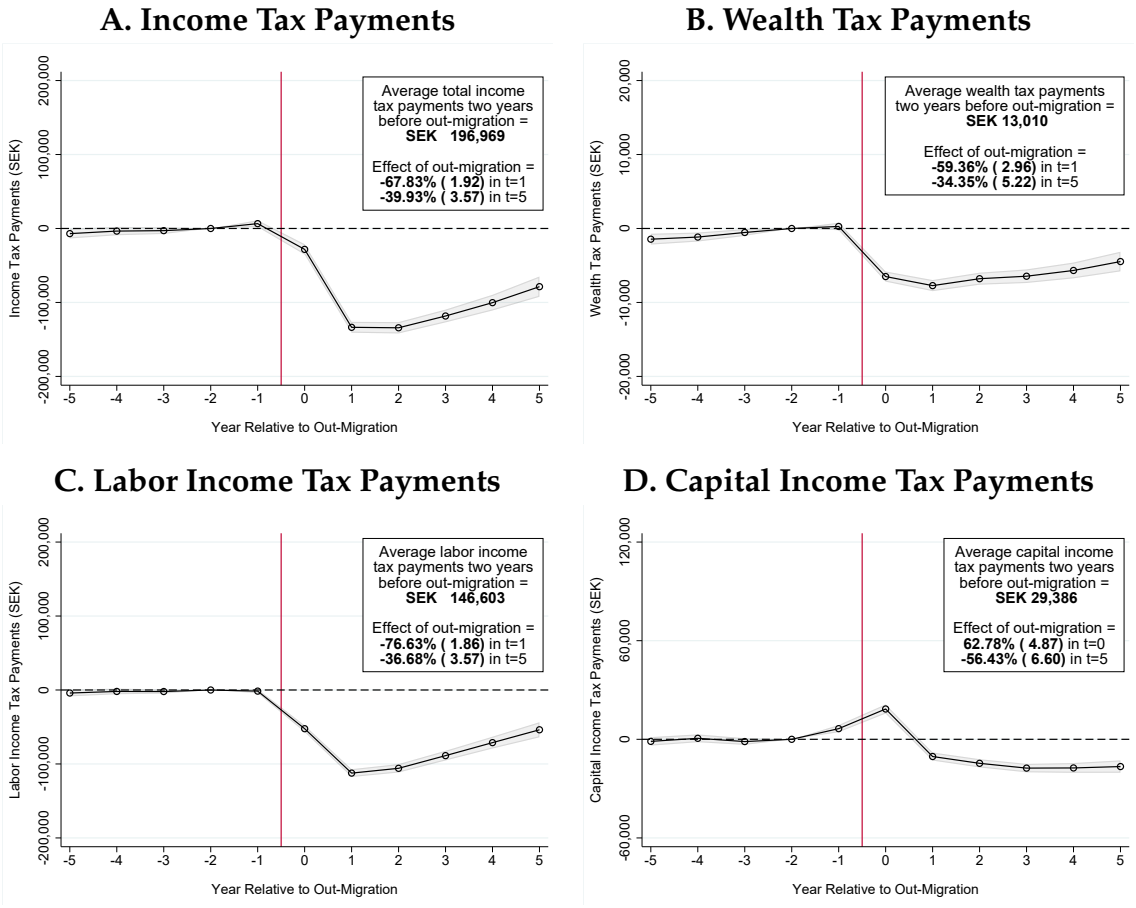
Table III.1: **Selection into Out-Migration from Sweden for Owners of Fast-Growing Active Closely-Held Businesses**

	All Individuals	Individuals in Top 2% of Net Wealth
Value Added Growth		
Year-on-Year, Top 25%	-20.01%	65.95%
	(6.54)	(12.50)
Past 3 Years, Top 25%	-19.09%	93.41%
	(7.13)	(13.88)
Past 5 Years, Top 25%	-32.10%	85.97%
	(7.52)	(14.97)
Year-on-Year, Top 50%	-25.31%	51.99%
	(4.10)	(7.87)
Past 3 Years, Top 50%	-27.90%	54.01%
	(4.13)	(8.00)
Past 5 Years, Top 50%	-27.47%	52.11%
	(4.77)	(9.36)
Employment Growth		
Year-on-Year, Top 25%	-29.86%	40.60%
	(6.22)	(12.29)
Year-on-Year, Top 50%	-19.92%	32.53%
	(4.43)	(8.82)

Notes: This table describes selection into out-migration for owners of fast-growing active closely-held businesses ("CHB") in Sweden. Each row considers a different indicator of ownership of a fast-growing active CHB. We compute the growth rate of each firm based on either its value added or its number of employees over different intervals of time (year-on-year, past 3 years, past 5 years). We then create yearly ranks of active CHBs based on those growth rates. In any given year, we define an individual as the owner of a fast-growing active CHB if the highest growth rank among the active CHBs she owns in that year is in the top 25% or above the median, depending on the specification. We use this indicator to estimate Equation 6 and Equation 7, together with age bins, education bins, a dummy for being foreign-born and a dummy for being an independent contractor. We run one separate regression for each indicator of ownership of a fast-growing active CHB shown in each table row. The equations were estimated on a sample including all individual-year observations where the individual was in the top 2% of household net wealth and a random 10% of the remaining observations. The first column of the table displays the estimated coefficients for each ownership indicator from Equation 6. The second column displays the estimated coefficients for each ownership indicator interacted with a dummy for being in the top 2% of household net wealth from Equation 7. All the coefficients displayed in the table are rescaled by the average predicted probability of out-migration and multiplied by 100 to be interpreted in relative terms.

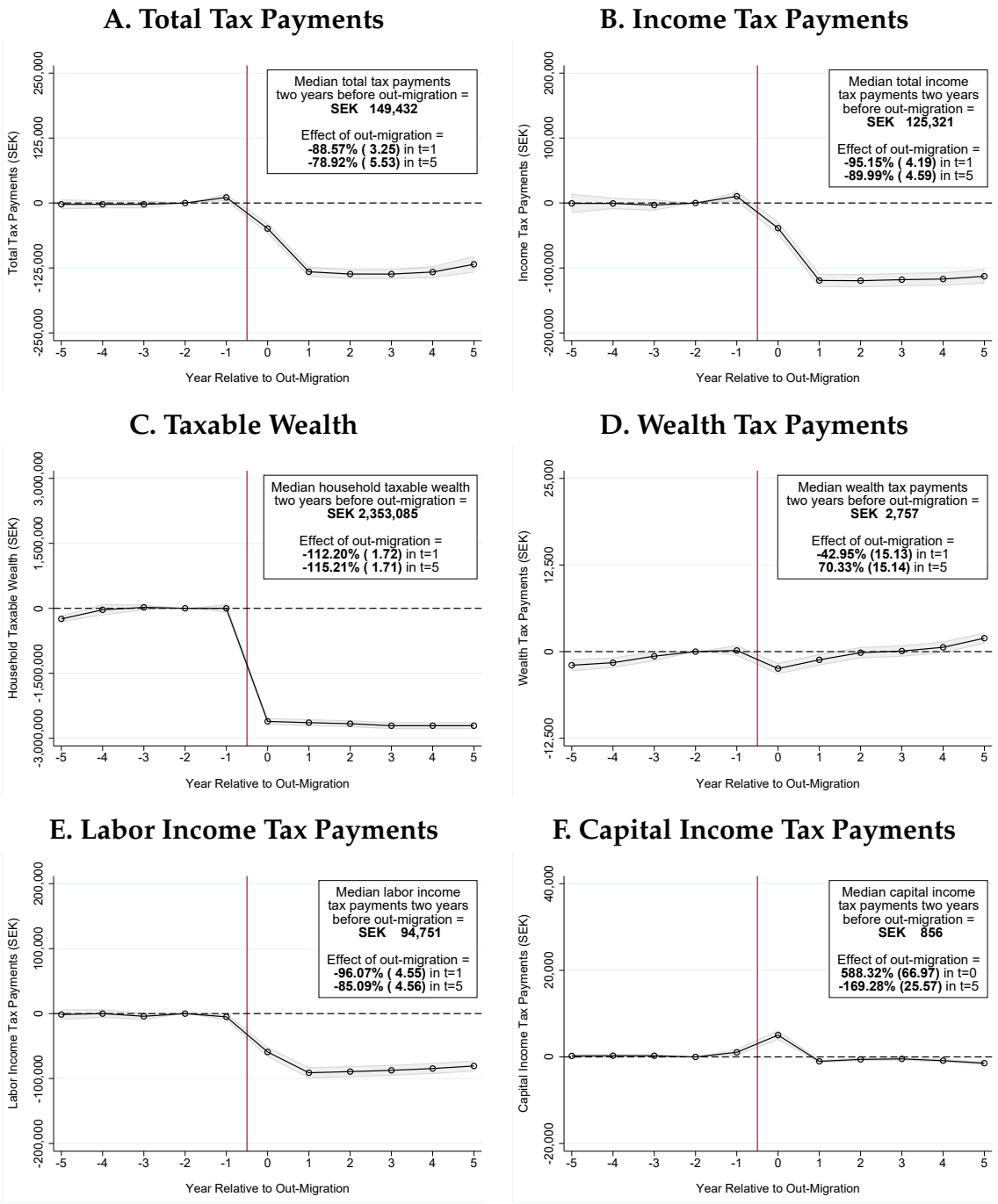
IV. Impact of Migration on Individual and Firm Outcomes: Additional Event Study Results

Figure IV.1: **Effects of Wealthy Out-Migration on Tax Payments**



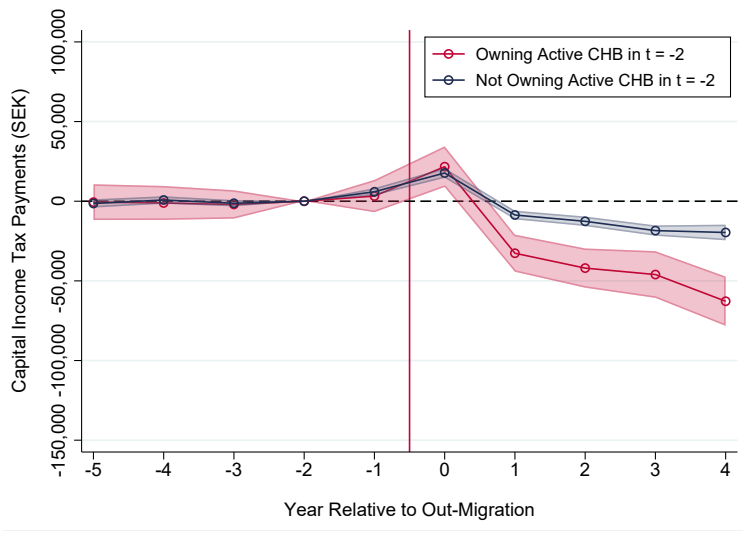
Notes: This figure describes the evolution of several additional tax outcomes of wealthy individuals' before and after they leave Sweden, compared to control wealthy individuals who do not move that same year. These include the components of total tax payments shown in Figure 6, Panel A. The sample includes individuals in the top 2% of the household net wealth 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 still 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% 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 IV.2: **Median Effects of Wealthy Out-Migration on Tax Payments, Top 2%**



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 whose household net wealth was in the top 2% of the household net wealth distribution in Sweden for at least one year before their true or placebo out-migration date. We plot the estimates β_j from Equation 7 in the main text estimated via median regressions.

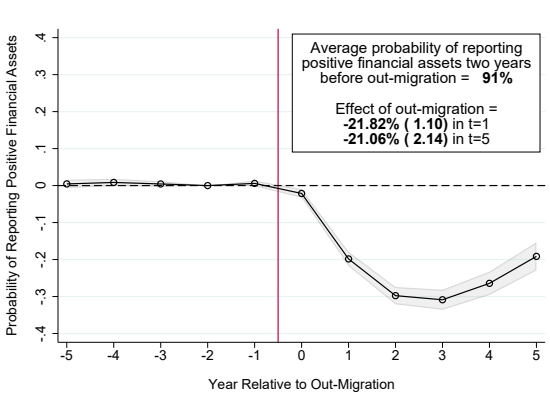
Figure IV.3: **Effect of Wealthy Out-Migration on Capital Income Tax Payments, Top 2%**



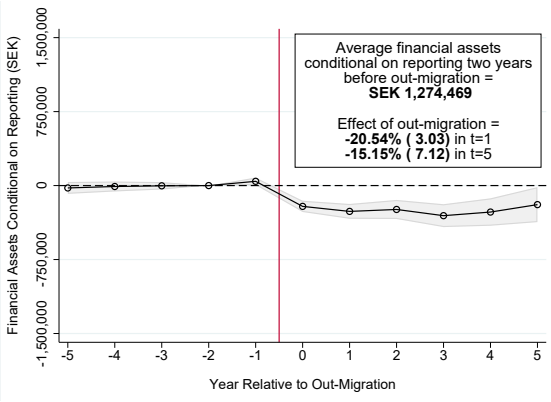
Notes: This figure describes the evolution of wealthy individuals' capital income tax payments before and after they leave Sweden, compared to control wealthy individuals who do not move that same year. We focus on individuals in the top 2% of the household net worth distribution in Sweden for at least 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 plot the estimates β_j from Equation 7 in the main text. We estimate the equation separately for wealthy individuals who own an active firm before out-migrating (red series) and wealthy individuals who do not control an active firm (blue series).

Figure IV.4: **Effects of Wealthy Out-Migration on Portfolio Composition**

A. Positive Financial Wealth

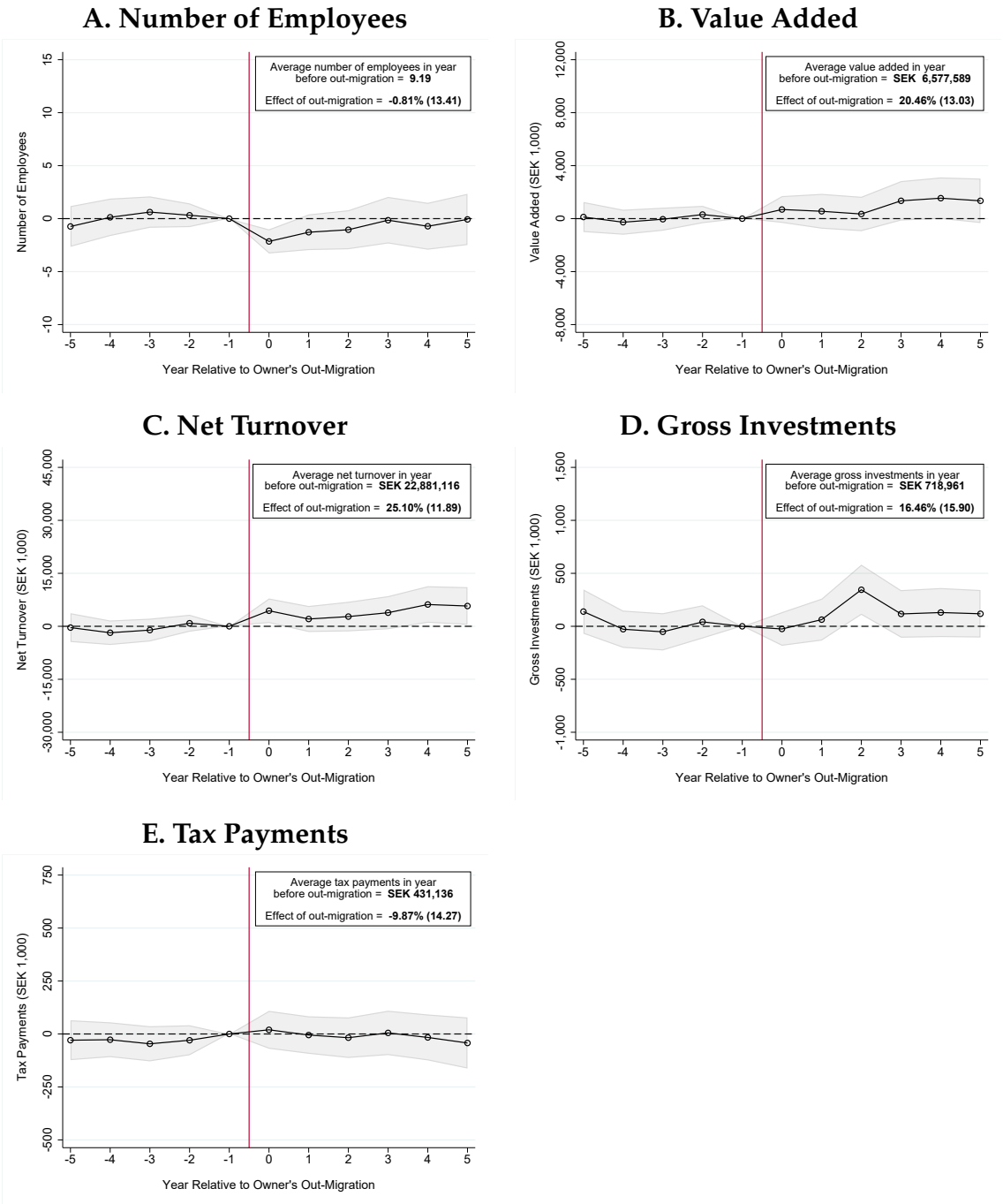


B. Financial Wealth (Conditional on Positive Financial Wealth)



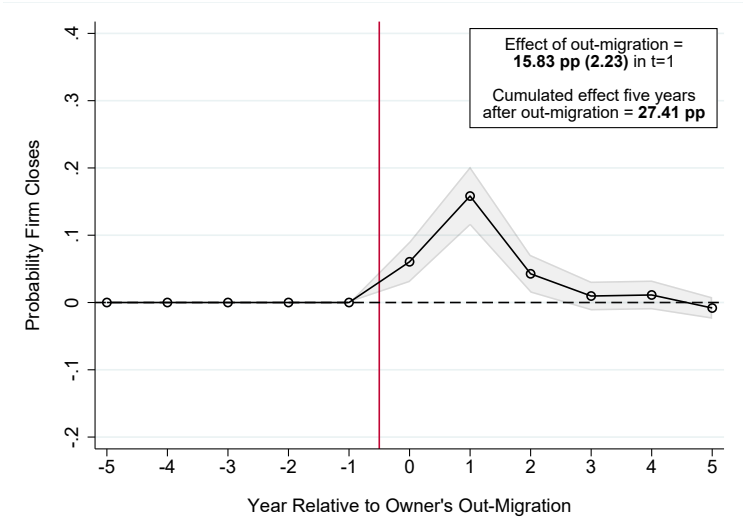
Notes: This figure describes the evolution of wealthy individuals' outcomes before and after they leave Sweden, compared to control wealthy individuals who do not move that same year. The sample includes individuals who were in the top 2% of the household net worth distribution in Sweden for at least 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 plot the estimates β_j from Equation 7 in the main text and their 95 percent confidence intervals. The estimates displayed in the text boxes are computed as the estimate of β_j when $t = 1$ 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 IV.5: **Effects of Wealthy Out-Migration on Closely-Held Businesses Outcomes Conditional on Existence of Firm, Top 2%**



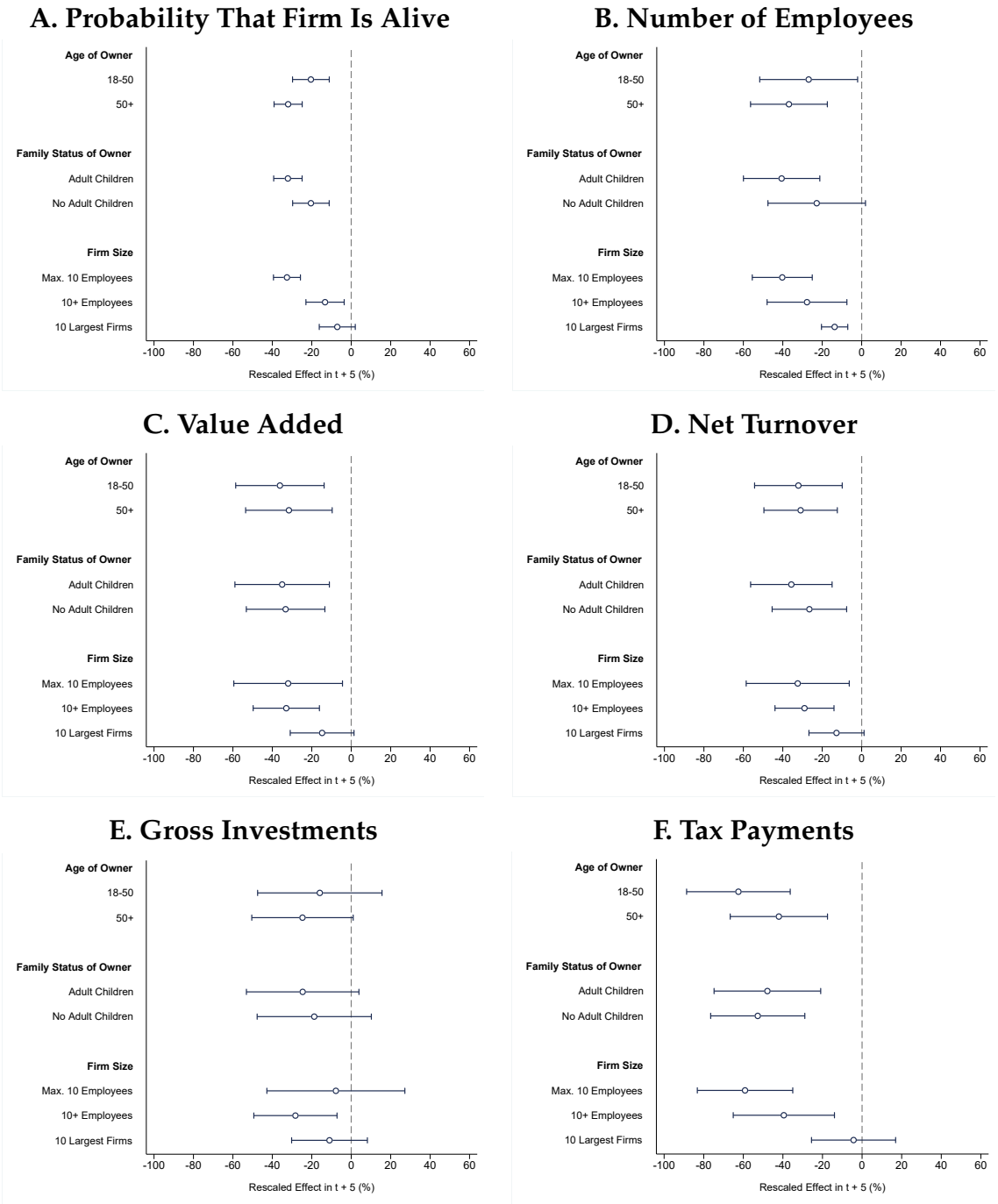
Notes: This figure shows the effects of wealthy owners' out-migration events on active firms controlled by individuals who were in the top 2% of the household net worth distribution in Sweden for at least one year before their migration event. We focus on out-migration events occurring between 2001 and 2007, with wealth ranks drawn from 2000-2006. We plot the estimates of β_j from Equation 7 in the main text. We restrict the analysis to firms that keep being active in Sweden throughout the period.

Figure IV.6: **Effect of Wealthy Out-Migration on Closure of Closely-Held Businesses, Top 2%**



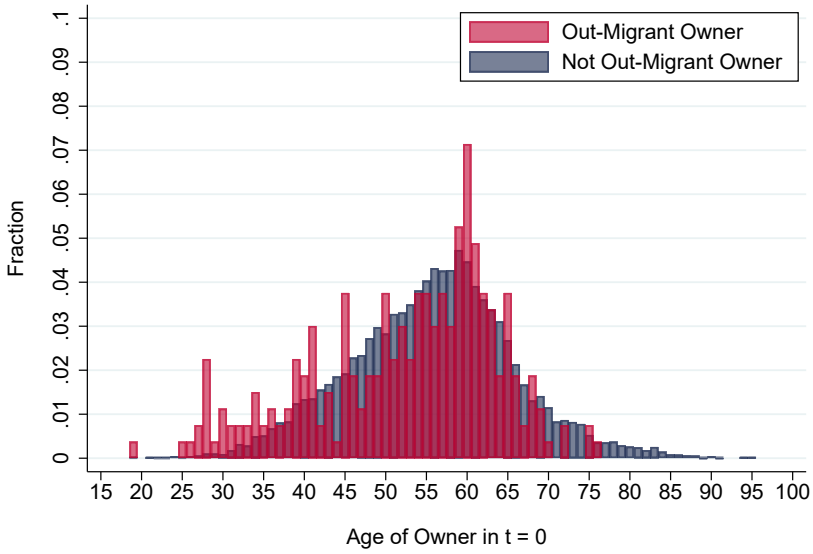
Notes: This figure shows the effects of wealthy owners' out-migration events on the probability that the firm they control before the out-migration event closes. We focus on out-migration events occurring between 2001 and 2007. The sample includes firms that in $t = -1$ were active closely-held businesses owned by individuals whose real or placebo out-migration happened in year t and who were in the top 2% of the household net worth distribution in Sweden for at least one year before t. We plot the estimates of β_j from Equation 7 in the main text.

Figure IV.7: **Effects of Wealthy Out-Migration on Closely-Held Businesses Outcomes by Wealthy Owners' Characteristics**



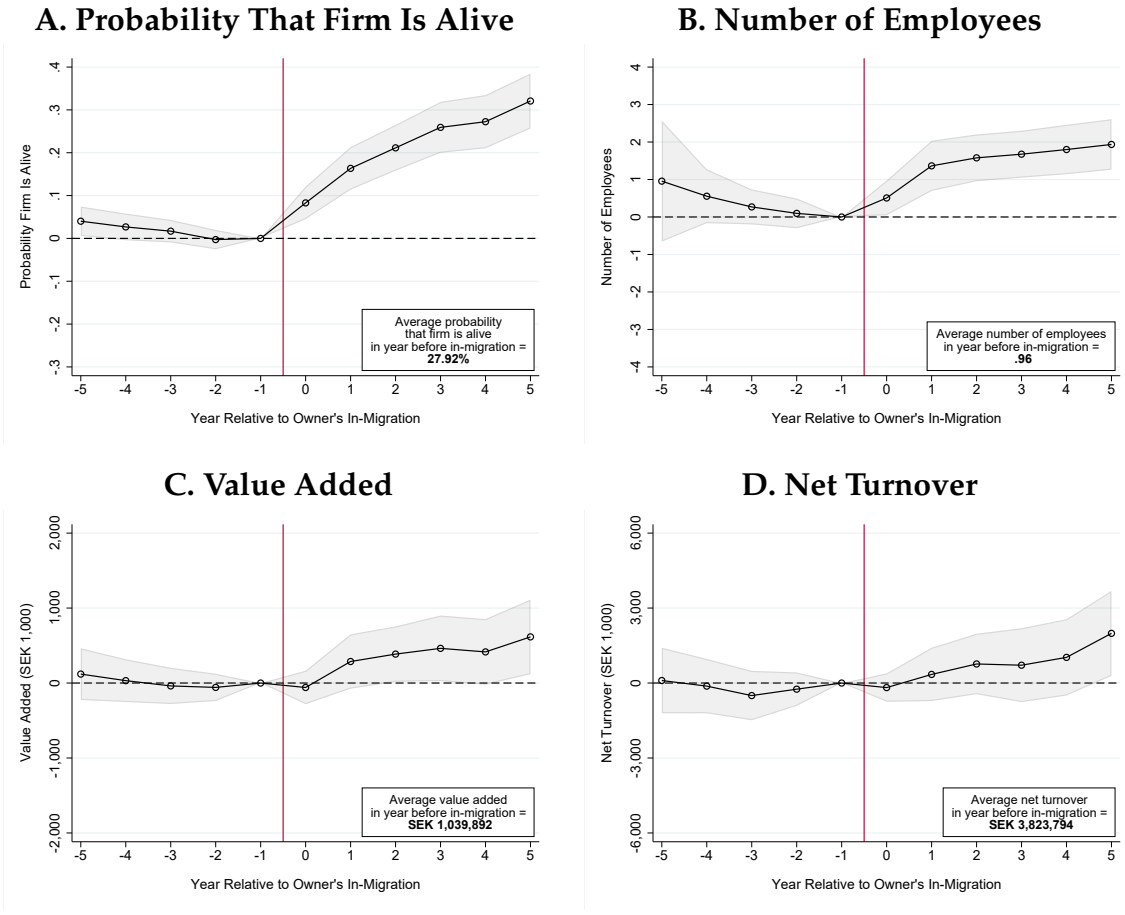
Notes: This figure shows the effects of wealthy owners' out-migration events on firm-level outcomes by wealthy owners' characteristics. Each coefficient and its confidence intervals refer to one separate regression. We focus on out-migration events occurring between 2001 and 2007. The sample includes 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 $t = 0$. We plot the estimates of β_j and their confidence intervals estimated from Equation 7 in the main text. The effects displayed in the figures are 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 IV.8: **Age Distribution of Closely-Held Business Owners, Top 2%**



Notes: This figure shows the age distribution of the wealthy owners of the closely-held businesses in our firm-level event-study sample in $t = 0$. These entrepreneurs own one of the closely-held businesses in the sample in $t = -1$ and are in the top 2% of the household net wealth distribution for at least one year before their observed or placebo out-migration event.

Figure IV.9: **Effects of Wealthy In-Migration on Firm Outcomes, Top 2%**



Notes: This figure shows the effects of wealthy owners' in-migration events on firm-level outcomes. The sample includes firms that were held by individuals who were in the top 2% of the household net worth distribution in Sweden for at least one year since their true or placebo in-migration date and before their potential following out-migration. The sample is further restricted to firms who were active closely-held businesses in at least one of the years in which they were held by the wealthy true or placebo in-migrant owner (between her true or placebo in-migration date and her potential following out-migration). We study in-migration events during the period 1999-2006, when a wealth tax was still in place in Sweden. We winsorize the bottom 1% and top 5% of all outcomes except for the number of employees, for which we winsorize only the top 5%. We plot the estimates of β_j from the in-migration counterpart of Equation 7 in the main text.

Table IV.1: **Test of Equality of Out-Migration and In-Migration Effects in Firm-Level Event Studies**

Sample of CHBs with owner in the top 2% of net worth

	Effects In Levels		
Outcome	**Out-Migration** $t_{out} = +5$	**In-Migration** $t_{in} = +5$	**T-Statistic** $t_{out} = t_{in}$
Prob. Firm Is Alive (pp)	-27.41 (2.91)	32.07 (3.27)	1.06
Number of Employees	-2.87 (0.67)	1.94 (0.34)	-1.25
Value Added (SEK 1,000)	-2,121.31 (490.43)	615.58 (253.93)	-2.73
Net Turnover (SEK 1,000)	-6,745.50 (1,528.62)	1,987.76 (868.31)	-2.71
Gross Investments (SEK 1,000)	-139.37 (64.17)	20.36 (41.38)	-1.56
Tax Payments (SEK 1,000)	-197.26 (36.18)	11.22 (16.96)	-4.66

Notes: This table compares the estimates of firm-level responses to out-migration events and in-migration events of wealthy entrepreneurs (in the top 2%) in Sweden. For each firm-level outcome in each row, we present β_5 estimated from Equation 7 in the main text, focusing on out-migration events (Column (1)) and in-migration events (Column (2)). Column (3) reports the t-statistic of a test of equality for the estimates of out-migration and in-migration effects.

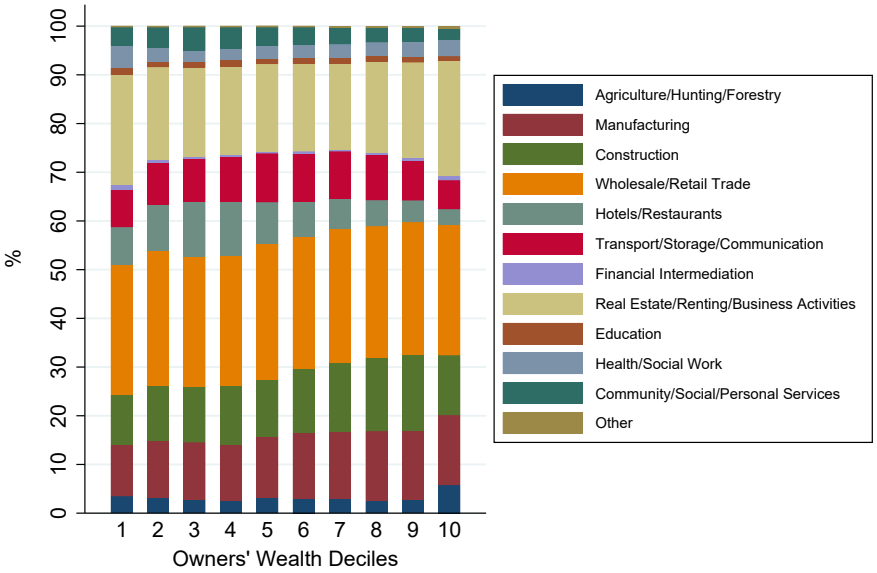
Table IV.2: **Descriptive Statistics on the Characteristics of Firms in the Event Study Samples**

	Control Firms				Treated Firms			
Variable	**Mean**	**Median**	**Std. Dev.**	**Obs.**	**Mean**	**Median**	**Std. Dev.**	**Obs.**
Panel A. Sample of active CHBs with owner in the top 2% of net worth								
No. of Owners	2.20	2	3.26	14,664	5.03	2	19.10	235
No. of Employees	9	4	12	14,664	10	4	13	235
Value Added	4,398	2,038	5,872	13,445	6,260	2,985	7,567	203
Net Turnover	14,353	5,389	21,004	13,445	21,462	6,328	26,991	203
Gross Investments	560	92	1,039	13,445	689	77	1,218	203
Tax Payments	202	44	362	13,445	376	87	521	203
Panel B. Sample of active firms with direct or indirect owner in the top 2% of net worth								
No. of Owners	3.64	2	8.64	22,433	24.22	4	111.72	717
No. of Employees	15	5	21	22,433	23	10	27	717
Value Added	6,567	2,557	9,743	20,705	11,698	5,208	14,202	655
Net Turnover	23,229	7,300	36,845	20,705	41,877	17,625	51,043	655
Gross Investments	732	108	1,416	20,705	1,033	114	1,819	655
Tax Payments	245	42	570	20,705	435	56	946	655

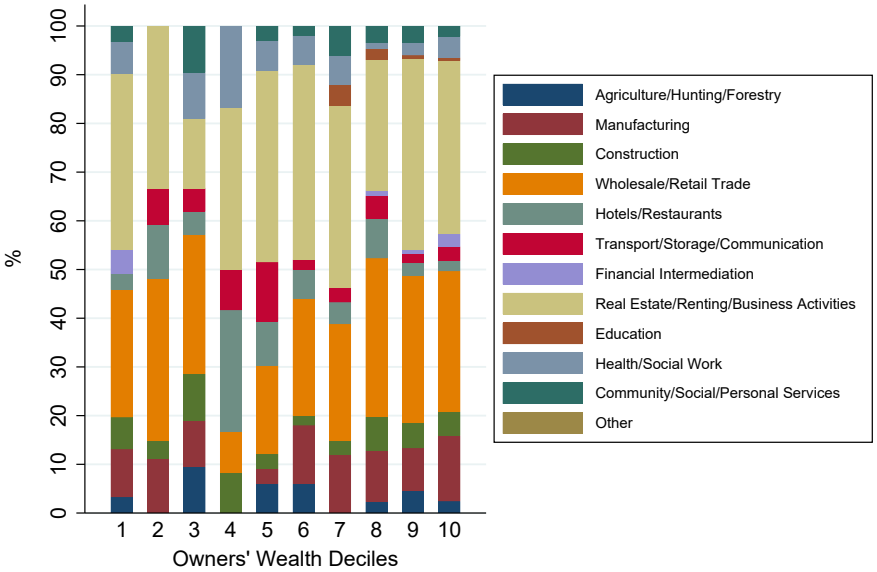
Notes: This table reports descriptive statistics for the sample of firms in our event-study sample. We study active firms controlled by individuals in the top 2% of the household net wealth distribution in Sweden at least one year before their true or placebo first out-migration event, as defined by our predicted wealth measure. In Panel A, we restrict our attention to closely-held businesses ("CHBs"), while in Panel B, we additionally look at indirectly held firms, identified using the registry of ownership links. We focus on out-migration events occurring between 2000 and 2007, with wealth ranks drawn from 1999-2006. The unit of measure for value added, net turnover, gross investments, and tax payments is SEK 1,000. All outcomes are measured in year $t-1$ relative to the true or placebo out-migration event. We winsorize the top 5% and bottom 1% of outcomes, except for number of employees, which we winsorize only the top 5% of.

Figure IV.10: **Distribution of Closely-Held Businesses Across Sectors by Owners' Net Worth**

A. All Active Closely-Held Businesses, All Years



B. Only Active Closely-Held Businesses with Out-Migrant Owners



Notes: This figure shows the composition of sectors for firms controlled by Swedish individuals with different levels of household net worth during the period 2000-2006. The category "Other" includes fishing, mining/quarrying, electricity/water/gas supply, public administration/defence, private households employing domestic staff, and extra-territorial organisations. A closely-held business is defined as active if it employs at least one worker beyond the owners. For each year, closely-held businesses are assigned the highest wealth rank among their owners in a given year. Panel B considers only firms with out-migrant owners in the year before their out-migration (our "treated CHBs" sample).

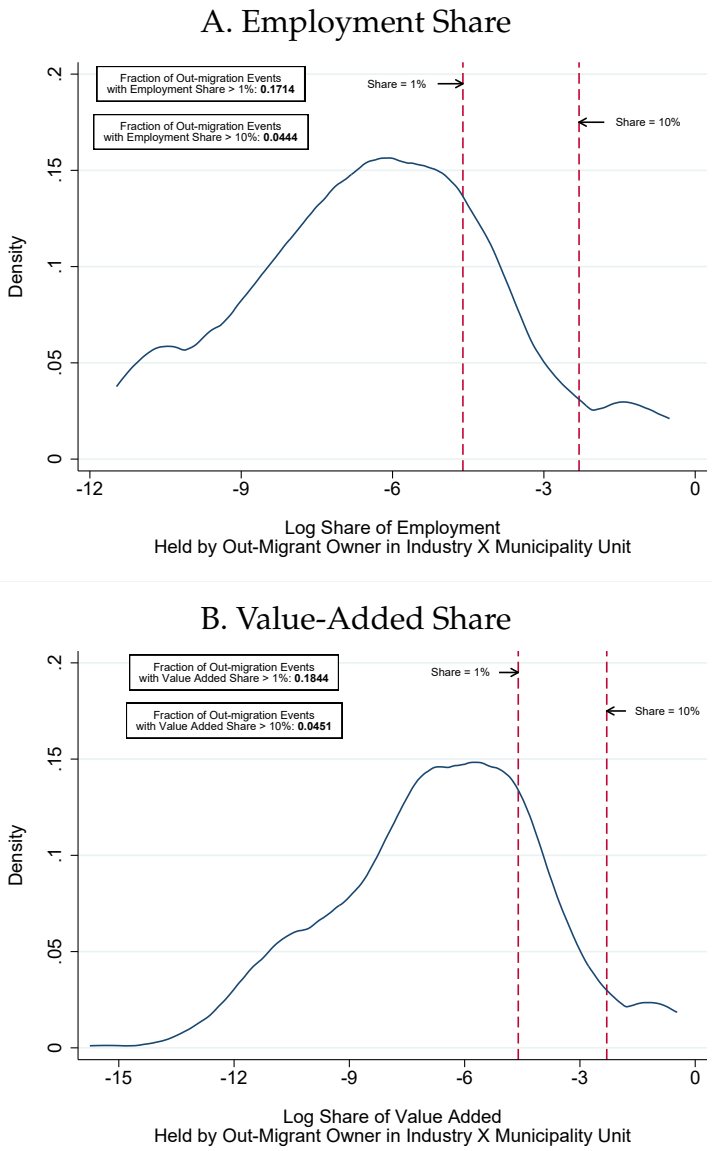
Table IV.3: **Effects of Wealthy Out-Migration on Closely-Held Business Outcomes in t+5 by Winsorization Method**

Sample of CHBs with owner in the top 2% of net worth

Winsorization	Number of Employees	Value Added	Net Turnover	Tax Payments	Gross Investments
None	-29.1%	58.0%	42.6%	1.7%	70.1%
	(27.41)	(49.46)	(39.85)	(45.97)	(48.48)
Top 1%	-33.7%	-12.9%	-22.2%	-75.7%	1.6%
	(10.87)	(15.62)	(11.37)	(38.35)	(17.49)
Top 5%	-33.3%	-24.0%	-31.7%	-133.7%	-21.9%
	(7.72)	(16.27)	(7.19)	(70.68)	(10.08)
Top 10%	-32.3%	-21.6%	-30.6%	-208.4%	-21.5%
	(6.12)	(20.41)	(6.34)	(129.84)	(8.79)
Bottom 1% + Top 1%		-21.8%	-22.2%	-35.8%	1.6%
		(11.37)	(11.37)	(11.89)	(17.49)
Bottom 1% + Top 5%		-34.2%	-31.7%	-50.5%	-21.9%
		(7.91)	(7.19)	(9.27)	(10.08)
Bottom 1% + Top 10%		-35.6%	-30.6%	-51.3%	-21.5%
		(6.99)	(6.34)	(8.07)	(8.79)

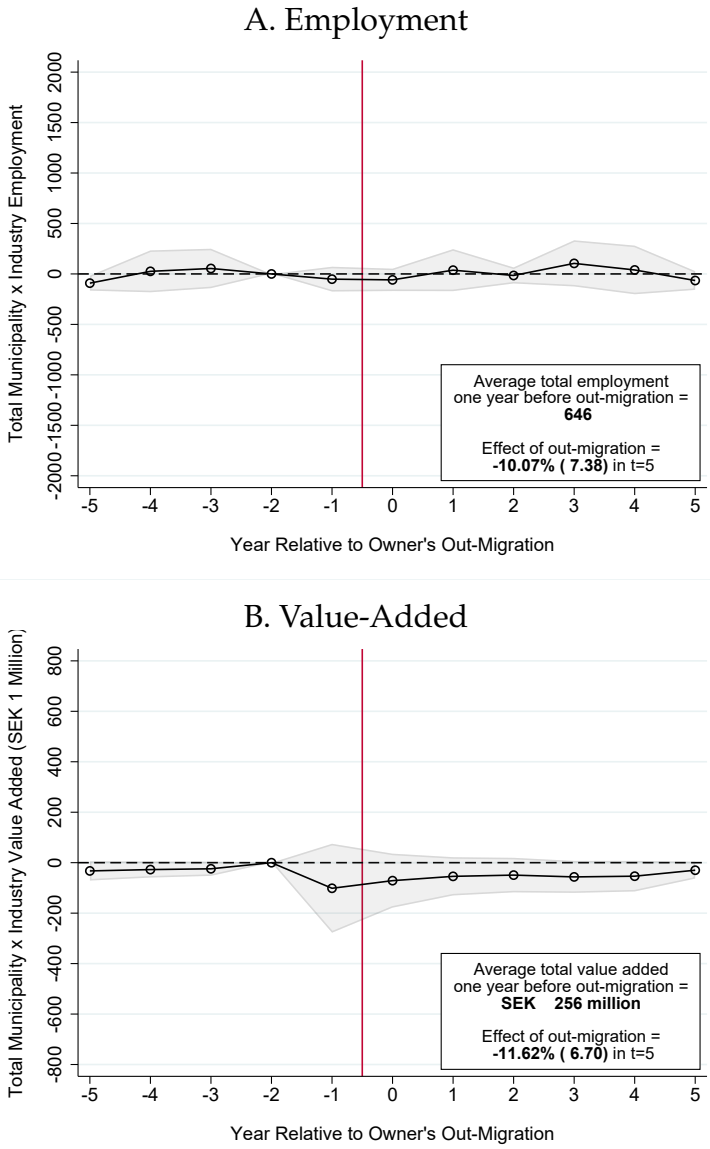
Notes: This table reports the estimated β_j in $t = 5$ from Equation 7 using various strategies to handle outliers. For each estimation procedure, we rescaled the estimated β_5 by the average outcome of the treatment group in $t = -1$ and multiplied it by 100. The standard errors are rescaled following the same approach. Each row considers a different winsorization methodology. Each column refers to a separate regression that corresponds to a specific outcome.

Figure IV.11: **Distribution of Employment and Value-Added Market Shares for Businesses of Owners Migrating Out of Sweden**



Notes: This figure plots the distribution of the log share of total employment (panel A) and total value-added (panel B) that closely-held businesses of wealthy out-migrants represent within local sectoral markets, in the year prior to migration. We define these markets as municipality times one-digit sector cells, and, for this exercise, restrict these cells to those with a total number of employees larger than 50.

Figure IV.12: **Effect of Migration of Owner of Granular Business (i.e. With Market Share $>$ 10%) on Market-Level Employment and Value-Added**

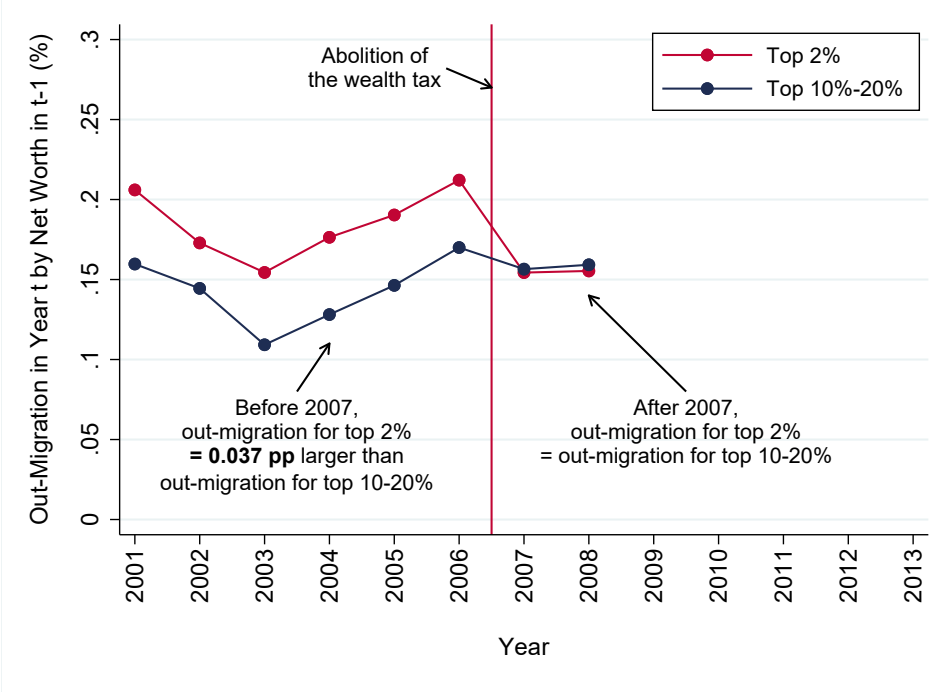


Notes: In this figure, we define local sectoral markets as municipality times one-digit sector cells, and, for this exercise, restrict cells to those with a total number of employees larger than 50. We focus on a treatment group of "granular" businesses that have an employment share of more than 10% in the year following out-migration of their owner. We run an event study on market-level outcomes (total employment in panel A and total value-added in panel B) and include a set of control markets, following the same strategy as in our baseline event study designs. The coefficients correspond to β_j from Equation 7.

V. Migration Elasticity Estimates

V.1. Additional Evidence: Sweden

Figure V.1: **Out-Migration Rates**



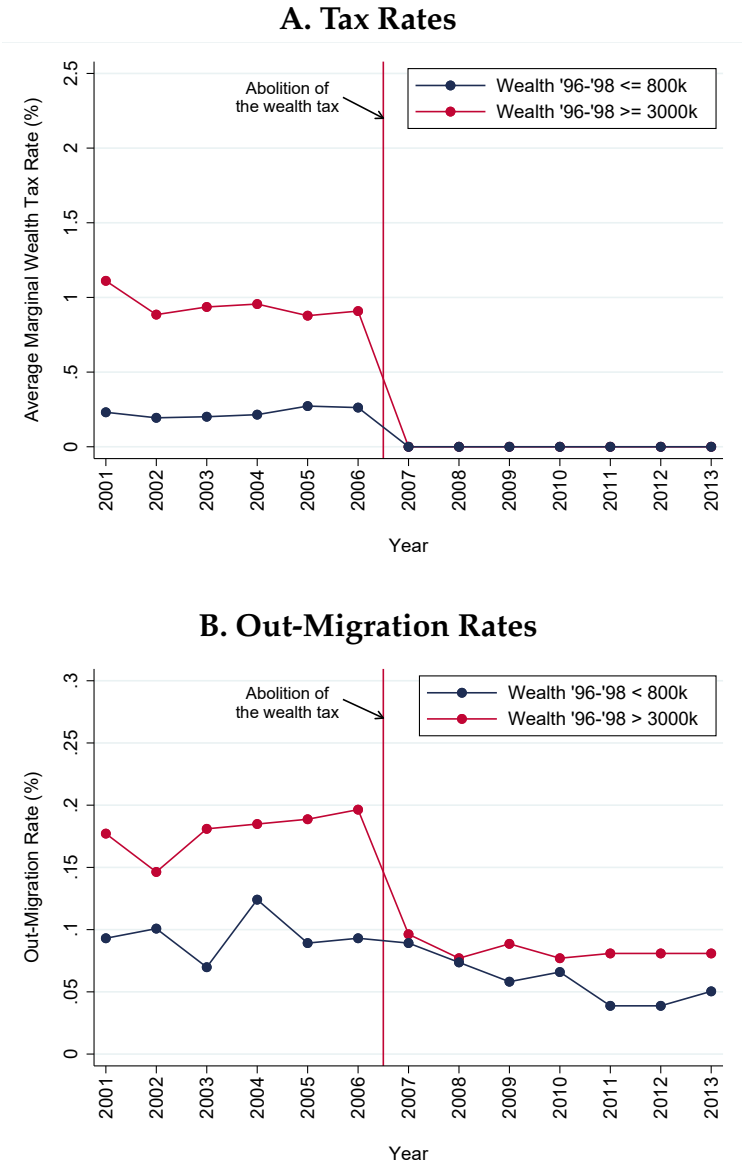
Notes: This figure shows the evolution of out-migration rates among the wealthy around the time of the wealth tax repeal. We show this for both our treated group in the top 2% of household net wealth (red series) and control group in the top 10-20% of household net wealth (blue series).

One challenge in comparing out-migration rates of wealthy individuals subject or not subject to the wealth tax is that we do not observe their total net wealth after 2007. We can circumvent this challenge by predicting individuals' wealth levels after 2007. The obtained measure of predicted wealth enables us to tackle two issues. First, we can compare out-migration patterns of treated and control individuals in the entire post-reform period, allowing to better gauge the credibility of our design and estimates. Second, using predicted instead of actual level of wealth avoids capturing potential endogenous responses to the reform through other channels than migration, for instance through savings responses following the change in wealth taxes (Jakobsen et al., 2020).

Figure V.2 illustrates this empirical strategy: we use 1996-1998 wealth levels as the simplest predictors of wealth levels at the time of the reform. We define individuals with household taxable wealth above SEK 3,000K in 1996-1998 as high exposure to the reform, since they were already above the highest wealth tax exemption threshold then. Individuals with taxable wealth below the minimum wealth tax exemption threshold of SEK 800K in 1996-1998 are the low exposure group. To verify that those categories of wealth levels in 1996-1998 translate to differences in exposure to the 2007 reform, Panel A shows the changes in effective wealth tax rates faced by those taxpayers. The figure confirms that past levels of wealth (measured 10 years before the reform) predict differential exposure to the wealth tax reform of 2007. Panel B shows the corresponding out-migration patterns for the same taxpayers. Out-migration rates of taxpayers with high exposure to the reform dropped suddenly in 2007, compared to out-migration rates of taxpayers in the control group. Compared to our previous specification in Figure 3, we can see that those patterns last after 2008, and up to 2013. This confirms that the drop in out-migration rates for the very wealthy persisted several years after the repeal of the wealth tax.

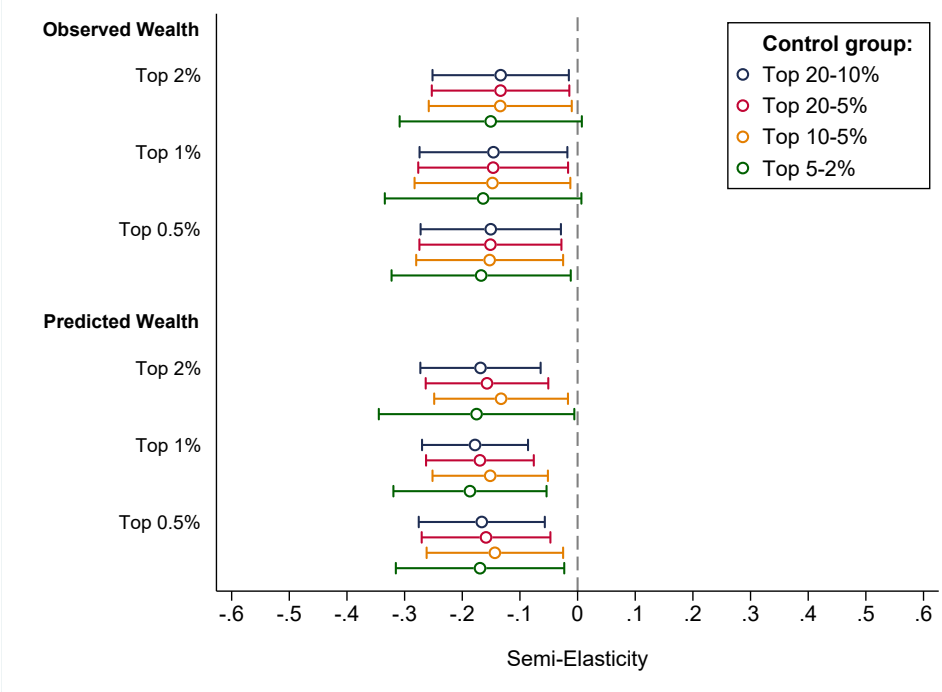
Our empirical approach in the paper (section 5) consists in generalizing this strategy, relying on the more elaborate prediction model of wealth described in Appendix II.1. Individuals are allocated to the treatment group if their predicted wealth belongs to the top 2%, and to the control group if their 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.

Figure V.2: **Out-Migration Rates by Predicted Exposure to the Wealth Tax Reform**



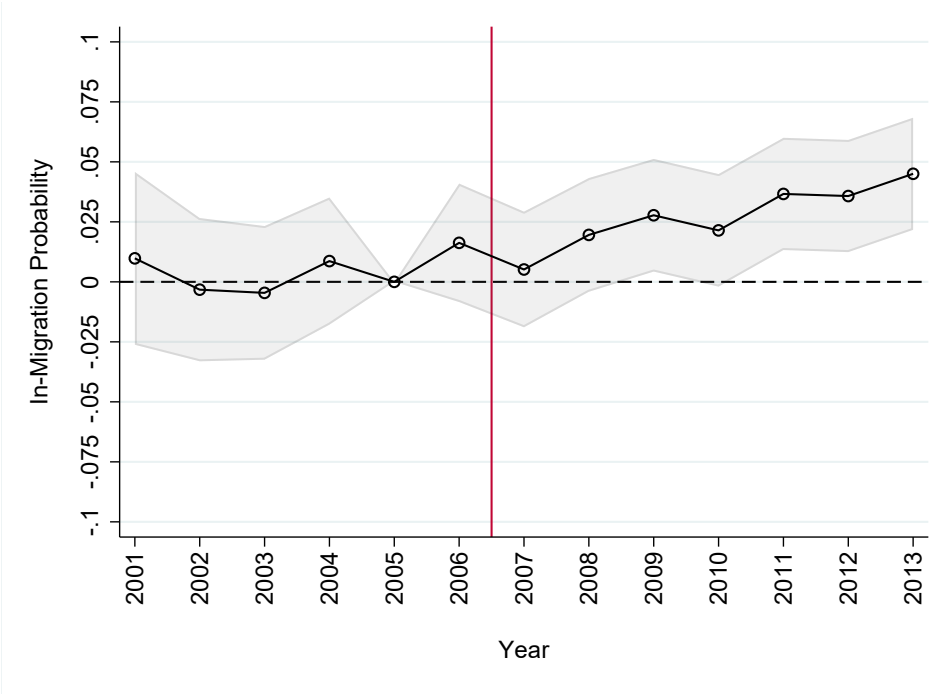
Notes: This figure shows the evolution of wealth tax rates (Panel A) and out-migration rates (Panel B) by exposure to the repeal of the wealth tax in Sweden, denoted by the vertical red line. We predict exposure to the reform by level of wealth in 1996-1998, comparing individuals above the highest exemption threshold during that period (3,000K, red) and individuals below the lowest exemption threshold (800K, blue).

Figure V.3: **Sensitivity of Migration Semi-Elasticities To Different Control Groups**



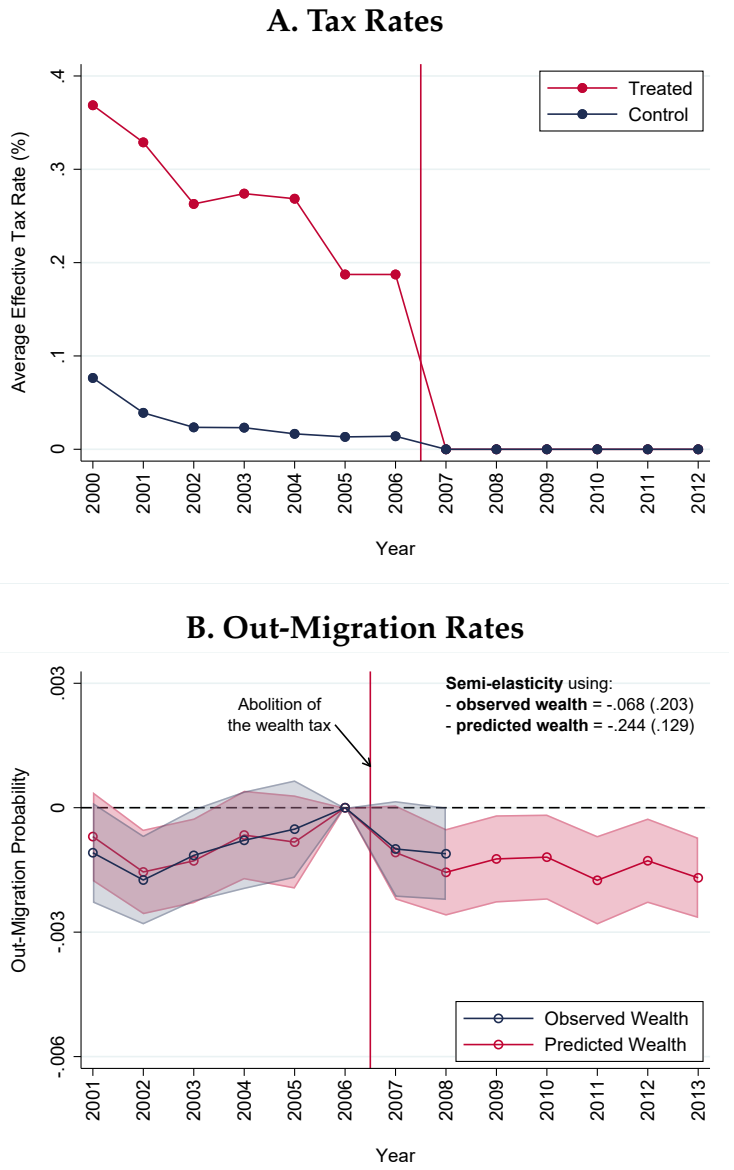
Notes: This figure tests the sensitivity of our migration semi-elasticities estimates to our choice of control group. The semi-elasticities plotted in the figure correspond to ε estimated from Equation 4 in the main text. We define treatment and control groups using observed level of wealth (top panel) or predicted level of wealth based on pre-reform assets and income flows (bottom panel). Each coefficient and its confidence intervals refer to one separate regression using the same treatment group (defined in left axis), but different wealth cut-offs for the control group. Our baseline and more conservative approach using the top 20-10% as the control group is shown in blue.

Figure V.4: **Return Migration Rate Response To The Abolition Of The Wealth Tax: Difference-In-Differences Estimates**



Notes: This figure shows the differential effects of the repeal of the wealth tax on the in-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 define the treatment and control group in 1999. We regress the yearly probability to return in Sweden on an interaction between year fixed effects and a dummy variable equal to one if the individual was subject to the wealth tax in 1999. We plot the estimated coefficients β_j from Equation 3 in the main text and their 95 percent confidence intervals.

Figure V.5: **Out-Migration Rates by Predicted Exposure to the Wealth Tax Reform (Entrepreneurs Only)**



Notes: This figure shows the evolution of average effective tax rates (Panel A) and out-migration rates (Panel B) of entrepreneurs by exposure to the repeal of the wealth tax in Sweden, denoted by the vertical red line. In Panel A, the treatment and control groups are defined by entrepreneurs with predicted household wealth in the top 2% and 20-10%, respectively. Panel B uses the same groups, defined separately using predicted household wealth (red) and observed household wealth (blue). We plot the estimated coefficients β_j from Equation 3 in the main text and their 95 percent confidence intervals. The semi-elasticities reported in the figure are estimated from Equation 4 in the main text.

V.2. Out-of-Sample Validation: Migration Elasticity Estimates in Denmark

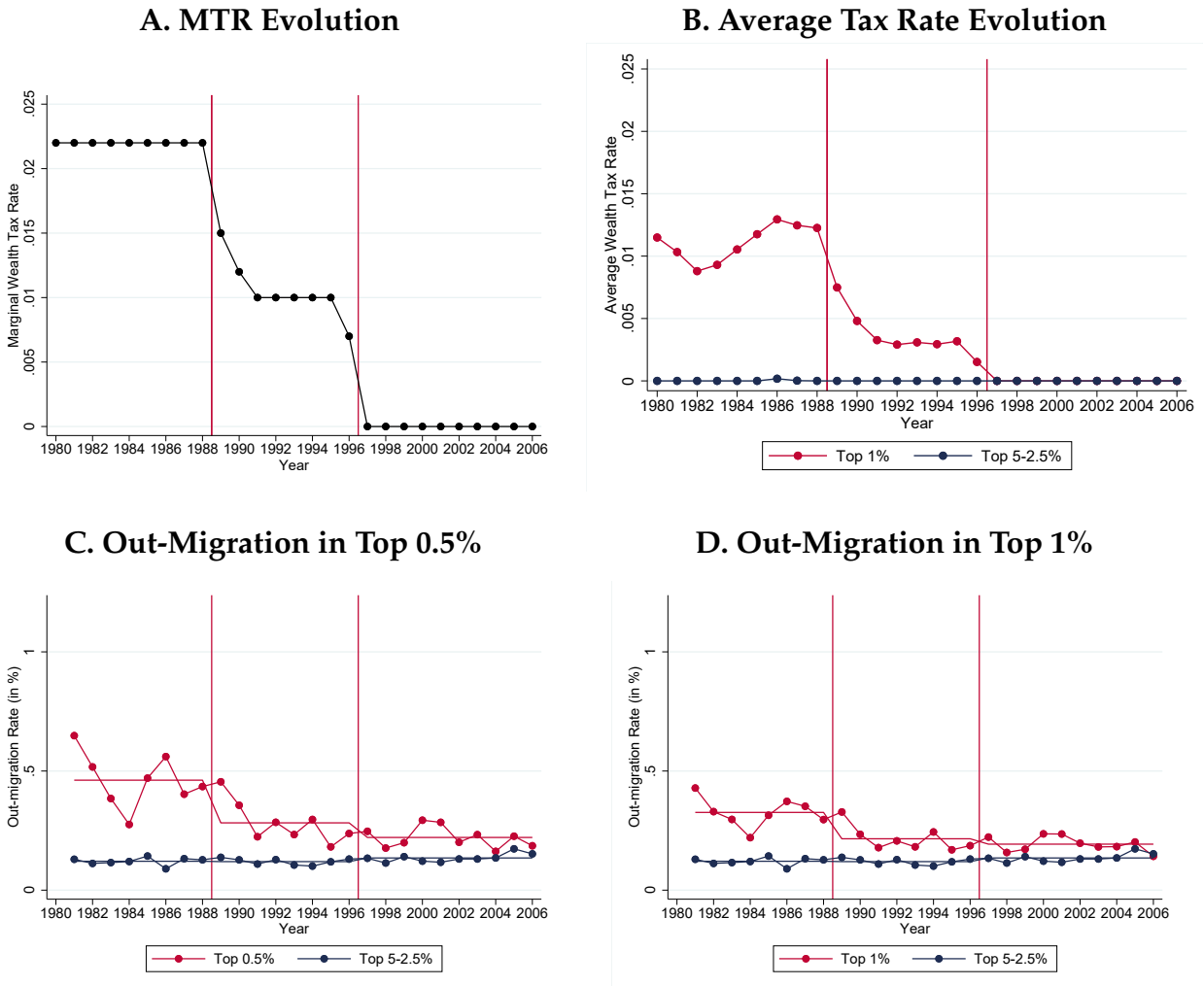
Net taxable wealth in Denmark was taxed every year at 2.2% until two major reforms. In 1988, the marginal tax rate on wealth above the exemption threshold was reduced to 1%. While the change in tax rate was large, this change was also gradual, and scattered over three years, as shown in Figure A.16, Panel A and B. The wealth tax was then entirely abolished between 1996 and 1997. Those two reforms provide potentially compelling identifying variation to study migration responses to the wealth tax.

We use the same identification strategy as with Sweden and compare out-migration patterns of wealthy tax payers just above the wealth tax exemption threshold, to those of taxpayers just below that threshold. The changes in marginal and effective wealth tax rates induced by the two Danish reforms are shown in Panel A and B of Figure V.6. The evolution of out-migration patterns around those changes for treated (red series) and control (blue series) individuals are shown in Panel C and D. Consistent with our main findings in Sweden, we find that reduced wealth tax rates are associated with a decrease in out-migration rates for taxpayers subject to the tax, with no corresponding changes for wealthy taxpayers not subject to the tax.

We note that the graphical evidence (from Figure V.6) is noisier in Denmark than in Sweden, mostly for two reasons. First, we have greater statistical power in Sweden because it is a larger country and it imposed wealth taxes on a larger share of taxpayers. Second, the Swedish wealth tax repeal offers a stronger research design for estimating migration responses: the tax changes were large, immediate, and unexpected. The Danish wealth tax reforms, on the other hand, were phased in gradually and across two reforms in close proximity to one another.

But as we show in the main text, the migration elasticities we obtain exploiting the Danish and Swedish reforms are very close, and not statistically different.

Figure V.6: **Effect of the Wealth Tax Reform on Out-Migration in Denmark**



Notes: This figure shows the out-migration patterns of wealthy taxpayers in Denmark around two large reforms in the wealth tax. The reforms generated two successive drops in marginal tax rates on wealth for wealthy taxpayers (Panel A) and drops if effective tax rates on wealth (Panel B). Panel C plots out-migration rates of the very wealthy (top 0.5%, red series) treated by the reforms and the wealthy not treated by the reforms (top 5-2.5%, blue series), before and after the two reforms, depicted by the two vertical red lines. Panel D plots out-migration rates of the wealthy (top 1%, red series) treated by the reforms and the wealthy not treated by the reforms (top 5-2.5%, blue series), before and after the two reforms, depicted by the two vertical red lines.

VI. Quantification of Aggregate Implications

In section 4 of our paper, we produce well-identified estimates of the semi-elasticities of migration flows with respect to the net-of-tax rate on wealth. Our evidence shows clear and significant effects on migration flows that appear immediately and are persistent. To draw policy implications, we need to translate these effects on migration flows into effects on the stock of the population of the very wealthy, and on the stock of taxable wealth.

Indeed, even though the estimated effects on migration flows are very small, one may wonder how large the effect on the stock may be if these small flow effects were to cumulate over time. This begs the question: how exactly and for how long should one cumulate these flow effects in order to properly measure the stock effect? It is clear that one needs to cumulate these flow effects somehow. But it is also clear that one cannot cumulate these flow effects infinitely. The latter would imply that the composition of the population of wealthy individuals can be considered as fixed: if a rich individual leaves, he is never replaced. Which would quite trivially imply that any significant constant negative effects on migration flows would inevitably drive the population of wealthy individuals to shrink to zero in the long run. In practice, a fraction of individuals constantly disappear from the population of wealthy individuals, through death, or because of wealth destruction, while a fraction of new individuals appear through wealth creation or inheritances. These constant inflows and outflows through birth and death, and through the creation, destruction, and transmission of wealth means that the composition of the population of the wealthy is not fixed, but constantly changing. The impact of migration flows on the stock of the population in the long run will therefore depend on the the relative magnitude of these different flows in and out of the population of wealthy individuals, which we can measure in the data.

Here, we explain how we use our data and the structure of a simple model to understand and measure precisely how these forces play out, in order to obtain a proper quantification of the stock elasticity.

VI.1. A Simple Framework Accounting for the Dynamics of the Population of Wealthy Individuals

Our framework accounts for the various forces that shape the dynamics of the local population of wealthy individuals. First, individuals can enter the population of wealthy individuals due to increases in their wealth. For now, we do not separate whether these increases come from wealth accumulation or from inheritances, but we come back to this

important distinction later. Second, individuals can exit the population of wealthy individuals due to wealth destruction or death. Finally, individuals can enter or exit the local population of wealthy individuals through migration.

We start with an OLG framework that provides the simplest formalization of these different forces. In each time period, indexed by t, a new group of individuals B_t is "born" into the population of wealthy individuals in Sweden. They "die" after T periods. In between their birth and their death, individuals can move in or out of the country. We define as the "age" of an individual the number of periods k since that individual was born in the wealthy population. At each time t, the total number of individuals of age k (N_t^k) present in Sweden corresponds to the number of individuals who are in Sweden and who were born in the wealthy population k periods ago.

$$
\begin{aligned}
&\text{Population of age 0 in } t && N_t^0 &&= B_t \\
&\text{Population of age 1 in } t && N_t^1 &&= B_{t-1}(1-\alpha_t^1) \\
&\text{Population of age 2 in } t && N_t^2 &&= B_{t-2}(1-\alpha_t^2)(1-\alpha_{t-1}^1) \\
& && \dots && \\
&\text{Population of age k in } t && N_t^k &&= B_{t-k}\prod_{j=0}^{k}(1-\alpha_{t-j}^{k-j}) = B_{t-k}S_{t-k}(k)
\end{aligned}
$$

where α_t^k is the net out-migration rate of individuals of age k in period t and $S_{t-k}(k)$ is the "survival rate" in Sweden at age k of individuals born in $t-k$.

At each period t, the total population N_t is simply the sum over all ages k of the number N_t^k of individuals who have been in the wealthy population for k number of periods. The total local stock of population of wealthy individuals is therefore $N_t = \sum_{k=0}^{T} N_t^k$.

VI.2. Microfoundations of Migration Responses

We are interested in how wealth taxation affects migration flow rates α_t^k of the very wealthy, and as a consequence, the local population stock of wealthy households. To get a microfoundation of migration behaviors, we rely on a random utility model. Individuals form utility over being in location $l \in \{S; O\}$, where S stands for Sweden, and O is the individual's next best location alternative. The utility of an individual of age k in location l in period t depends on her consumption opportunities in l, as well as some idiosyncratic location taste shock $\mu_{l,t}^k$. Consumption opportunities are simply her net-of-tax

labor income and net-of-tax wealth in location l.

$$\mathbb{U}_{l,t} = \underbrace{u((1-\mathbf{t}_{l,t})y^k_{l,t} + (1-\tau_{l,t})W^k_{l,t})}_{u^k_{l,t}} + \mu^k_{l,t}$$

where $y^k_{l,t}$ and $\mathbf{t}_{l,t}$ are respectively earnings and taxes on earnings in location l; W is wealth, and τ is the effective tax rate on wealth. Individuals of age k for whom $\Delta\mu^k_t = \mu^k_{O,t} - \mu^k_{S,t} < \Delta u^k_t = u^k_{O,t} - u^k_{S,t}$ leave Sweden (or stay out of Sweden), while individuals move into Sweden (or stay in Sweden) when the opposite is true. Wealth taxation affects location decisions by affecting the threshold level of utility Δu^k below which individuals decide to be in or out of Sweden. We assume that idiosyncratic shocks $\mu^k_{l,t}$ are independent, and taken from the same distribution $F(.)$ every period. These shocks generate constant expected migration flow rates in and out of Sweden every period when earnings and taxes are stationary (i.e. when $\Delta u^k_t = \Delta u^k_{t-1},\ \forall t$). We define the net expected out-migration flow rate α^k_t as the expected rate of out-migration minus the expected rate of in-migration into Sweden of individuals of age k. Based on our assumptions, α^k_t are constant over time ($\alpha^k_t = \alpha^k_{t-1},\ \forall t$) when earnings and taxes are stationary.

VI.3. Impact of Wealth Tax Changes on the Steady State Population Stock

Our framework focuses on the quantification of the extensive margin effects of wealth taxation. We therefore abstract away from effects of the wealth tax on the birth rate into the wealthy population through intensive margin effects. We consider a simple version of the steady state population where birth rates B_t are constant over time ($B_t = B,\ \forall t$).

At the steady state, the population stock N is therefore given by:

$$N = B \cdot \sum_{k=0}^{T} \prod_{j=0}^{k} (1-\alpha^{k-j}) = B \cdot \sum_{k=0}^{T} S(k) = B \cdot D \tag{8}$$

where D is the average time that a wealthy individual spends living in Sweden over her lifespan.

We are now considering the impact of change in the net-of-tax rate on wealth in Sweden $d(1-\tau_S)$. For convenience, we will be dropping the subscript S for the tax rate going forward, and always refer to the tax rate on wealth *in Sweden* when writing τ.

The elasticity of the population stock N with respect to the net-of-tax rate on wealth in Sweden is:

$$\frac{dN/N}{d(1-\tau)/(1-\tau)} = \frac{B}{N} \cdot \sum_{k=0}^{T} \frac{\partial(\prod_{j=0}^{k}(1-\alpha^{k-j}))}{\partial(1-\tau)/(1-\tau)} \tag{9}$$

We define $\xi^j = \frac{\partial(1-\alpha^j)}{\partial(1-\tau)} \cdot \frac{1-\tau}{1-\alpha^j}$. This is the elasticity of the instantaneous probability of remaining in Sweden w.r.t the net-of-tax rate on wealth.

Note that because α^j is small, $1-\alpha^j \approx 1$. This means that $\xi^j \approx \frac{-\partial\alpha^j}{\partial(1-\tau)/(1-\tau)} = -\varepsilon^j$.

In other words, an empirical analog of ξ^j is the opposite of the semi-elasticity of the net-migration rate w.r.t the net-of-tax rate on wealth ε^j. The latter is the semi-elasticity that we estimate in specification (4) in section 5 of the paper.

We now decompose $\frac{dN/N}{d(1-\tau)/((1-\tau)}$ into the effect of the tax change on the population of each age k:

Percentage effect on population of age 0	$\frac{dN^0/N^0}{d(1-\tau)/(1-\tau)}$	$=$	0
Percentage effect on population of age 1	$\frac{dN^1/N^1}{d(1-\tau)/(1-\tau)}$	$=$	$\frac{\partial(1-\alpha^1)}{\partial(1-\tau)} \cdot \frac{1-\tau}{1-\alpha^1} = \xi^1$
Percentage effect on population of age 2	$\frac{dN^2/N^2}{d(1-\tau)/(1-\tau)}$	$=$	$\xi^1 + \xi^2$
		...	
Percentage effect on population of age k	$\frac{dN^k/N^k}{d(1-\tau)/(1-\tau)}$	$=$	$\sum_{j=1}^{k} \xi^j$

This allows us to rewrite equation (9) as:

$$\frac{dN/N}{d(1-\tau)/(1-\tau)} = \sum_{k=0}^{T} \frac{S(k)}{D} \cdot \sum_{j=1}^{k} \xi^j \tag{10}$$

Discussion Equation (10) shows that the effects of a change in the wealth tax on the net flows of migration do cumulate over time. The percentage effect on the population of age 1 is $\xi^1 \approx -\varepsilon^1$, the percentage effect on the population of age 2 is $\xi^1 + \xi^2 \approx -\varepsilon^1 - \varepsilon^2$, etc. This is quite intuitive: if individuals leave at age k, they are not available in the

population of wealthy individuals in Sweden for the subsequent periods of their lives. As a consequence, it matters if the effects on out-migration are concentrated on younger or older individuals. If younger individuals move out of Sweden, this will tend to have a larger effect on the population stock in the steady-state, because they are going to be missing for all the subsequent periods when they could have been alive and in Sweden. If older individuals tend to move instead, this will have a smaller effect on the population stock.

The interest of formula (10) is that it connects easily to our data and empirical setting. Indeed, we can use the fact that $\xi^j \approx -\varepsilon^j$ and rewrite:

$$\frac{dN/N}{d(1-\tau)/(1-\tau)} \approx \sum_{k=0}^{T} \frac{S(k)}{D} \cdot \sum_{j=1}^{k} -\varepsilon^j \tag{11}$$

We can thus provide a direct estimate of the elasticity of the population stock based solely on measures of D and S which we can observe in the data, and based on our estimates of the effects of the wealth tax on the net out-migration flows at age k, ε^j. These are the estimates that we get from a regression like specification (4) in section 5 of the paper. In Figure 5 of the paper, we show results where we implemented this specification for the net-migration rate α of the population on average, and also results where we look at heterogeneity in the migration rate effect ε^k by age k.

Simplified Formula and Intuition Results in section 5 of the paper suggest that semi-elasticities ε^k are approximately constant. That is, the semi-elasticity of the net-migration rate α w.r.t to the net-of-tax rate does not seem to vary with age k. Using this, and the fact that $S/D \approx 1/T$ we can get a simplified formula for the effect of a change in the wealth tax rate on the size of the population of wealthy individual:

$$\frac{dN/N}{d(1-\tau)/(1-\tau)} \approx -\varepsilon \cdot \frac{(T+1)}{2} \tag{12}$$

This formula can be implemented simply with our estimate of the average semi-elasticity of net out-migration flows $\hat{\varepsilon}$ from specification (4) in section 5 and a measure of T, the average "lifespan" of a wealthy individual. Note that T in the steady-state is directly related to the birth rate of individuals into the population of wealthy individuals: $B = 1/T$. The longer individuals' lifespan in the wealthy population, the lower the net birth rate in

the population of wealthy individuals, i.e. the lower the rate at which the population of wealthy individuals regenerates.

Formula (12) has a simple interpretation: to get an estimate of the effect on the population stock N of wealthy individuals, we simply need to 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 a wealthy individual to migration, it is harder to replace her.

Calibrations We start by implementing our simplified formula. To calibrate T, we measure in our data the average time spent in the top 2% of the wealth distribution. We find that on average wealthy individuals spend 15.1 years of their lives in the top 2% of the wealth distribution. We check empirically that this estimate of T matches the hazard rates in and out of the top 2% of the wealth distribution that we observe every year. These estimates imply that around 30% of the population of the top 2% is replaced every five years. This is due to the steep lifecycle profile of wealth. For ε (i.e. the average semi-elasticity of net out-migration flows), we take the sum of the out-migration and in-migration semi-elasticities from Figure 4 in the main text.[6]

Simplified Formula: Calibration 1 *The estimated elasticity of the stock of the population of wealthy individuals with respect to the net-of-tax rate on wealth is:*

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

Note that because τ is small, $1-\tau \approx 1$, and so $d(1-\tau)/(1-\tau) \approx -d\tau$. As a result, the elasticity can also be interpreted as the percentage change in the size of the wealthy population when the effective tax rate on wealth is increased by 1 percentage point.

This estimate confirms that, even when we properly cumulate the very small flow effects to account for the steady state effect on the stock of the population, migration effects remain extremely modest. In the long run, a 1 percentage point increase in the effective tax rate on wealth would only decrease the size of the population of the very wealthy in Sweden by 1.77 % due to migration responses. One should note that a 1 percentage point increase in the effective tax rate on wealth is a large increase. The abolition of the wealth tax in Sweden, which is one of the largest wealth tax reforms ever studied amounted to a

[6]Note that for Sweden, we do not have an estimate of the semi-elasticity of in-migration flows. Given the estimates for out-migration flows are very similar for Sweden and Denmark, we therefore take the Danish estimate of the semi-elasticity of in-migration flows.

0.5 percentage point decrease in the effective tax rate. In other words, the abolition of the Swedish wealth tax only increased the long run population of the wealthy in Sweden by about 1%, which is small.

We can also calibrate our exact formula (10) using estimates of semi-elasticity of migration flows ε^k by age. We find an estimated elasticity of population size of 1.88 (1.26), which is almost identical to our simplified formula.

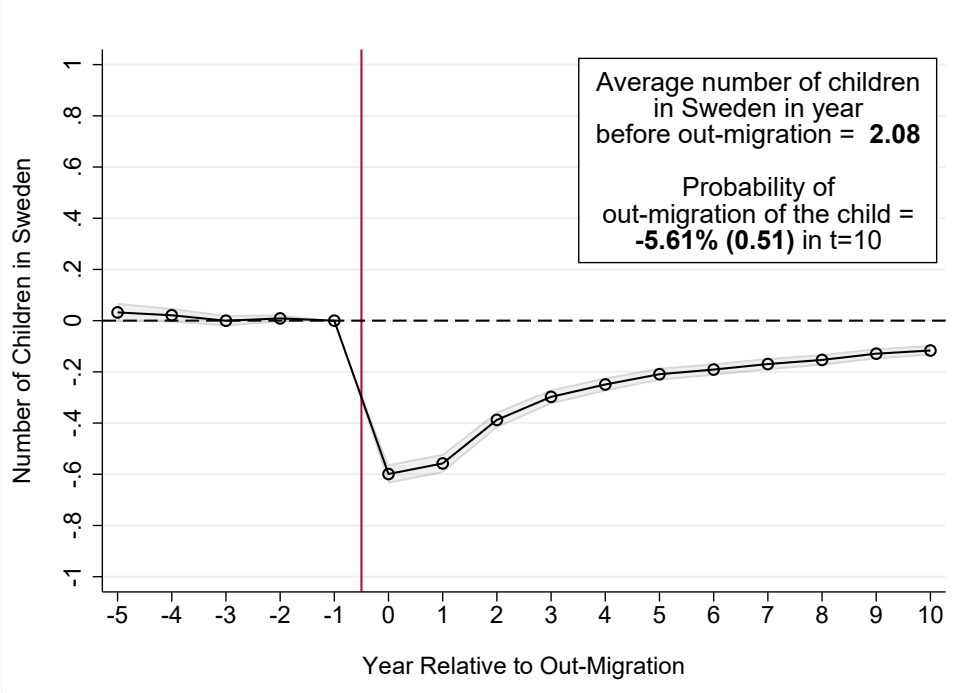
Dynastic effects Our approach so far assumes that there is no effect of migration on the birth rate into the wealthy population. In practice though, individuals who have had wealthy parents are more likely to become wealthy. This creates a dynastic connection between wealthy individuals today and wealthy individuals tomorrow. And if children of wealthy parents are also out-migrating with their parents, out-migration responses today may also affect the birth rate into the wealthy population, potentially increasing the effect on population size. Accounting for these dynastic connections, the formula for the semi-elasticity becomes:

$$\frac{dN/N}{d(1-\tau)/(1-\tau)} = \frac{\partial B}{\partial(1-\tau)} \cdot \frac{1-\tau}{B} + \sum_{k=0}^{T} \frac{S(k)}{D} \cdot \sum_{j=1}^{k} -\varepsilon^j \tag{13}$$

where, compared to our baseline formula, we now need to add the elasticity of the birth rate into the wealthy population with respect to the net-of-tax rate on wealth. The size of this elasticity will depend on the importance of these dynastic effects: if all new wealthy individuals are heirs of wealthy parents, and if heirs systematically outmigrate with their parents, then this could significantly increase the impact of wealth taxes on the population size.

Importantly, we can directly calibrate formula (13) and assess how these dynastic effects affect our baseline results. First, we start by estimating the probability that children of wealthy parents out-migrate when their parents do so. In Figure VI.1 below, we report the results of an event study similar to specification (7) in the main text. The event is out-migration of a wealthy individual in the top 2% of the wealth distribution. The outcome is a dummy equal to one if one of her children is living in Sweden. The graph shows precisely estimated but small effects of parental migration parents on the out-migration probability of their children. Ten years after her wealthy parent has out-migrated, the probability that a child also remains out of Sweden increases by just 5%.

Figure VI.1: **Event Study: Probability for Children of Wealthy Parents to Live in Sweden Around the Event of Their Parents' Out-Migration**



Notes: This figure shows the probability for children of wealthy parents (in the top 2% of household net wealth distribution) to live in Sweden around the event of their parents' out-migration. We plot the coefficients β_j (and their confidence intervals) estimated from Equation 7 in the main text, where the outcome is the number of children residing in Sweden in a given year. The effect displayed in the text boxes is the estimate of β_{10} divided by the average outcome in the treatment group in $t = -1$, multiplied by 100.

To get an estimate of the impact of these dynastic effects on birth rates, we now simply need to multiply the migration elasticity for the population of parents by the estimate of the impact of parental migration on children migration.

We find a total effect on birth rates into the wealthy population of $\frac{\partial B}{\partial(1-\tau)} \cdot \frac{1-\tau}{B} = .001$. And calibrating formula (13), we find an estimated elasticity of population size of 1.87 (.50), almost identical to our original estimate. In other words, although we can clearly detect the presence of dynastic migration effects, these effects are very small and do not affect significantly our baseline estimate of the elasticity of population size with-respect-to wealth taxation.

Robustness to Tax Evasion One potential concern with our estimates of migration responses to the wealth tax is that we may have mismeasurement in the denominator $d\tau$ due to tax evasion. For instance, 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 is evaded. And as a result, our measure of tax rates τ is an overestimate of their effective tax rates on wealth $\tilde{\tau} = (1-e)\tau$. A corollary is that our estimated elasticities may overestimate the true elasticity with respect to the effective net-of-tax rate $(1-\tilde{\tau})$:

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

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 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.[7] 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 main qualitative message of our baseline results, namely that the impact of wealth taxes on the size of the population of the wealthy is small.[8]

Elasticity w.r.t Net-of-Tax Rate on Capital Income While our estimated elasticities of population with respect to the net-of-tax rate on wealth have an intuitive interpretation, their magnitude can be hard to compare to existing estimates of migration elasticities, which are typically expressed with respect to the net-of-tax rate on income. It is however easy to convert our estimate into an elasticity of population with respect to the net-of-tax rate on capital income. For this, we simply compute the change in capital income taxation induced by the wealth tax. To do this, we need to compute the implied capital income that wealth taxpayers receive each year out of their total net wealth and add it to the tax rate t^K they pay on their other capital income. We define $t \approx \tau/r + t^K$ the average tax rate on capital income. Over our period of study, we observe in our data $r = .042$ and $\tau \approx .006$ which translates into $t = 14.3\% + 20\%$. Therefore, we obtain:

[7] 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.

[8] As we pointed in Kleven et al. (2020), the availability of tax evasion opportunities through wealth offshoring may actually structurally affect migration elasticities downwards, as it makes it cheaper to move one's wealth rather than residence to avoid the burden of taxation.

$$\frac{dN/N}{d(1-t)/(1-t)} = \frac{dN/N}{d(1-\tau)/(1-\tau)} \cdot \frac{d\ln(1-\tau)}{d\ln(1-t)} \approx .049 \ \ (.013) \tag{14}$$

When appropriately rescaled, the implied migration elasticity is thus small. Panel B of Figure 5 compares our estimates to migration elasticities available in the literature, which come from two strands of papers. The first focuses on migration elasticities of top income earners, the second, much less developed, investigates migration responses to capital taxation, but relies exclusively on intra-national variation across local jurisdictions. Two insights emerge from the comparison. First, our estimates accord in magnitude to cross-border migration elasticities of top incomes. These elasticities are typically found to be quite small, around .1, except when focusing on specific subsegments of the labor force such as foreign nationals and expatriates. Second, our elasticity is substantially smaller than migration elasticities to capital taxation obtained by Agrawal et al. (2023) or Brülhart et al. (2022), who investigate intra-national migration of wealthy taxpayers in Spain and Switzerland respectively. This is consistent with the fact that we focus on international migration responses, rather than within-country moves, and that we measure real relocation responses, rather than avoidance through artificial changes in reporting, for instance.

VI.4. Aggregate External Effects on Employment and Economic Activity

We explain here how we combine our estimates of migration responses to the wealth tax with our estimates of the persistent impact of migration on individual and firm-level outcomes to quantify the aggregate effects of wealth taxation on employment, investment and business dynamism.

VI.4.1. Formula & Heterogeneous Treatment Effects of Migration

Our approach is based on a simple decomposition of the effect of wealth tax-induced migration on any aggregate outcome Y into three terms:

$$\frac{dY/Y}{d(1-\tau)/(1-\tau)} = \underbrace{\frac{\partial N}{\partial(1-\tau)} \cdot \frac{1-\tau}{N}}_{\text{Migration Elasticity}} \times \underbrace{\frac{\partial Y}{\partial N} \cdot \frac{1}{Y^w}}_{\text{Migration Impact}} \times \underbrace{\frac{NY^w}{Y}}_{\text{Top Wealth Share of Y}} \tag{15}$$

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.

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:

$$\frac{\partial Y}{\partial N} \cdot \frac{1}{Y^w} = \underbrace{\left.\frac{\partial Y}{\partial N}\right|_m \cdot \frac{1}{Y^m}}_{\text{Migration Impact for Marginals}} \times \underbrace{\frac{Y^m}{Y^w}}_{\text{Selection into Migration}} \tag{16}$$

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.

For this reason, we now probe into the presence of heterogeneous treatment effects.

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 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 infra-marginal 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.[9]

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.[10] 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

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

[10] 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.

this should alleviate concerns that treatment effects of migration are strongly different among compliers, we now test for treatment effect heterogeneity more formally.

In the main text, in Table 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 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 2.

To compute this LATE, we proceed as follows. To start, we know that the estimate in the pre-abolition period β^{pre} is a weighted average of β^m, the treatment effect of migration for individuals who migrate because of the wealth tax (the "marginals"), and β^i, the effect for individuals who would migrate irrespective of the wealth tax ("infra-marginals"). The respective weights are simply ω and (1-ω), where ω is the fraction of migrants who migrate because of the wealth tax, so

$$\beta^{pre} = \omega\ \beta^m + (1-\omega)\ \beta^i$$

After the repeal of the wealth tax, the estimate β^{post} captures β^i directly, the effect for the "infra-marginals" only, i.e.

$$\beta^{post} = \beta^i$$

Note that we have identified ω in section 5 of the paper: estimating the causal impact of the abolition of the wealth tax on migration flows, we found that one third of flows disappeared after the repeal of the wealth tax (i.e. ω = .30)[11]. Our estimates of ω, β^{pre} and β^{post} allow us to back-out migration effects for "marginals", as re-arranging the above gives

$$\beta^m = \left(\beta^{pre} - (1-\omega)\ \beta^{post}\right) * \omega^{-1} \tag{17}$$

So far, our approach assumes that β^m and β^i are the same in the pre-abolition and post-abolition periods. We can relax this assumption, and compute an alternative LATE, which allows for aggregate dynamics in the treatment effects. To do so, we additionally estimate a β^{pre} and β^{post} for our control group (individuals in the top 20-10% of net wealth).

[11] Replicating this analysis focusing on wealthy entrepreneurs only, we find ω = .51. We use this estimate when computing the LATE for the 'Firm-level' outcomes and characteristics in Table 2 and Table VI.1.

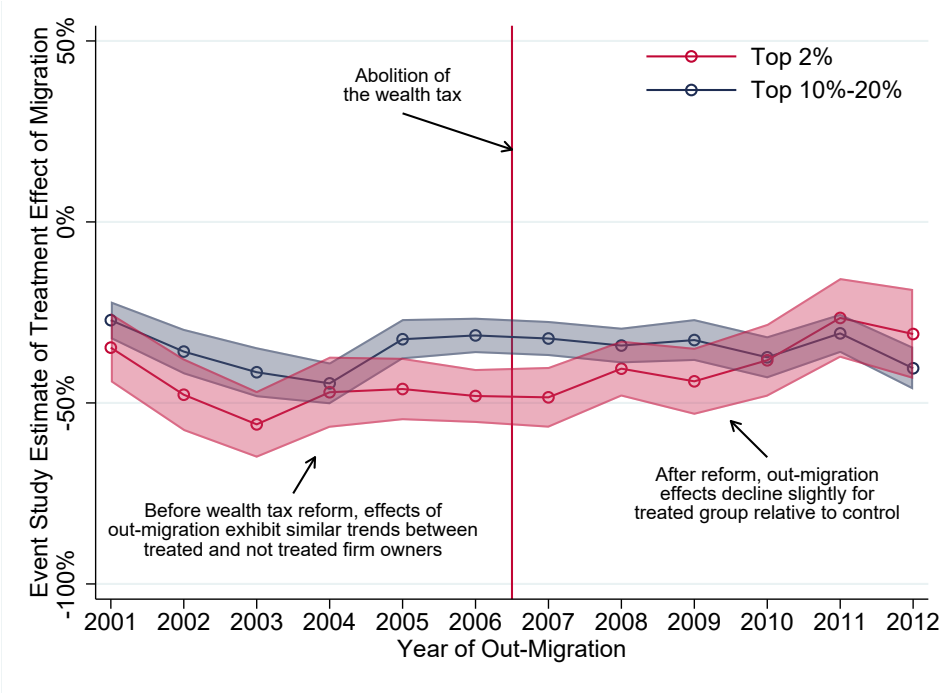
Since these out-migrants are not affected by the abolition, there are no "marginals". If there are aggregate dynamics in treatment effects, they will be captured by these estimates. As such, we can estimate an event-study version of a Difference-in-Difference-in-Difference-in-Differences specification: the difference-in-differences in treatment effects pre-abolition and post-abolition, for the treatment group versus the control group. Specifically, we estimate the following DDDD version of specification (7):

$$\begin{aligned} Y_{it} = & \sum_{j \neq -1} \beta_j^{DDDD} \cdot \mathbb{1}[M_i = 1] \cdot \mathbb{1}[t = j] \cdot \mathbb{1}[P_i = 1] \cdot \mathbb{1}[T_i = 1] + \\ & \sum_{j \neq -1} \gamma_j^1 \cdot \mathbb{1}[M_i = 1] \cdot \mathbb{1}[t = j] \cdot \mathbb{1}[P_i = 1] \; + \; \ldots \; + \\ & \sum_{j \neq -1} \delta_j^1 \cdot \mathbb{1}[M_i = 1] \cdot \mathbb{1}[t = j] \; + \; \ldots \; + \\ & \sum_{j \neq -1} \theta_j^1 \cdot \mathbb{1}[t = j] \; + \; \ldots \; + \; \varepsilon_{it} \end{aligned} \tag{18}$$

where P_i is a dummy indicating if an out-migration happens post-abolition and T_i is a dummy indicating if an out-migrant is in the treatment group. All the lower-order interaction terms are included in the regression. Here, β^{DDDD} gives us the difference in treatment effects post-abolition, after subtracting overall dynamics in treatment effects.

Appendix Figure VI.2 provides a simple graphical illustration of this double difference approach, focusing on one outcome, total tax payments. This graphical illustration enables us to test for the validity of the parallel trend assumptions, at the heart of identification. For each year, we plot the coefficient estimates of the event study for all migrations happening in that year. We do this separately for individuals belonging to the treatment group (i.e. in to the top 2% of wealth, in red) and for individuals belonging to the control group (i.e. the top 20 to top 10% of wealth, in blue). We find that the effects of migration are quite stable over time in the pre-abolition period, and evolve similarly for the treated and control group of migrants. After the abolition, we observe a small decline in the magnitude of the effects of migration among the treated group, while the estimates remain unchanged for control migrants. The *double difference* gives therefore very comparable results to the simple difference approach. The LATE estimates for compliers derived from the double difference approach are reported in column (5) of Table 2, and are very similar to the estimates from column (4). In the next section, we use column (5) as our preferred estimates of migration impacts among the wealth tax compliers.

Figure VI.2: **Event-study estimates of the impact of migration on total tax payments, by year of migration, for the treatment and control group in the wealth tax reform natural experiment**



Notes: This figure reports the coefficient estimates of the event study for all migrations happening in that year. We focus on individual total tax payments. We do this separately for individuals belonging to the treatment group (i.e. in to the top 2% of wealth, in red) and for individuals belonging to the control group (i.e. the top 20 to top 10% of wealth, in blue).

We can then compute the LATE as

$$\beta^{\tilde{m}} = \left(\beta^{pre} - (1-\omega)\left(\beta^{pre} + \beta^{DDDD}\right)\right) * \omega^{-1} \tag{19}$$

This is the second LATE that we report in column (5) of Table 2 in the main text.

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.

Table 2 conveys three important insights:

(i) For almost all outcomes, we cannot detect statistically significant differences between the effects pre and post abolition. For only one out of the 10 outcomes the difference is statistically significant at the 5% level.

(ii) This in turn implies that for almost outcomes, we cannot reject that migration effects are the same for "marginals" and "non-marginals".

(iii) For most outcomes however, the point estimates for pre-abolition effects tend to be slightly larger than for post-abolition. As a result, the point estimates of the LATE for our marginals tend to be somewhat larger than the estimates for the pre-abolition period. Even though these LATE estimates are a bit imprecise, we use them as our baseline to calibrate equation 15.

Table VI.1: **Test of Equality of Out-Migration Characteristics Before and After the Repeal of the Wealth Tax**

Characteristic	Top 2%	Top 2% Out-Migrants *Pre-Abolition*	Top 2% Out-Migrants *Post-Abolition*	T-Statistic	Compliers (Marginal Migrants)
	(1)	(2)	(3)	(4)	(5)
A. Mean Individual Characteristics					
Sample of individuals in the top 2% of net worth					
Age	**61.89**	**48.98**	**50.23**	**−3.65*** **	**46.06**
	(0.02)	**(0.24)**	**(0.25)**		**(0.98)**
Higher Education (%)	**43.60**	**61.90**	**65.86**	**−2.95*** **	**52.65**
	(0.09)	**(0.94)**	**(0.96)**		**(3.84)**
Cognitive Skills Percentile	50.17	63.11	60.34	1.86*	69.58
	(0.14)	(1.09)	(1.02)		(4.34)
Non-Cog. Skills Percentile	50.16	61.49	60.42	0.67	63.97
	(0.15)	(1.16)	(1.09)		(4.64)
Foreign Born (%)	1.62	10.20	11.38	-1.35	7.46
	(0.02)	(0.58)	(0.64)		(2.46)
Owns Active CHB (%)	**7.08**	**8.98**	**6.39**	**3.50*** **	**15.02**
	(0.05)	**(0.55)**	**(0.49)**		**(2.17)**
Wealth Rank Within Top 2%	**50.00**	**50.28**	**48.35**	**2.02** **	**54.78**
	(0.05)	**(0.68)**	**(0.67)**		**(2.75)**
B. Mean Firm Characteristics					
Sample of firms with owner in the top 2% of net worth					
Number of Employees	22.57	22.81	22.55	0.16	23.06
	(0.31)	(1.00)	(1.26)		(2.28)
Value Added (SEK 1,000)	10,340.92	11,698.29	11,185.00	0.57	12,183.62
	(162.71)	(554.90)	(707.21)		(1,269.89)
Net Turnover (SEK 1,000)	38,690.96	41,877.01	40,315.60	0.49	43,353.36
	(711.61)	(1,994.41)	(2,490.15)		(4,538.66)
Tax Payments (SEK 1,000)	501.58	434.88	375.01	1.01	491.48
	(14.38)	(36.98)	(46.57)		(84.35)
Gross Investments (SEK 1,000)	1,646.18	1,032.77	877.61	1.45	1,179.48
	(45.78)	(71.07)	(80.33)		(157.76)

Notes: This table compares the characteristics of wealthy out-migrants (in the top 2% of household net wealth), and firms owned by wealthy out-migrants, in Sweden, who migrate before (Column (2)) and after (Column (3)) the abolition of the wealth tax (from 2001-2006 and 2007-2012, respectively). We present the group average of these characteristics in $t = -1$, i.e. the year before out-migration. Column (4) reports a t-statistic of a test of equality of these averages. For the firm characteristics, we focus on active firms which are either directly or indirectly held by wealthy out-migrants. We winsorized the top 5% and bottom 1% of these firm characteristics (except for number of employees, which we winsorized only the top 5% of). As we need to measure characteristics post-abolition, we use predicted wealth to define exposure to the reform here. For comparison, Column (1) shows the average pre-reform characteristics within the entire top 2% (i.e. without restricting to out-migrants) and the firms owned by them. In Column (5), we compute the average characteristics of marginal out-migrants using the methodology detailed in Appendix VI.4. We use the post-reform characteristics to infer the average characteristics of the infra-marginal out-migrants. Specifically, Column (5) reports the $\bar{Z}^{m}_{t-1}$ from the following equation: $\bar{Z}^{m}_{t-1} = \left(\bar{Z}^{pre}_{t-1} + (1-\omega)\,\bar{Z}^{post}_{t-1}\right) * \omega^{-1}$, where $\bar{Z}^{pre}_{t-1}$ and $\bar{Z}^{post}_{t-1}$ are the average characteristics in $t-1$ of wealthy out-migrants and firms owned by wealthy out-migrants pre-abolition and post-abolition, respectively, and ω is the fraction of migrants who migrate because of the wealth tax.

VI.4.2. Results & Robustness

Results of our calibrations of equation 15 are reported in Figure 10. The Figure shows a baseline calibration, and alternative calibrations for robustness. We explain the details of these calibrations below.

- Baseline calibration: For $\frac{\partial N}{\partial(1-\tau)} \cdot \frac{1-\tau}{N}$, this baseline calibration uses 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 12, with $T = 15.1$. For the impact of migration, we use estimated out-migration effects for treated entrepreneurs coming from the LATE estimates in Table 2 and we do similarly for in-migration effects. 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, i.e. $w_{in} = \frac{\varepsilon_{in}}{\varepsilon_{in}+\varepsilon_{out}}$ and $w_{out} = \frac{\varepsilon_{out}}{\varepsilon_{in}+\varepsilon_{out}}$. Finally, the estimates for the top wealth share (i.e. the fraction of total outcome due to firms with owners in the top 2%) come from Table 1, Panel C. The standard errors of the final estimated aggregate effect are again computed using the delta method, where we assume zero covariance between the migration elasticity and migration effects.

- Alternative calibration 1: The migration effects estimated for the specific subset of "marginals" using Table 2 are not incredibly precise. Besides, results suggest that migration effects are not statistically different for marginals and infra-marginals. In light of this, and to gain in precision, in this first alternative calibration, we estimate the average migration impacts over the whole sample of treated entrepreneurs, rather than retrieving the LATE for marginal migrants. As such, we replace the estimates in the baseline calibration with those from Figure 8 (for directly and indirectly held firms).

- Alternative calibration 2: In the same spirit of gaining in terms of precision, in this second alternative calibration, we use the out-migration elasticity estimated over the whole population which is much more precisely estimated than the elasticity for entrepreneurs only. These estimates come from the 'Top 2%' coefficient in Figure 4 and are for the entire treated group, i.e. individuals with predicted household wealth in the top 2%.

- Alternative calibration 3: In the third and final alternative calibration, we account for firms' buy-outs after closure. The out-migration treatment effects are given by

taking the estimates accounting for directly and indirectly held firms and buyouts from Figure 8.

For each of these alternative calibrations, all parameters remain the same as in the baseline unless explicitly specified.