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TAXING TOP WEALTH:
MIGRATION RESPONSES AND THEIR AGGREGATE ECONOMIC IMPLICATIONS

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ABSTRACT

Using administrative data on wealth, firm ownership structure, and migration in Sweden and Denmark, we document international migration patterns among the very wealthy, their impact on the economy, and how they respond to wealth taxation. We show that more than 20% of taxpayers liable to pay wealth tax are business-owners, and that the employment, investments, and value-added of these businesses are negatively affected when their owner migrates out of the country. Exploiting three large reforms, we then isolate the causal effect of wealth taxation on the international location choices of the wealthy. We find significant effects on out-migration flows from increases in the effective wealth tax. But, we also document that the overall level of these migration flows is remarkably small, with annual net-migration rates below .01%. As a result, we find that the aggregate economic effects of tax-induced migration are modest in Scandinavia: a one percentage point increase in the average wealth tax rate on the top 2% decreases the stock of wealthy taxpayers by at most 2% in the long run, and lead to a reduction of at most .03% in aggregate employment and at most .1% in aggregate value- added. Hence, our results suggest that trickle-down effects of tax-induced migration by the wealthy do exist, but that they are quantitatively small.

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1 Introduction

Following recent proposals in the US and Europe to raise wealth taxation on the rich (Saez and Zucman, 2019), a growing literature on wealth taxes has emerged (Seim, 2017; Duran-Cabré et al., 2019; Jakobsen et al., 2020; Brülhart et al., 2022; Londoño-Vélez and Avila-Mahecha, 2023). However, this literature is silent on the possibility of international migration responses to wealth taxes, which has been a key concern in the public debate.

Most studies of tax-induced mobility have focused on the effects of personal income taxation on top earners (Kleven et al., 2013, 2014; Akcigit et al., 2016; Muñoz, 2023). The effects of wealth taxation on top wealth holders may be very different. First, top wealth holders differ from top earners along various dimensions (such as age and family structure). If these characteristics are associated with different location preferences and moving costs, the migration effect of wealth taxation will differ from that of income taxation. Second, avoidance and evasion opportunities differ for wealth and income. It may be easier to move wealth than personal income, reducing the incentive to avoid wealth taxes through individual migration (Kleven et al., 2020). Third, a large portion of top wealth holders are entrepreneurs and business owners. In Sweden, almost 40% of individuals in the top 0.1% of the wealth distribution own companies. If the expatriation of wealthy entrepreneurs influences the economic activity of their firms in the home country, migration responses to wealth taxes could have important trickle-down effects on the economy.

International migration responses to wealth taxes are understudied due to a lack of micro data on wealth, migration, and entrepreneurship combined with the absence of credible identifying variation. This is because relatively few countries have taxed wealth, and even fewer countries collect high-quality data on personal wealth and international migration. Furthermore, linking wealthy individuals to their firms is typically infeasible, preventing an estimation of the full economic effects of out-migration at the top of the wealth distribution. A few recent exceptions have managed to overcome some of these challenges (Brülhart, Gruber, Krapf and Schmidheiny, 2022; Agrawal, Foremny and Martínez-Toledano, 2023; Moretti and Wilson, 2023) and credibly identify migration responses to wealth taxation.[1] However, those studies focus on within-country migration and cannot measure economic spillovers of migration.

In this paper, we leverage administrative records on wealth, entrepreneurship, and international migration in Sweden to document the migration patterns of the very wealthy, their responsiveness to wealth taxation, and the implications of their expatriation for the aggregate economy. Sweden

[1] Agrawal, Foremny and Martínez-Toledano (2023) compare the number of wealthy filling taxes in Madrid and non-Madrid after a change in wealth taxation in Madrid; Brülhart et al. (2022) use the same strategy comparing two cantons in Switzerland. In the U.S., Moretti and Wilson (2023) investigate Forbes 400 individuals cross-states location responses to the estate tax, a one-time wealth tax at death; while Dray, Landais and Stantcheva (2023) focus on the introduction of property taxes in the 19th century.

has had a progressive wealth tax since the beginning of the 20th century. The comprehensive data collected by the Swedish government allow us to track the international migration history of the entire population, while observing all components of taxable and non-taxable wealth, including business assets (in listed and unlisted firms) controlled directly or indirectly by the wealthy.[2] We use this data to provide a detailed description of the implications of out-migration by the wealthy for tax revenue and real economic activity. We then exploit the repeal of the Swedish wealth tax in 2007 to causally identify international migration responses to wealth taxation.

Our data allow us to document three important facts for the study of migration responses to wealth taxes. First, entrepreneurs are disproportionately represented among wealthy taxpayers: 19% of individuals in the top 2% of the wealth distribution own a firm, as opposed to less than 3% in the bottom half. Second, firms owned by the wealthy are larger and often serve as parent companies of other firms. Individuals in the top 2% of the wealth distribution control through their privately held businesses close to 10% of total Swedish employment and 15% of total Swedish value added. Without our data on ownership links, we would underestimate the economic activity linked to the firms held by the wealthy by a factor of three. Third, our migration register shows that migration events reflect real mobility responses rather than simple changes in tax residency. Having established these facts, our analysis proceeds in three steps.

In the first step, we document international migration patterns among the wealthy when wealth is taxed. We show that, in contrast to public discourse, international migration flows are extremely small in this population. In Sweden, about 0.2% of individuals in the top 2% of the wealth distribution (those subject to the wealth tax) leave the country each year. The taxable wealth of these out-migrants represents only 0.09% of total taxable wealth in Sweden. These out-migration flows are matched by in-migration flows of a roughly similar magnitude. In the 8 years prior to the abolition of the wealth tax, net migration flows at the top of the distribution were on average positive in Sweden. We also study selection into migration at the top of the wealth distribution: we find negative selection on age, education, and cognitive ability. However, wealthy individuals are more likely to out-migrate when they own a business. This suggests that entrepreneurs may be sensitive to wealth taxation and the effects on firm outcomes therefore deserve special attention.

In a second step, we estimate the economic implications of out-migration by the very wealthy. We compare, in an event-study design, the evolution of different outcomes for wealthy individuals who leave Sweden, relative to wealthy individuals who stay. We find large declines in tax payments and significant portfolio reallocation around migration events. We also find that migration

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

events are associated with a significant reorganization of the business assets owned by the wealthy. We show that, when an entrepreneur subject to the wealth tax out-migrates, employment in their businesses drops by 33%, value-added by 34%, tax payments by 50%, and gross investments by 21%. These effects are driven mostly by the extensive margin: the probability that a firm remains alive decreases by 27% after its wealthy owner leaves Sweden. The public discourse often focuses on what would happen to the firms controlled by wealthy taxpayers if they were to leave. Our data enable us to delve beyond such firm-level effects and explore the reallocation of economic activity within Sweden following the out-migration of wealthy entrepreneurs. We find substantial reallocation: 60% of the firms closed by their wealthy owners upon out-migration end up being absorbed by other companies in Sweden, and employees at these firms experience limited persistent losses in labor earnings or employment prospects. Overall, our results indicate that the impact of wealthy entrepreneur expatriation on aggregate domestic activity is mitigated by reallocation forces in the Swedish labor market.

In the third and final step, we estimate the causal effect of wealth taxation on the location decisions of wealthy households. We exploit the unexpected repeal of the Swedish wealth tax following a surprise win of the right coalition in the 2006 elections. Using a difference-in-differences design, we find clear evidence of international migration responses to the reform. After the repeal of the wealth tax, out-migration flows dropped dramatically for taxpayers exposed to the wealth tax shock relative to those who were unexposed. We estimate that the reform reduced the propensity of wealthy taxpayers to leave Sweden by more than 30%. We relate those migration responses to the actual changes in effective wealth tax rates faced by wealthy taxpayers. We find that a one percentage point increase in the effective wealth tax rate increases out-migration rates of wealthy taxpayers by .17 percentage point and reduces in-migration rates by .05 percentage point.

A unique feature of our study lies in the ability to replicate the same analysis exploiting two large wealth tax reforms in Denmark. We find remarkably similar migration elasticities with respect to the wealth tax rate in the Danish context. This provides support for the internal and external validity of our results.

In the last section of the paper, we put the three parts of our empirical analysis together in order to draw policy implications. We first translate our estimated effects on migration flows into effects on the stock of wealthy individuals and the wealth tax base. We find that the stock elasticity is small: a one percentage point increase in the effective wealth tax rate decreases the stock of wealthy taxpayers by less than 2 percent in steady state. As for the revenue implications, we show that, even when accounting for the fiscal externalities on other tax bases, wealth-tax induced migration responses alone are much too small to make the abolition of the wealth tax pay for itself. Finally, we combine our estimates from the second and third parts of the analysis to compute the

aggregate economic implications of wealth-tax induced migration, accounting for the effects on the businesses controlled by those relocating. The main insight is that the effects on overall economic activity are extremely limited. A one percentage point increase in the effective wealth tax rate on the top 2% decreases total employment by .03%, total investment by .04%, and total value-added by only .09%. This is despite the fact that wealthy entrepreneurs account for a substantial share of overall economic activity through the firms they control directly and indirectly. The reason for such small effects lies in the small migration elasticity, which is largely due to the fact that migration flows at the top of the wealth distribution are tiny.

Our paper contributes to various strands of literature. First, we contribute jointly to the nascent literature on behavioral responses to wealth taxation (e.g., Seim, 2017; Duran-Cabré et al., 2019; Jakobsen et al., 2020) and to the literature on migration responses to taxation. The former is virtually silent on international migration responses despite their prominence in the public debate. The latter is mostly focused on migration response to income taxes among top earners (see Kleven et al., 2020 for a review). Evidence on migration among top wealth holders is scarce and almost entirely focused on within-country mobility (Brülhart, Gruber, Krapf and Schmidheiny, 2022; Agrawal, Foremny and Martínez-Toledano, 2023; Moretti and Wilson, 2023; Iacono and Smedsvik, 2023).[3] Top wealth holders tend to own businesses—much more so than top income earners—and studying their international migration patterns is therefore critical. The notion that tax-induced migration of wealthy entrepreneurs will have large spillovers on business activity is particularly strong in the policy debate on wealth taxation. Our paper contributes by being the first to examine this threat carefully, and showing that these spillovers, while real, are significantly smaller than anticipated.

Our results also add to a body of work investigating the impact of managers and CEOs on firm performance, using variation from retirements, family successions, and deaths. Those studies have documented large negative effects of CEOs deaths 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 particularly 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 relatively small in magnitude and mostly explained by firm restructuring and sale.[4] Many owners retain control of their firms when they move or restructure their activity rather than

[3] One exception is worth mentioning. Advani et al. (2023) studies a subset of rich taxpayers called *non-dom* in the UK. Following a reform in the *non-dom* tax regime, they find small elasticities of emigration flows to the net-of-tax rate.

[4] For instance, Smith et al. (2019) find a 26 percentage point decrease in firm survival following an owner retirement event, and a 82% drop of profits per worker (after accounting for buy-outs). Even after conditioning for firm survival, they find a 45% decrease in profits per worker after a retirement event. In contrast, we find almost no effects of owners' out-migration after we condition for firm survival.

shutting it down. This suggests that, when it comes to entrepreneurs, migration is much less disruptive than retirement or death for their business activity.[5]

The remainder of the paper is organized as follows. Section 2 describes the institutional background and section 3 describes our data. Section 4 documents the international migration patterns of the very wealthy. Section 5 investigates the impact of migration events on individual and firm level outcomes. Section 6 identifies the causal impact of wealth taxes on migration flows using tax reforms. 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: from 1911 to 2007, Sweden levied an annual progressive wealth tax on the value of assets, net of debt, held by the wealthy. While the wealth tax schedule varied significantly over a hundred years, wealth tax revenue typically represented between 0.5 and 1% of total tax revenue, that is between 0.2 and 0.4% of GDP (Henrekson and Du Rietz, 2014). 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 that period.

Tax Base: The Swedish wealth tax was a comprehensive tax on all forms of wealth. Net taxable wealth was defined at the household level and included all financial and non-financial assets, assessed at market value, minus debt. However, important exemptions did affect the tax base. First, a 25% exemption was applied to the value of real estate assets. Second, certain asset categories such as pension savings, art and jewellery were fully exempt from the wealth tax. Third, certain stocks benefited from exemptions which varied 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.[6] From 1991 on, shares of unlisted firms and of firms on the so-called O-list of the Stockholm stock-exchange (small caps and start-ups) were fully exempt. Finally, and most importantly, wealth from closely-held businesses was tax exempt. That is, individuals owning more than 25% of a firm were fully exempted from the wealth tax on the value of these stocks. This type of exemption for corporate equity was widespread among European countries with wealth taxes (e.g. France, Germany, Denmark, etc.).

[5] 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 very similar results to ours in terms of magnitude.

[6] 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.

Reporting & Enforcement: The long history of wealth taxation in Sweden, and the requirement to assess wealth at market value, meant that reporting and enforcement were quite sophisticated and advanced relative to similar European 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 about taxpayers' financial and real estate assets, as well as liabilities, to the tax authority. The government could use market prices from stock markets and real-estate prices to value those assets. Information was carefully reported in comprehensive administrative registries: the land registry and the financial asset registry (KURU). Taxpayers then received pre-populated wealth tax returns at the beginning of the year $t+1$ and had to self-report non-third-party reported components of their wealth holdings and liabilities. Self-reported components of taxable wealth included cars and other durables, and, most importantly, stocks in closely-held businesses held directly. For the latter, taxpayers were required to report in a dedicated form (the *K10* tax form) the number and prices of shares of their closely-held businesses, as well as any transaction or dividend payments related to those shares. To prevent households from evading the wealth tax by artificially registering their personal assets as (tax-exempt) business assets, firm owners had to prove to the tax authority that the assets they registered on the firm were essential for the firms' operation. The tax authority used several concrete 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 considered as business asset and thus was taxed as personal wealth.[7]

Despite the best effort of the tax administration, evidence suggests that tax evasion was still prevalent at the top end of the wealth distribution, in particular, through dissimulation of assets in tax havens (Alstadsæter et al., 2019). In Section 7, we explore the implications of tax evasion for the robustness of our conclusions using available estimates of the amount of evaded assets at the top of the wealth distribution.

Residence Rules: The Swedish wealth tax applied to Swedish residents, who were taxed on the value of their worldwide assets.[8] Residence for tax purposes is evaluated as living in Sweden, staying permanently in Sweden, or having a significant economic connection to Sweden. Note that, for tax authorities, owning a business in Sweden is a sufficient condition to be considered as a Swedish resident for tax purposes.[9] Furthermore, even if deemed non-resident for tax purposes, households still had to pay wealth taxes on the value of their assets held in Sweden, according to the

[7]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.

[8]Sweden had tax agreements with most countries to prevent double taxation of assets.

[9]See e.g. https://skatteverket.se/privat/internationellt/bosattutomlands.

"limited tax liability regime" (*begränsad skattskyldighet*).[10] This means that in theory, migrating out of Sweden was not enough to avoid the wealth tax: one would also have to reallocate assets out of Sweden, creating potential distortions to wealth allocation and investment decisions. In practice though, it seems that the enforcement of the wealth tax for non-residents was relatively weak, and special bilateral treaties offered additional grey area. Therefore little is known on the impact of the Swedish wealth tax on asset location. Our investigation below allows to shed unique light on this issue.

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 schedule: the tax rate was zero below an exemption threshold, while wealth above this threshold was taxed at a 1.5% marginal tax rate.[11] This exemption threshold varied over time: from SEK 800K in 1991, it was progressively increased in the early 2000s to reach 3,000K in 2006. In 2001, a separate (lower) threshold was also introduced for single individuals. Panel A of Appendix Figure I.1 shows the evolution of the wealth tax threshold over time, expressed as a percentile of the household taxable wealth distribution. The threshold was at its lowest in 1999, at which point all households above the 92-th percentile of the taxable wealth distribution were taxable. In 2006, the threshold had increased significantly so that only couples belonging to the top 2% of the taxable wealth distribution were liable to the tax. For singles, the lower threshold meant that in 2006 they remained taxable if their taxable wealth was above the 92-th percentile of the distribution. Panel B of Appendix Figure I.1 shows a similar exercize but where we rank household by their net wealth (instead of their taxable wealth). The graph displays, for various top fractiles of the net wealth distribution, average taxable wealth against the tax threshold for singles and couples in 2006. It confirms that single household were taxable if they belonged to the top 8% of the net wealth distribution, while couples were taxable if they belonged to the top 2% of that distribution. In what follows, we therefore consider all individuals belonging to households with total net wealth in the top 2% of the distribution as "treated" by the wealth tax over the period 1999-2006, and we consider individuals belonging to households with total net wealth in the top 8% to top 2% of the distribution as "partially treated" by the wealth tax over the period 1999-2006. All households with net wealth below the 92-th percentile were never affected by the wealth tax.

Abolition of the Wealth Tax in 2007: During the period 1999-2006, despite the increase in the level of the tax exemption threshold, the Swedish wealth tax continued to generate substantial tax

[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.

revenues: annual wealth tax payments accounted for $\approx 1.2\%$ of total annual tax revenues over that period. For households in the top 2% of the wealth distribution, the effective wealth tax rate remained stable, amounting to an annual average tax of $\approx .5\%$ on their total net wealth (Figure 10 Panel A). After the general election of October 2006, following the surprise ousting of the Social Democrats after twelve years in power, a coalition of centre-right parties took office, and decided to abolish the wealth tax with immediate effect on January 2007.[12] We use this large and surprise decline in the wealth tax to identify the elasticity of migration to wealth taxation in Section 6.

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 for closely held businesses to prevent that entrepreneurs avoid progressive taxation of wage income by shifting their labor income to dividends.[13] The "3:12 rules" put a cap, for owners of closely held businesses, on the amount of dividends and capital gains that can be taxed at the flat rate on capital income. Appendix Figure I.2 displays the evolution around 2007 of the effective tax rate on capital income, defined as the sum of all taxes paid on capital *income* (thus excluding wealth tax payments and other taxes on the stock of capital), divided by the sum of all realized capital income. It shows that, overall, effective tax rates on capital income were very stable around the abolition of the wealth tax, and evolved similarly for top wealth holders compared to the rest of the population.

Apart from the wealth tax, Sweden levied two other taxes that applied to the stock of wealth, rather than to income flows generated by wealth. First, Sweden has a property tax, levied on real estate. The property tax was administered centrally until 2008, when it was replaced by a municipal level "fee" organized by municipalities. Sweden also levied a tax on inheritances, which was progressively abolished between 2003 and 2005. But inheritance taxation had, before its abolition, little bite on top wealth holders, as it had already been greatly reduced in 1991. Furthermore, we note that, beyond having little bite, inheritance taxation was unlikely to affect location decisions of the very wealthy because extremely stringent residence rules made it hard to avoid the inheritance tax by migrating out of Sweden. Indeed, deceased individuals needed to have lived out of Sweden and stopped being a tax resident for at least ten years at the time of death for their assets not to be subject to inheritance taxation in Sweden.

[12] The main argument used by the coalition to justify the abolition of the tax related to its negative migration effects on the wealthy. For instance, Bengt Westerberg, the leader of the Swedish Liberal Party who spearheaded the abolition of the tax 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."*

[13] 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.

In Appendix Figure I.3, we sum all tax payments corresponding to taxes on capital and taxes on capital income. That is, we sum, for each household, their capital income taxes, property taxes, inheritance taxes and wealth taxes. We then scale this by the net wealth of the household. This gives a measure of the effective tax rate on household net wealth accounting for all taxes paid by Swedish households. The figure then reports how this effective rate evolved around 2007 for households in the top 2% of the wealth distribution, relative to households further down the distribution who were not liable to the wealth tax.

The figure shows that effective tax rates on household wealth remained stable across groups before 2007, and then decreased strongly for top wealth holders right in 2007 with the abolition of the wealth tax. The relative decrease in effective tax rates for households in the top 2% of the wealth distribution amounts to 0.4 percentage point, which corresponds precisely to the effect of the abolition of the wealth tax documented in panel A of Figure 10. This large, persistent downward shock on capital taxation for top wealth holders relative to the rest of the distribution validates the appeal of using the abolition of the Swedish wealth tax in a difference-in-difference design to identify the causal impact of wealth taxation on location decisions of wealthy households.

2.2 Wealth Taxation in Denmark

Sweden was not an exception in Europe, where many other countries, especially in Scandinavia, had a long history of progressive wealth taxation. To get out-of-sample validation of our results for Sweden, we therefore complement them with similar analyses in Denmark, a country which arguably offers a useful point of comparison. As small dynamic open economies well-integrated within the European Union, and with high levels of redistribution, Sweden and Denmark share many similarities, as did their wealth tax systems, which we compare in Appendix Table II.1.[14] Furthermore, Denmark did experience two important reforms of its wealth tax, in 1988, and then in 1996, when the tax got repealed. These two reforms can be leveraged to estimate migration elasticities from alternative identifying variation, offering a direct assessment of the external validity of our main results for Sweden.

3 Data

Our analysis relies on granular and exhaustive administrative registries on wealth, firm ownership structure and migration. This data infrastructure is unique because it covers, for the universe of Swedish households, all assets (and not just taxable wealth) without censoring or top-coding, and enables us in particular to link households to all the business assets they control, including in non-public firms. It is further unique because this information is matched to precise records of the

[14] We provide more details on the Danish tax in Appendix I.2.

universe of migration events in and out of Sweden.

3.1 Wealth and Income Registers

We use comprehensive administrative migration, income and wealth registers covering the population of all Swedish residents from Statistics Sweden. Data on wealth comes from the wealth tax register (*Förmögenhetsregistret*), which covers the asset portfolios for 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 each December, spanning the years 1993 to 2007. In terms of financial assets, we have access to data on all savings categorized by asset class, including bank accounts, bonds, stocks, mutual funds, private retirement accounts, and more. The dataset also contains information on the total outstanding debt including mortgage debt, consumer credit or student debt. Regarding 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.[15]

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, education), LISA contains exhaustive information on all labor earnings, taxes and transfers and capital income on an annual basis for the period 1990 to 2017.[16] Furthermore, we merge this data with the matched employer-employee registries (RAMS) for the period 1985 to 2017, which reports the universe of individual employment contracts in establishments of firms operating in Sweden. Finally, we complement this data with information on individuals' cognitive and non-cognitive ability as measured by army enlistment tests.

Note that 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 (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 for the period pre-2007, and then use to predict net wealth after 2007. The model 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 anymore, we have precise information on the past value of all 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. We describe the model in

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

detail in Appendix II.1. Importantly, our prediction model performs exceptionally well, as showed 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).

Firm Ownership Registries: 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 registry (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 returns and was established to monitor dual taxation avoidance and ensure that wages are not categorized as dividends for optimization purposes. The *K10* tax returns must be filed yearly and for each company when an individual (i) owns the firm, (ii) or works in the firm and (iii) at least 50% of the shares are owned by at most four individuals.[17] 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.

One limitation is that the *K10* tax returns only link individuals to the private firms they own directly. This is potentially problematic because the largest closely-held businesses are likely to be held through holdings 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.[18] 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. And eventually, this gives us a complete mapping between individuals and all the firms that they own both directly and indirectly in Sweden.

Each listed or 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 bill, investments or assets.

[17] Members of the same family are considered as one and the same individual for the purpose of reporting requirements in the K10 registry. As a consequence, one cannot avoid reporting ownership in the K10 by giving away shares to spouse, children or grand-children.

[18] We detail our matching algorithm in Appendix II.3.

Figure 1 shows the importance of measuring entrepreneurship among wealthy taxpayers. We focus on closely-held businesses (i.e. firms over which the individual has direct control defined as having more than 20% of voting rights) and LLC (limited private companies) operating in Sweden. We exclude of our analysis ownership of foreign companies. Panel A describes direct ownership of companies by level of net worth and type of firms in Sweden. The fraction of business owners increases dramatically at the top of the wealth distribution: less than 3% of individuals in the bottom half of the wealth distribution own a firm, as opposed to more than 10% in the top decile. This proportion reaches 19% for the top 2% and 38% 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, but their contribution to business activity is also quite granular. Panel B illustrates this fact by looking at the contribution of firms controlled by wealthy taxpayers to the overall level of employment in the Swedish economy.[19] 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 employ 9.2% of total Swedish employment. Concentrating on the very top of the distribution, we see that individuals in the top 0.1% control 3% of total Swedish employment through the businesses they privately own. Panel B also highlights an 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. This is due to a third of unlisted firms owned by the wealthy acting as parent companies for at least one subsidiary in the Swedish economy.

Migration Register: Migration registers enable us to measure precisely when migration events occur and for how long. Upon arrival in Sweden, any taxpayer is required to request a national identification number. Similarly, if taxpayers want to stop paying taxes in Sweden, they need to report their move to the local tax authorities. Furthermore, Swedish citizens leaving the country for 12 months or longer must annually report the number of days spent in Sweden for population registry purposes and to determine tax residency. This means that our administrative data source on migration is very precise. This also 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 analysis, an out-migration event is when an individual started the year out of Sweden and finished the year outside Sweden. Similarly, an in-migration event is when an individual started the year outside Sweden, and was in Sweden at the end of the year. Note that people can move multiple times during the same year, but we aggregate all moves at the annual

[19] We exclude owners when calculating the employment contribution of closely-held businesses.

level to define those migration events.

In Figure 2, 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 the rest of the population.

Wealth and Migration Data in Denmark: The administrative data on wealth and migration that we use, in Denmark, for our validation analysis, shares many similarities with the Swedish data described above. We provide all details in Appendix II.2, and summarize in Panel B of Appendix Table II.1 the main differences in terms of data availability and coverage between Sweden and Denmark.[20]

4 Migration Patterns Among the Wealthy

4.1 Migration Flows

We start by describing gross migration flows by wealth level in Sweden and in Denmark in Figure 3. We rank individuals by level of net wealth at market value in year t, and then compute the fraction of individuals who out-migrate in $t+1$ for each wealth fractile in t. Similarly for in-migration rates, we rank individuals by level of wealth in year t and we look at fraction who in-migrated in $t-1$ for each wealth fractile in t. We compute those statistics focusing on the period where the wealth tax is still in place, which corresponds to 1999-2006 for Sweden (Panel A) and to 1989-1996 for Denmark (Panel B).

Out-migration flows are much smaller in the top decile than in the rest of the population. For instance, in Sweden, 0.2% of individuals in the top decile of the wealth distribution leave the country each year, against 0.6% in the bottom 50%.[21] The fraction of individuals leaving Sweden is however increasing at the very top of the wealth distribution, in particular above the wealth tax exemption threshold. For instance, out-migration rates in the top 0.1% are twice larger than for the wealthy not subject to the wealth tax. Nevertheless, those outflows remain small in magnitude,

[20] 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 in particular to identify the elasticity of in-migration with respect to the net-of-tax rate on wealth in Section 6. One of the downside of the Danish data, is that we do not have access to information on closely-held businesses as we do in Sweden, to track the effect of migration on the economic outcomes of firms owned by the very wealthy.

[21] In Figure III.1, we show the distribution of out-migration rates for all deciles.

with less than 0.4% of these very wealthy individuals out-migrating each year. This finding holds even after weighting those outflows by the level of wealth of out-migrants at the top of the wealth distribution. The taxable wealth of out-migrants subject to the wealth tax represents 0.09% of total taxable wealth in Sweden.

Though public attention often focuses on the wealthy leaving, our data also allows us to look into their arrival, which is equally important. Panel A2 shows that the in-migration patterns of the very wealthy are similar to their out-migration behavior, with small inflows on average, but larger in-migration rates at the very top. If we put these in- and out-migration rates together to compute net migration rates, we observe a small positive net migration rate of about 0.05% in Sweden at the top of the wealth distribution. Far from seeing an exodus of wealthy taxpayers, Sweden was therefore experiencing small net migration gains at the top of the wealth distribution in the period preceding the abolition of the wealth tax.[22]

Our unique setting and data environment enables us to benchmark our analysis in two countries. This is useful to show that the patterns we document are not driven by a specific context but rather capture generalizable trends about international migration of the very wealthy. We find that the size and patterns of out-migration flows at the top of the wealth distribution are extremely similar in Denmark. The overall out-migration rate in the top decile is around 0.1% and we observe the same increasing pattern in wealthy migration rates above the exemption threshold in Denmark too, with larger international mobility rates at the very top: about 0.4% of individuals in the top 0.1% of the wealth distribution migrate out of Denmark every year. The net migration rate is very close to zero, except for the top 0.1%, where it is slightly negative, with a net-migration of around -0.1%.

We also describe the geographic patterns of migration at the top of the wealth distribution. In Appendix Figure III.4, we decompose 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. The second top destination is the US. Another noteworthy destination is Switzerland, while in comparison, the share of migrants to Switzerland is considerably smaller among the wealthiest not treated by the wealth tax.

4.2 Who Migrates In and Out?

We use the granularity of our joint dataset on wealth and migration to study selection into migration out of and in 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

[22] See Figure III.2 for year by year out-migration and in-migration figures.

(respectively in-migration) in Sweden. We run a first regression model:

$$\mathbb{P}\{Y = 1\} = \boldsymbol{\beta}'\mathbf{X}_0 \tag{1}$$

where $\mathbf{X}_0$ 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.[23] 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.[24]

The blue coefficients in Figure 4 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, or move-in 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 and in 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:

$$\mathbb{P}\{Y = 1\} = \mathbb{1}\{W < P_W\} \cdot \boldsymbol{\beta}'\mathbf{X}_0 + \mathbb{1}\{W \geq P_W\} \cdot \boldsymbol{\beta}'_{\boldsymbol{w}}\mathbf{X}_0 \tag{2}$$

Where we use $\mathbf{X}_0$ to denote vectors and we set P_W =P98, i.e. the 98th percentile of the wealth distribution. The red coefficients in Figure 4 plot the estimated vector of coefficients β_w representing the effects of the various components of $\mathbb{X}_0$ 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 age gradient is inverted, and we observe much less drain

[23] 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.

[24] 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.

on younger people than on older people. The same pattern appears for the cognitive ability and education gradients. Despite the public discourse, there is in fact much less brain drain at the top of the wealth distribution than in the rest of the population. However, 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 showed 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.

Overall, these effects seem to confirm that wealthy entrepreneurs were more likely to leave Sweden during the period where the wealth tax was in place, and the effects of wealthy migration on firms' outcomes thus deserves special attention.

5 What Happens When the Wealthy Migrate?

We now study the implications of out-migration and in-migration events at the top of the wealth distribution on individual and firm-level outcomes.

5.1 Individual-Level Event Studies

Selection of Events: We define as our event of interest the first time an individual in the top 2% of the net worth distribution out-migrates from Sweden.[25] We focus here on migration events happening over the period 2000-2007. But we investigate in Section 7 how these event-study estimates compare before and after the abolition of the wealth tax.

We build a control group by randomly assigning placebo migration dates to the wealthy that never out-migrate from Sweden. Our approach does not match on pre-event characteristics, allowing us to detect potential self-selection into out-migration based on pre-migration dynamics in individual or firm outcomes. In our final dataset, we have 3,517 out-migration events of wealthy taxpayers leaving Sweden, and 255,888 corresponding placebo events. Using this sample, we proceed to investigate the dynamic effects of out-migration on individuals' outcomes by estimating the

[25]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.

following event-study specification:

$$y_{it} = \alpha \; + \; \sum_j \beta_j \cdot M_i \cdot D_{i,t-j} \; + \; \gamma \cdot M_i \; + \; \sum_j \delta_j \cdot D_{i,t-j} \; + \; \varepsilon_{it} \tag{3}$$

Where y_{it} is an individual-level outcome (for instance tax payments in Sweden) measured in year t, M_i is a dummy equal to one if the individual is part of the treatment group who ever out-migrated from Sweden, and $D_{i,t-j}$ are time-to-event dummies. We estimate Equation 3 using OLS, clustering standard errors at the individual-level, and omitting event time $j = -2$. The estimated coefficients β_j capture how the outcomes of out-migrants evolve compared to non-migrants, everything being expressed relative to the level of the outcome two years prior to the migration event. We plot the series of estimated β_j for several individual-level outcomes. To properly account for zeroes, we regress all outcomes in levels (instead of logs).[26] But to get a better sense of the magnitude of the estimated effects, we also report in each panel the estimates of β_1 and β_5 rescaled by the average value of the outcome variable for the treated group two years before the event.

Effects on Individual Tax Payments in Sweden: We first look at the effects of out-migration events on tax payments in Figure 5. Interestingly, we detect no signs of differential trends between migrants and non-migrants prior to migration. This suggests that migration events are not driven by previous increases in individual tax burdens. On impact (i.e. one year after migration), we find that out-migration has a large negative effect on total tax payments, which drop by 66%. Breaking down this total effect by type of tax payments, we estimate that, on average, out-migration events are associated with a 90% decrease in taxable wealth (Panel C) and a 57% decrease in wealth tax payments (Panel D), but also with a 68% decrease in income tax payments (Panel B). Because income tax payments represent almost 90% of individual tax payments made by the wealthy, this implies that migration events of wealthy taxpayers have significant fiscal externalities on income tax bases.

While the immediate effect of migration is large, with a drop of around 60-65% in tax payments, the medium-run effect (i.e. 5 years after migration) is significantly smaller, around 35-40%. This reflects the fact that a large fraction of the wealthy eventually moves back to Sweden: as we documented in Figure 2, around 40% of wealthy out-migrants are back after 5 years. In Appendix Figure IV.1, we estimate a median regression model. The results exhibit a clear "wheelbarrow" shape with no convergence, which confirms that the longer run dynamics is essentially driven by the extensive margin of individuals coming back to Sweden.

Finally, Figure 5 reveals a distinctive pattern for capital income taxes in Panel F. Out-migration is associated with a temporary jump in capital income tax payments that occurs exactly in the

[26] Outcomes are also winsorized for the bottom 1% and top 5%.

year of the event. After this initial jump, capital income tax payments start declining in a similar fashion as other tax payments. This suggests that Swedish residents liquidate some of their assets and pay related taxes on capital gains upon leaving.[27] To better understand changes in assets composition following changes in residence, we next investigate portfolio reallocation upon out-migration events.

Portfolio Reallocation: We use our detailed information on asset composition and asset transactions to investigate how the wealthy reallocate their wealth before and after they leave Sweden. Results are presented in Figure 6. We start by looking at real estate wealth. We show in Panel A that wealthy taxpayers sell their real estate property in Sweden when leaving the country, with most of the effect concentrated in the year of the out-migration event. This is consistent with the duration analysis performed in Figure 2 and confirms that out-migration events at the top of the wealth distribution reflect real responses rather than artificial changes in tax residency. We then look at financial wealth (excluding business assets). We first find a significant decline in the probability to report any positive financial wealth in Sweden (Panel C). We note that this large decline of about 20 p.p. is still far from 100%. In other words, a large fraction of wealthy out-migrants continue to hold financial investments in Sweden after departing. Panel D also shows evidence of a response at the intensive margin. Conditional on reporting positive financial wealth in Sweden after they leave, wealthy out-migrants decrease their financial wealth held in Sweden by 16%. We can then use the detailed information on financial asset transactions to document that this decline in the value of financial portfolio held in Sweden corresponds to active sales of assets held in Sweden. Panel B shows for instance a significant jump in event year 0 in the probability to sell more than 10% of the value of one's portfolio.

Figure 2 reveals that out-migration is associated with lower housing and financial wealth in Sweden, confirming that the wealthy actively reallocate part of their assets when leaving the country. One specificity of top wealth individuals is that they also own companies. The threat that migration of wealthy entrepreneurs may have large economic spillovers due to reallocation of business assets looms large in the public debate about wealth taxation. We therefore now turn to documenting how the business assets that wealthy individuals control are affected when these individuals migrate.

5.2 Firm-Level Event-Studies

To study the impact of migration of wealthy individuals on Swedish businesses, we use the rich information on closely-held businesses and ownership structures of Swedish firms presented in section 3. This information allows us to link wealthy individuals to the firms that they control either

[27] We show in Figure IV.2 that the temporary jump in capital income tax payments upon out-migration is the same for entrepreneurs and non-entrepreneurs.

directly or indirectly. Table 1 starts by presenting descriptive statistics on closely-held businesses, broken-down by wealth level of owners. It reveals that the business assets held by the wealthiest individuals are indeed quite granular, and account for a significant fraction of Swedish economic activity. The average company directly controlled by wealthy taxpayers has an average size of 14 employees (excluding the owner), which is almost twice larger than the average closely-held business in Sweden. Despite being bigger on average, unlisted companies held by wealthy owners are not characterized by higher value added per employees than the average unlisted firm in the economy. However, closely-held businesses owned by the wealthy do have higher gross investments on average, in absolute and per employees, than average firms. Consistent with their larger size, those firms also have more owners than others: 56% of active closely held businesses owned by an individual in the top 2% have more than one owner, against 46% for the full population of active CHBs.[28]

The penultimate column of Table 1 quantifies the contribution of wealthy taxpayers' firms to the overall Swedish economy. Panel B focuses on closely-held businesses directly owned by top wealth owners. It shows that they account for 3.5% of total employment in Sweden, 6.9% of aggregate value-added, and 10.7% of firms' tax payments.[29] Panel C shows that these figures increase significantly once we account for all the businesses that are also held indirectly by wealthy taxpayers through complex ownership structures. The businesses controlled by the top 2% of richest households represent 9% of total employment in Sweden, 15% of total value-added, 19% of corporate tax payments and 12% of aggregate business investment.

Selection of Events: Our main empirical strategy is to track firm outcomes before and after one of their owner leaves Sweden during the period 2000-2007. We start by focusing on firms directly controlled by the wealthy. We define as our event of interest the first time a direct owner in the top 2% of the wealth distribution out-migrates from Sweden.[30] When firms have multiple owners, we set the event date as the first emigration date among all the emigrant owners of that firm over time. We also restrict our analysis to closely-held businesses that are active (with at least one employee that is not the owner) in the year before the emigration event.

We build a control group of firms by assigning a random placebo migration date to the wealthy owners who have never emigrated. And we keep in the control group firms held by at least one

[28] Figure IV.8, Panel A, starts by describing unlisted companies' sectoral activity by level of wealth of their owner. Economic activities such as hospitality or construction are less represented in firms held by wealthy owners as opposed to companies held by low-wealth individuals. Overall, most closely-held businesses with wealthy owners operate in the wholesale and retail trade sector as well as in real estate, renting and business activities.

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

[30] 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 worth distribution in the years that precede the out-migration event.

owner who was in the top 2% of the wealth distribution for at least one year before the placebo event date, and who has never emigrated. To be consistent with our construction of the treated group, we finally restrict the firm to be active in the year before the placebo event time.

After having selected our control and placebo groups, we balance our firm-year level dataset by attributing zero outcomes (e.g., employment, turnover, investment etc.) when the firm is not active.[31] In our final dataset, we have 298 events of top 2% rich owners leaving. The treated and control firms included in our event-study sample are described in Appendix Table IV.3. Treated firms are bigger in terms of size, employment and value-added, meaning that out-migrants own firms that are larger than non out-migrants with the same level of net worth.

To investigate the dynamics of firm outcomes before and after one of their owner leaves Sweden, we estimate the following firm-level event-study specification:

$$y_{ft} = \alpha \; + \; \sum_j \beta_j \cdot M_f \cdot D_{f,t-j} \; + \; \gamma \cdot M_f \; + \; \sum_j \delta_j \cdot D_{f,t-j} \; + \; \varepsilon_{ft} \tag{4}$$

Where y_{ft} is a firm-level outcome (for instance employment) measured in year t, M_f is a treatment dummy equal to one if one of the firms' wealthy owner has out-migrated from Sweden, and $D_{f,t-j}$ are time to event dummies. We estimate Equation 4 using OLS, clustering standard errors at the owner-level. We omit β_{-1} and the estimated coefficients β_j capture the evolution of firm outcomes in year j to the out-migration event, relative to the level of outcome in the year preceding that event, and relative to the control group.

We present the main results in Figure 7. In Panel A, we show the survival probability of firms before and after their owner out-migrates. We find a large decrease in the probability that the firm remains alive after the event, with an average decrease of 28 percentage points.[32] Consistent with the reallocation we observed for other assets in Figure 6, this suggests that out-migration is associated with significant reorganization of business assets. It is important to note however that we define a firm as being alive when its firm identifier is present in the administrative firm registry. The disappearance of a firm's identifier from the registry may be the consequence of business closure, but could also be the result of a buy-out by another firm, or of business restructuring implying a change in firm identifier. The economic implications of a closure are potentially quite different from that of a buy-out or of business restructuring. We return to this issue below, and show that a the majority of firm disappearances are not actually business closures.

[31] To deal with outliers, we also winsorize the top 5% of the distribution of our outcomes, and the bottom 1% of the distribution of outcomes that are not bounded by zero (i.e., that can take negative values). We show the robustness of our results to different strategies for handling outliers in Appendix Table IV.4.

[32] Appendix Figure IV.4 shows the corresponding results when using instead the probability of observing the firm closing in a given year.

In the remainder of Figure 6, we continue to focus on the outcomes of firms directly controlled by wealthy outmigrants, not accounting for the outcomes of the firm if it is acquired and changes identifier. We find large and persistent negative effects on employment, value added, net turnover, tax payments and investments. For instance, the number of employees of firms held by wealthy households decreases by more than 30% after its owner leaves Sweden. The effects on other components of firms' balance sheets have similar magnitudes.[33] Interestingly, these negative impacts appear almost entirely driven by the extensive margin of firm disappearance documented in Panel A. Appendix Figure IV.3 replicates the same event-studies conditional on a firm's existence. Results indicate a small negative intensive margin effect on employment, and small positive but insignificant effects on value-added and investment.

Those average effects could potentially mask significant heterogeneity. For instance, older entrepreneurs may be more likely than younger owners to shut down their firms when they leave Sweden. Similarly, having children or not could change owners' incentives to close their firms in Sweden after leaving the country. We investigate in Appendix Figure IV.5 whether spillovers of out-migration vary based on owner characteristics and the type of firms involved. In Panel A, we show that the probability to close the firm upon out-migration of the owner does not vary by the owner's age, nor by whether the owner has adult children or not. We also find similar effects of owners' out-migration on firm survival for both smaller and larger firms. Spillovers in terms of employment (Panel B), value-added (Panel C), investment (Panel D) and tax payments (Panel E) do not vary by age of the owner, by the number of children of the owner, nor by size of the firm.

In-Migration Events: Wealthy taxpayers reorganize their business assets when they leave Sweden. But what happens when they come back (or come in)? To get a full picture of the economic implications of international migration of the wealthy, we also need to study the *in-migration* of wealthy entrepreneurs in Sweden. We conduct a similar firm-level analysis, now focusing on firms owned by wealthy entrepreneurs who arrived in Sweden between 1998 and 2006. We apply the same sample restrictions and balancing procedures as in our baseline analysis. We estimate Equation 4 by replacing M_f with a dummy variable set to one if the firms' wealthy owner has migrated to Sweden. The entire path of β_j coefficients is presented in Appendix Figure IV.7. Our analysis reveals that the probability of owning an active business in Sweden rises sharply after in-migration. The effects on firm-level employment, value-added, and net turnover mirror those observed after out-migration, but with opposite signs. In other words, wealthy taxpayers close businesses when they leave, but open (or re-open them) when they come back. In Figure 8, Panel A, we plot $-\hat{\beta}_5$

[33] Table IV.4 compares our baseline reduced-form estimates to estimates using different levels of winsorization. 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, tax payments and investments. These positive effects are driven by one outlier firm that continued to grow strongly after its owner migrated out of Sweden.

for in-migration events, and compare these estimates to the effects of outmigration events.[34] To benchmark the relative magnitude of those estimates, we rescaled the coefficients $-\hat{\beta}_5$ for in-migration events by the average value of the outcome in the outmigration sample the year prior to outmigration. We find that the probability to have an active business, and the number of employees controlled by the wealthy react similarly to out-migration and in-migration events. The impact of in-migration on turnover and investment appears smaller however, compared to outmigration events.

Firms Held Indirectly by the Wealthy: The analysis conducted in Figure 7 so far ignores potential downstream effects through the indirect participation of wealthy expatriates in other Swedish private firms. This is potentially problematic because indirect ownership is frequent among wealthy entrepreneurs, and because the firms they own through holdings are large. Table 1, Panel C, shows that when considering subsidiaries held by wealthy taxpayers, the average number of employees at firms owned by individuals in the top 2% increases from 14 to 22. Value-added and net turnover are also approximately 60% larger than when exclusively analyzing firms directly held by the wealthy.

For a more comprehensive assessment of the implications of wealthy entrepreneurs' out-migration, we estimate the effects of out-migration events on all firms owned by the wealthy, including those held through other private companies. We estimate the same specification but we consider outcomes of all firms (e.g parent and subsidiaries) owned by the wealthy. We report our estimates with pink dots in Panel B of Figure 8. We find very similar effects once we account for economic activity at firms held indirectly. Five year after the out-migration of their wealthy (direct or indirect) owner, employment at those firms decreases by 19%, value added decreases by 33% and tax payments decrease by 45%. Therefore, the out-migration of wealthy individuals has significant effects on their firms, even those that they control indirectly.

Firm Acquisitions: Some companies that disappear upon their owner out-migration could, in fact, be bought and merged with existing firms. If this is the case, real economic activity in Sweden remains the same, but the company appears with a new tax identifier in our dataset. To examine how much of our results could be explained by this phenomenon, we develop an algorithm described in Appendix V that traces employees' transitions between Swedish firms subsequent to a closure event. We detect events where at least 50% of employees were employed together at a given firm and end up working together at a new firm after the closure. We interpret those events as "buy-outs" where the closing firm has been absorbed by another Swedish firm.[35]

[34] We provide formal tests of equality for the magnitude of the effects of out-migration and in-migration events ($\beta_5^{in} = -\beta_5^{out}$) in Appendix Table IV.1

[35] Other studies have previously used similar algorithms to detect outsourcing or mass layoffs events. Smith et al. (2019) use a similar method to detect firm reorganization upon owners' death or retirement.

We find that, among closely-held businesses controlled by wealthy out-migrants, more than 60% have half or more of their employees move to the same firm after a closure event.[36] Thus, more than half of our estimated firm-level effects can be attributed to firms being sold or merged following the owner's departure.[37] Therefore, our baseline analysis over-estimates the negative spillovers effects of out-migration events on economic activity in Sweden. To account for this reallocation of firms across owners within Sweden, we implement a version of our analysis that takes into account buy-outs. Specifically, we can identify the identifiers of firms that absorb companies held by wealthy migrants after the closure event. We can thus consider economic activity at the newly formed entity as our main outcome of interest (i.e., the sum of old firm and new firm outcomes). We plot the estimates using this alternative strategy in the Panel B of Figure 8. Once we account for buy-outs, we find that out-migration of wealthy migrants is associated with almost no change in employment of Swedish residents, and smaller changes in other economic outcomes: value-added decreases by 16%, net turnover by 8.5%, tax payments by 36% and gross investments by 9%.

Worker-Level Analysis: Even in the presence of real firm closure upon the out-migration of wealthy owners, firm-level estimates presented in Panel B of Figure 8 miss the reallocation of workers across Swedish firms. Workers could transition to a new job immediately after their firm disappears, resulting in minimal changes to aggregate employment in Sweden, even when firms close due to out-migration patterns. On the other hand, workers could also be paid different wages after those transitions, or after their firm is reoganized or sold to another company.

Our data enable us to study this mechanism directly. To do this, we take a worker-level approach and replace firm-level outcomes y_{ft} by gross earnings and unemployment probability measured for each worker employed at our treated and control firms in the year before an out-migration event (real or placebo) occurs. We then trace the dynamics of individual labor market outcomes, before and after the event. As showed in Figure 9, we find that out-migration of wealthy entrepreneurs is associated with a decrease in their employees' gross earnings (Panel A) and a slight increase in their unemployment probability (Panel B). However, these effects appear small in magnitude. For instance, the probability to be unemployed increases by just about .5 percentage point after the owner of their firm left Sweden.

Summing Up: Overall, our analysis provides the first systematic evidence that migration events for the wealthy are associated with substantial changes in business activity of the firms they own.

[36] In the U.S., Smith et al. (2019) use a similar methodology to detect reorganizations. They find that only 22% of closing treated firms (i.e, experiencing an owners' death event) and 28% of closing control firms had 50% of their employees move to the same employer.

[37] However, this does not appear to be specific to closure events triggered by out-migration, since we observe a similar pattern in our control firms that do not experience any out-migration events, but experience closure events for other reasons. We find that 65% of our control firms experiencing a closure event qualify as being restructured or sold.

We show that outmigration correlates with reduced economic activity of the firms controlled by the wealthy. Interestingly, we also show that the effects are symmetric for in-migration. Our data offers the opportunity to delve 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 owner end up being absorbed by other companies in Sweden, and employees at these firms do not experience a persistent loss in labor earnings or employment prospects. The public discourse often focuses on what would happen to a particular firm controlled by wealthy taxpayers if they were to leave. Our analysis indicates that the impact of wealthy out-migration on aggregate economic activity is strongly mitigated by reallocation forces in the Swedish labor market.

It is useful to compare these estimates with the findings of the nascent literature investigating the impact of managers and CEOs on firm performance, using variation from retirements, family successions, and deaths. Those studies have documented large negative effects of CEOs deaths and retirement on their firm performance (Smith et al., 2019).[38] We study a different type of owner-specific event, and show that out-migration of wealthy individuals affects the economic outcomes of the firms they control. However, our effects are smaller in magnitude and are mostly explained by firm restructuring and sale. For instance, Smith et al. (2019) find a 26 percentage point decrease in firm survival following an owner retirement event, and a 82% drop of profits per worker (after accounting for buy-outs). Even after conditioning for firm survival, they find a 45% decrease in profits per worker after a retirement event. In contrast, we find almost no effects of owners' out-migration after we condition for firm survival. This suggests that migration are much less disruptive, in terms of economic activity, than owners' retirement or death. Owners often retain control of their firms upon migration (and simply manage their firms from abroad). And they seem to plan the reorganization of their business assets.

While this evidence is intentionally descriptive, as we want to characterize what effectively happens when wealthy individuals migrate, it is interesting to ask ourselves how the specific context of migration events of wealthy entrepreneurs may explain the patterns we observe in the data. In particular, one may speculate that migration is often happening at specific moments in the life of a business owner, such as around retirement, or when an owner decides to sell her business. This in turn would imply that our results would tend to capture not simply the effect of migration, but also the impact of other events happening concomitantly. To further investigate this possibility, we look at the age distribution of firm owners at the time of migration in Appendix Figure IV.6. We find that owners migrate at all ages. And even though we can detect a small excess mass around age 60 to 65 relative to the age distribution of all firm owners, it does not seem that migration events are

[38] Jäger and Heining (2022) also show that manager deaths cause a drop in co-workers' wages.

predominantly triggered by retirement. Furthermore, we showed in Appendix Figure IV.5 that the effects of outmigration are not statistically different for young and old owners, which assuages the concern that our estimates pick up the specific impact of retirement rather than migration itself.

We finally note that these results alone are not sufficient to infer the migration-induced effects of wealth taxation on firms and economic activity. For this, we need first to measure the causal effect of wealth taxes on migration patterns. Second, we need to measure whether the effects of migration on firm outcomes differ when migration is tax-induced. This is what we do in the next two sections, using sharp and exogeneous variations in wealth tax rates to isolate the causal effects of wealth taxes on the out-migration patterns of the rich.

6 International Migration Responses to Wealth Taxation

6.1 Identification and Graphical Evidence

Our main source of variation is the repeal of the wealth tax in Sweden. As described in Section 2, the reform led to a sharp, unanticipated and persistent decline 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) source of variation available to identify the impact 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 decline in the taxes paid on their assets. Panel A of Figure 10 displays the evolution of effective average tax rates on total net wealth for the richest 2% of Swedish households. For them, the graph shows that the abolition of the wealth tax implied a sudden drop of their average effective tax on wealth of about .5%.

Difference-In-Difference Strategy: We exploit this large variation in wealth tax liability in a difference-in-difference design, comparing individuals at the top of the wealth distribution who are treated by the reform to individuals further down the wealth distribution who are unaffected by the wealth tax. For the treated group, we focus on individuals belonging to the top 2% of households with the highest net wealth. As we discussed in section 2.1, this group was always liable to the wealth tax prior to its repeal. For the control group, we use as a baseline strategy individuals belonging to the top 20% to top 10% of richest households. Two arguments motivate our choice. First, as we showed in Appendix Figure I.1, individuals belonging to the top 10% to top 2% have been partially affected by the wealth tax over the period 1999 to 2007 due to variation in the exemption threshold and the creation of a specific threshold for single individuals. Second, while it would seem better to choose a control group very close to our treatment group in terms of net wealth, we also need to account for potential contamination bias. The reason is that individual

wealth grows over their lifecycle so that households close to the exemption threshold may become liable to the wealth tax in the future. As a consequence, they could react to wealth tax variation in anticipation of becoming liable in the future. Our control group offers no such contamination problem: among individuals classified in our control group in 1999, only 0.99% end up in the top 2% by 2006.

By choosing a control group slightly further down the wealth distribution, one may worry about the validity of the assumption of common time shocks across treatment and control. The main validity check to assuage this concern is the absence of differential pre-trends in the outcome of interest prior to the reform. Before we inspect the data for common pre-trends, let us also briefly comment on potential identification threats posed by other changes in capital or labor taxation around the time of the 2007 reform that might have affected control and treatment groups differently. As we discussed in section 2.1, there were a few reforms of labor and capital taxation around the time of the abolition of the wealth tax. To check whether these reforms affected our two groups differentially, we plotted in Appendix Figure I.2 the effective labor taxes and capital income taxes (excluding the wealth tax) paid by our treatment and control groups, both before and after the repeal of the wealth tax. Panel A shows that effective capital income tax rates (excluding wealth taxes) did not drop for either our treated nor control group in 2007 and evolved similarly in both groups. Panel B further confirms that effective tax rates on labor income evolved similarly in our treated and control groups during the period. Appendix Figure I.3 shows that it is only when wealth tax payments are taken into account that effective tax rates on capital evolve differentially in the treatment group relative to our control group. Overall, we find no evidence that our treatment and control groups experienced any other significant and differential tax shock around the time of the abolition of the wealth tax.[39]

Panel B of Figure 10 displays graphically our differences-in-difference setting, and provides compelling evidence of the effect of the Swedish reform on migration flows. The figure plots outmigration rates for taxpayers in our treatment group (in red), and in our control group (in blue). The red vertical line denotes the repeal of the wealth tax in Sweden. Three important insights emerge from the graph. First, we see, consistent with the evidence from Figure 3, that, before the reform, outmigration rates were significantly larger for individuals in the treatment group, who were subject to the wealth tax. Second, the figure shows that out-migration rates were evolving very similarly in the treated and control groups up to 2006, before the reform. Both outmigration series appear almost perfectly parallel: they experience a decline in the first few years, and a slight increase after 2003. This evidence is very reassuring regarding the validity of the parallel trend

[39]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.

assumption underlying our identification strategy. Third, after the repeal of the wealth tax, there is a sudden and large drop in the out-migration rates of individuals subject to the wealth tax, compared to out-migration rates of wealthy individuals not subject to the tax. The outmigration flows in the treatment group immediately and fully converge to those observed in the control group in 2007. The figure provides compelling evidence that the wealth tax repeal significantly reduced out-migration rates of wealthy taxpayers exposed to the reform relative to wealthy taxpayers with no exposure.

Predicting Wealth After 2007 To Measure Long Term Exposure to the Reform: Out-migration series in Figure 10 stop in 2008 because of the break in the way the administrative data on total net wealth was collected after the abolition of the wealth tax. The question we ask now is: how persistent was the decline in out-migration rates of the treatment group after the abolition of the wealth tax? The answer to this question is critical to assess the aggregate implications of wealth taxation. As explained in section 3, we built a consistent measure of predicted wealth before and after 2007 that takes advantage of 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 anymore, we have precise information on the past value of all 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. Our prediction model, described in detail in Appendix II.1, has an important advantage. Because we use predicted wealth based on characteristics that are pre-determined at the time of the reform, rather than actual wealth, we avoid defining control and treatment groups based on a wealth variable that is endogenous to the wealth tax level.

Appendix Figure VI.1 illustrates this empirical strategy in a transparent way. 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 into 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 10, 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.

We then move to a strategy that generalizes this approach and relies on the more elaborate prediction model of wealth described in Appendix II.1. Individuals are allocated to the treatment group if their household predicted wealth belongs to the top 2%, and to the control group if it falls between the top 20% to top 10% of predicted wealth. For each individual, we regress the yearly probability to out-migrate Y_{it} on the interaction between a year fixed effect and a dummy T_i equals to one if the individual is in the treatment group[40]:

$$Y_{it} = \alpha + \sum_j \beta_j \cdot \mathbb{1}(t = j) \cdot \mathbb{1}(T_i = 1) + \gamma_t + \delta \cdot \mathbb{1} \cdot (T_i = 1) + u_{it} \tag{5}$$

We plot the estimated coefficients β_j (and their confidence interval) in Figure 11. The reform is associated with a large and permanent decrease of the probability to out-migrate for treated taxpayers. We find no evidence of significant pre-trends in out-migration before the reform, which confirms that the migration patterns of wealthy individuals just below the exemption threshold form a credible counterfactual for the mobility patterns of the wealthy individuals subject to the wealth tax.[41] Our estimates indicate that one year after the reform, the probability to out-migrate decreased by 0.05 percentage points for the wealthy. This represents a 30% reduction in the propensity to leave Sweden for the treated group compared to the year before the event, and compared to the control group. In other words, two important conclusions emerge from these results: the effects of the abolition of the wealth tax on out-migration flows are remarkably small in magnitude, but the wealth tax accounted for a substantial part of wealthy out-migration flows before the reform. Specifically, more than one-third of expatriation events among the top 2% of wealthiest households were caused by the wealth tax before 2007.

6.2 Identifying Elasticities of Migration Flows to the Wealth Tax

We showed compelling graphical evidence of international migration responses to the wealth tax in Sweden. We now proceed to estimate migration elasticities with respect to the wealth tax, the policy-relevant parameter in our context. For this, we relate the differences in out-migration patterns of the treatment and control group to the change in effective wealth tax rates induced by the reform. We obtain estimates of the elasticities from a 2SLS regression of the form:

$$Y_{it} = \alpha_0 + \varepsilon \ln(1 - \tau_{it}) + \beta_1 \cdot \mathbb{1}(i = T) + \beta_2 \cdot \mathbb{1}(t \geq t_0) + u_{it} \tag{6}$$

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

[41] A worry discussed in Section 2 was that changes in inheritance taxation in 2004 could affect the very wealthy more than the just wealthy. However, we find no evidence of significant differential out-migration patterns for those years, as showed in the pre-trends of Figure 11. This is consistent with the fact that the law limits the ability of individuals to avoid inheritance taxation through international migration, due to strict rules on tax residency definition at death.

where Y_{it} is the out-migration rate of group $i = \{T, C\}$ in year t, and t_0 is the year of the reform. The log net-of-tax rate is instrumented with a reform dummy interaction $\mathbb{1}(i = T) \times \mathbb{1}(t \geq t_0)$. Because τ_{it} is very small in our context, ε has a simple interpretation: if τ increases by 1 percentage point, out-migration rate at the top of the wealth distribution increases by ε percentage point.

We estimate the regression model using OLS and we collapse the data at the year-wealth group level, which is the level at which the exogenous wealth tax variation occurs. We also estimate Equation 6 separately for different groups of taxpayers to estimate heterogeneous migration responses to wealth tax reforms.

The estimates are showed in Panel A of Figure 12, using out-migration rate as the outcome variable. The semi-elasticity ε is around -.17 for the entire population of wealthy taxpayers in Sweden. This implies that a 1 percentage point increase in the wealth tax in Sweden leads to a 0.17 percentage point increase in the out-migration rate of wealthy taxpayers. International migration elasticities do not vary much by age or level of education. We also find that wealthy entrepreneurs and non-entrepreneurs are similarly responsive to a given change in wealth taxes.

In Appendix Figure VI.2, we explore the sensitivity of these estimates to our choice of control group. As we explained above, we made a somewhat conservative choice of using, as our baseline control group, individuals in the top 20 to top 10% of the wealth distribution, to avoid any contamination bias. One concern with this conservative strategy is that the common trend assumption is less likely to hold across groups that are not immediately contiguous in the wealth distribution. We already showed that there was no evidence of any differential pre-trends prior to the reform, which is comforting. Reassuringly, we find here that our estimated semi-elasticities are remarkably similar if we choose control groups that are further up the wealth distribution.

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

As with any difference-in-difference strategy, and despite the evidence pointing to the absence of unobserved shocks simultaneous to the Swedish reform, one may still raise valid doubts about the internal validity of our estimates. Furthermore, one may also wonder about their external validity, given migration elasticities are fundamentally non structural parameters. How transportable are these estimates to other contexts? 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, we take advantage of the existence of two large wealth tax reforms in Denmark which offer a unique opportunity to cross-validate out-of-sample our migration elasticity estimates. In 1988, the marginal tax rate on wealth above the exemption threshold was reduced from 2.2% to 1%. While the change in tax rate was large, this change was also gradual, and happened over three years, as showed in Appendix Figure VI.4, Panel A and B. The wealth tax was then entirely

abolished between 1996 and 1997. Those two reforms provide alternative identifying variation to study migration responses to the wealth tax.

We use the same identification strategy and compare out-migration patterns of wealthy tax payers above the wealth tax exemption threshold to those of taxpayers below that threshold. In Denmark, this threshold was more stable at a higher level in the wealth distribution than in Sweden. We therefore define our treatment group as individuals belonging to households in the top 1% of the wealth distribution, and our baseline control group consists of individuals in the top 5 to top 2.5% of the distribution. The evolution of out-migration patterns around those changes for treated (red series) and control (blue series) individuals are showed in Panel C and D of Appendix Figure VI.4 and we report in Panel A of Figure 12 the corresponding semi-elasticity estimates using the same specification (6) as before. Consistent with our 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. The main takeaway from Figure 12 is that the migration elasticities we obtain exploiting the Danish and Swedish reforms are remarkably similar, and not statistically different, despite being estimated in different countries, contexts, and datasets.[42]

6.4 In-Migration Responses

Our analysis in this section has so far focused on out-migration. But in-migration responses do matter too, as the total effect of taxation on the wealth tax base depends on net migration rates of the wealthy. To measure how wealth tax reforms influence the in-migration margin, 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-difference specification (5) used above. The estimated coefficients are plotted in Appendix Figure VI.3 and show a significant but small positive effect of the reform on return migration, building up over time. While these results demonstrate that the

[42] We note that the graphical evidence is noisier in Denmark, mostly for two reasons. First, we have more power in Sweden because the share of taxpayers subject to the wealth tax is larger in Sweden prior to the 2006 reform, and Sweden is a larger country. Second, the design offered by the Swedish context is better for identification. The Danish reforms are gradual whereas the Swedish change in wealth tax rate is large, sudden and not expected.

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-difference identification of in-migration elasticities similar to the one carried in section 6.3 above for out-migration. Estimates are displayed in Panel B of Figure 12, 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 .05 percentage point. 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 .22 percentage point. 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.

7 Aggregate Implications of Wealth Tax-Induced Migration

In the previous section, we provided evidence of significant effects of wealth taxation on migration flows that appear immediate and persistent. To draw policy implications, we need to translate our estimated effects on migration flows into effects on the stock of the population of the very wealthy, and on the stock of taxable wealth.

7.1 Interpreting the Magnitude: Stock Elasticity

Although our estimated effects on migration flows are very small, these flow effects can 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? In Appendix section VII, we show that the total effect on the steady-state stock of population depends on the relative magnitude of the flow migration elasticity and of the natural rate at which the stock of the population of the wealthy

regenerates itself, through births and deaths, and through the creation, destruction and transmission of wealth. We explain the way these forces play out in the context of a simple OLG framework, and obtain formulae for the quantification of the stock elasticity. These formulae depend solely on our estimated flow elasticities and on moments that capture the replacement rate of the wealthy population. These moments can be easily measured in the data.

Our preferred quantification approach relies on the following simplified formula, which captures the total effect of a change in the wealth tax rate on the size of the population of wealthy individuals:

$$\frac{dN/N}{d(1-\tau)} \approx \varepsilon \cdot \frac{(T+1)}{2} \tag{7}$$

To be implemented, this formula only requires our estimate of the average semi-elasticity of net outmigration flows ε from the previous section (i.e., accounting for both out- and in-migration flow effects), and a measure of T, the average "lifespan" of a wealthy individual. Note that T captures the speed at which the population of the wealthy regenerates itself in the absence of migration.[43] The longer individuals's 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 (7) has a simple interpretation: to get an estimate of the effect on the population stock N, 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.

Based on our estimates, we find that the percentage change in the size of the wealthy population when the effective tax rate on wealth is increased by 1 percentage point is:

$$\frac{dN/N}{d(1-\tau)} = 1.76 \quad (0.49)$$

This estimate confirms that, even when we properly cumulate the flow migration effects, the impact of wealth taxation on the size of the population of the very wealthy remains extremely modest. The main reason behind the small magnitude of this aggregate effect is that migration flow rates are very small to start with among the wealthy. In panel A of Figure 13, we explore the sensitivity of our calibrations. We show that our conclusions are very robust to our assumptions on replacement rates

[43] In the steady-state, T is simply the inverse of the birth rate of individuals into the population of wealthy individuals: $B = 1/T$.

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 VII.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.[44] 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., Alstadsæter et al. (2019)). 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 $(1-\tilde{\tau})$:

$$\frac{dN/N}{d(1-\tilde{\tau})} = \frac{dN/N}{d(1-\tau)} \cdot \frac{1}{(1-e)} \tag{8}$$

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). 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.[45] 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 13 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 1.9 and the lower bound elasticity is 1.83, 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.

Elasticity w.r.t Net-of-Tax Rate on Capital Income: While our estimated semi-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.[46] We find that

[44] All details regarding our sensitivity analysis can be found in Appendix VII.

[45] 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.

[46] To do this, we 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

the elasticity $\varepsilon_{N,1-t}$ of the size of the wealthy population with respect to the net-of-tax rate on capital income is:

$$\varepsilon_{N,1-t} = \varepsilon_{N,1-\tau} \cdot \frac{d\ln(1-\tau)}{d\ln(1-t)} \approx .048 \quad (.013) \tag{9}$$

When appropriately rescaled, the implied migration elasticity is thus small. Figure 13 panel B 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 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 discrepancy is consistent with the fact that we focus on international migration responses, which are generally found to exhibit lower elasticities than within-country mobility.[47]

7.2 Aggregate Tax Revenue Implications

How much constraints do these behavioral migration responses impose on tax policy? A natural way to answer this question is to measure the aggregate tax revenue implications of these migration responses when increasing the tax rate on wealth, accounting for all tax externalities generated by the wealthy when they migrate. When focusing solely on these extensive migration responses, the impact of a change in wealth tax τ on total tax revenues ($dGBC$) is the sum of a mechanical effect $N \cdot W \cdot d\tau$, and of a behavioral effect $dN \cdot \mathbb{T}$, where N is the size of the wealthy population, W is average wealth among the wealthy and $\mathbb{T} = \mathbf{t} + \tau W$ are total taxes paid by the wealthy, including all non-wealth taxes $\mathbf{t}$.

The overall distortions imposed by migration responses are simply determined by the ratio of the

rate on capital income. Over our period of study, we observe in our data $r = .042$ and $\tau \approx .006$ which translates into $t = 14.3\% + 20\%$. All details of our computations are available in Appendix VII.

[47] Agrawal et al. (2023) also suggest that observed within-country mobility patterns mask significant avoidance rather than real location responses. In their context for instance, a significant fraction of reported mobility appears to be tax avoidance, wealthy taxpayers using their secondary homes as primary addresses.

behavioral to mechanical effects of a change in the tax rate on wealth:

$$dGBC/d\tau = N \cdot W(1 - \frac{dN/N}{d(1-\tau)}(\mathbf{t}/W + \tau)) \tag{10}$$

To measure the behavioral revenue effect, we use the average $\tau = .6\%$ for the top 2% of wealthy individuals observed over the period 1999-2006 in Sweden. And we find that $\mathbf{t}/W = 13.1\%$.[48] Using our semi-elasticity estimate $\frac{dN/N}{d(1-\tau)} = 1.76$, this means that the behavioral revenue effect is equal to .24. In other words, for every additional Swedish Kr of tax revenues levied mechanically by raising the wealth tax rate by $d\tau$, only .24 Kr are lost due to behavioral migration responses. This clearly outlines that migration responses alone, even accounting for all tax externalities, are too small to suggest that the Swedish wealth tax rate was anywhere near the Laffer rate before 2007.

7.3 Aggregate External Effects on Employment and Economic Activity

Our results suggest that the fiscal implications of migration responses to the wealth tax are small. But in policy debates, the focus is often shifted to the negative externalities on aggregate employment, investment and business dynamism that may be exerted by outmigration of the very wealthy.

For any aggregate outcome Y such as employment, investment or value-added, the migration-induced effect on Y of an increase in the wealth tax rate is simply given by:

$$\frac{dY/Y}{d(1-\tau)} = \underbrace{\frac{NY^w}{Y}}_{\text{Top Wealth Share of Y}} \times \underbrace{\frac{\partial Y^w}{\partial N} \cdot \frac{1}{Y^w}}_{\text{Migration Impact}} \times \underbrace{\frac{\partial N}{\partial(1-\tau)} \cdot \frac{1}{N}}_{\text{Migration Semi-Elasticity}} \tag{11}$$

where Y^w is the average outcome (i.e. employment, investement, etc.) generated by a wealthy entrepreneur. In other words, we can measure these external effects by simply combining our estimate of the semi-elasticity of population w.r.t. net-of-tax rate on wealth, with our event-study estimates of the impact of migration on firms outcomes from section 5.2, and multiplying this by the share of aggregate Y controlled by the very wealthy that we have measured in Table 1.

Results are reported in Figure 14 and all details about these computations are available in Appendix VII. To understand these estimates, we provide as an example the decomposition of the aggregate effect for employment. We have estimated that a 1 percentage point increase in the effective average tax rate on wealth causally decreases the steady-state stock of wealthy entrepreneurs by 1.76%. These entrepreneurs control (directly or indirectly) firms that represent 9.2% of total employment

[48] Note that we measure $\mathbf{t}$ as total non-wealth tax payments paid by the wealthy in Figure 5 plus the total change in corporate tax payments made by firms directly or indirectly owned following a migration event from Figure 7.

in the Swedish economy (Table 1 panel C). And their migration causes a 18.7% reduction in employment in the firms they own directly or indirectly (Figure 8 panel B). As a consequence, we estimate that a one percentage point increase in the effective average tax rate on wealth causally decreases aggregate employment by $(9.2\% * 18.7\% * 1.76\%) = .030\%$ through tax-induced migration of the wealthy.

The main insight from Figure 14 is that the migration-induced effects of wealth taxes on overall economic activity are extremely limited. A one p.p. increase in the effective tax rate on wealth of the top 2% of wealthiest taxpayers is found to decrease total employment by .03%, total investment by .04% and total value-added by only .09%. This is despite the fact that wealthy entrepreneurs account for a substantial share of overall economic activity through the firms that they control directly and indirectly. The main reason for such small effects lies in the very small migration elasticity, which itself is largely due to the fact that migration flows at the top of the wealth distribution are actually remarkably tiny.

We should also stress that our results are calibrated based on a 1 percentage point change in the effective tax rate on wealth. Such a variation is sizeable: it is twice the size of the variation induced by the abolition of the Swedish wealth tax for the top 2% of wealthiest taxpayers. This implies that the long run effects on economic activity of the abolition of the Swedish wealth tax are twice smaller than the ones reported in Figure 14. Another way to present these effects is to compute the implied fiscal cost per job "created" by migration responses induced by the abolition of the wealth tax in Sweden. We find that each job created cost about SEK 2,341,000 Swedish Kr. of tax revenues (i.e. approximately 230,000 euros of public funds per job created), which is about ten years of the average salary in Sweden at the time.

Our quantification procedure relies on a few assumptions that are worth highlighting and discussing. First, for the migration impact $\frac{\partial Y^w}{\partial N} \cdot \frac{1}{Y^w}$, we take estimates that account for firms held both directly and indirectly, but we do not use our estimates accounting for buy-outs. This means that the effects we are estimating are clear upper bounds on the actual impact of migration, as they do not account for the important reallocation effects documented in section 5.2. Second, our calibration uses our estimates of the event-study impact for outmigration from Figure 8, and assumes perfect symmetry in the effect of in- and out-migration on firms' outcomes. We have however noticed in panel A of Figure 8 that in-migration effects are somewhat smaller than out-migration effects for a few outcomes like investment or value-added. This in turn means that our estimate of the aggregate external effect is an upper-bound on the true effect.[49]

[49] We should also mention that we have used for the migration elasticity our baseline elasticity for the whole population of wealthy taxpayers. In practice, the elasticity that matters is the migration elasticity for the population of entrepreneurs. Fortunately, our estimates from Figure 12 confirm that the semi-elasticity estimates for in- and out-migration flows for firm owners are almost identical to the estimates for the wealthy population as a whole. While the

Our two-step procedure, which separately identifies migration elasticities and migration impacts before combining these two set of estimates, also relies on the implicit assumption of no treatment effect heterogeneity in migration impacts. This allows us to use the estimates from the event-study design in section 5.2 to calibrate $\partial Y^w/\partial N$. But the LATE estimate for migration impacts that we identify in our event study design might not be the same as the LATE on the individuals who are at the margin of migration with respect to a change in the wealth tax rate (and decide to migrate when the wealth tax is abolished). A worry would therefore be that the repeal of the wealth tax changed the way out-migration events and the out-migrants' firms outcomes co-vary. In Appendix Table IV.5, we provide direct evidence that this was not the case: the effects of wealthy owners' out-migration are shown to be quantitatively and qualitatively similar before and after the repeal of the wealth tax.[50]

8 Conclusion

This paper analyzes the international migration patterns of the very wealthy in Scandinavia and their responsiveness to wealth taxation. Using exhaustive administrative registries on international migration, wealth and entrepreneurship, we provide a detailed description of the real economic implications of wealthy out-migration. We show that wealthy out-migration is associated with tax revenue losses from wealth, income, and local taxes. We also find evidence of negative spillovers of out-migration through reduced employment, investments, and tax payments at firms held by wealthy entrepreneurs. To isolate the contribution of wealth taxation to out-migration, we exploit three large reforms in Scandinavia. We provide clear graphical evidence of international migration responses to wealth taxes. We find that a one percentage point increase in the effective average tax rate on wealth increases net out-migration by .22 percentage point (+0.17 pp for out-migration and -.05 pp for in-migration).

Putting all this evidence together allows us to explore the aggregate implications of wealth tax-induced migration on a range of outcomes. Our results indicate that these aggregate effects are small. A one percentage point decrease in the effective average tax rate on wealth increases the

semi-elasticities of migration flows are similar, the semi-elasticities on the stock may differ if the average birth and death rates of entrepreneurs at the top of the wealth distribution differs from the rest of the wealthy population. The data indeed suggests that the hazard rate out of the population of wealthy conditional on being in Sweden is larger for entrepreneurs, which may reflect the fact that they engage in riskier investments than the rest of the wealthy population. This in turn suggests that our estimate is an upper-bound on the migration elasticity of entrepreneurs.

[50] One alternative would have been to look at the evolution of economic outcomes after 2007, using the wealth tax repeal as an exogenous shifter of wealthy entrepreneurs out-migration flows. The issue is that the reform is not a good instrument for the effect of migration on economic outcomes, such as firm-level employment, investment, or value-added. This is because the tax change could have also affected those outcomes through intensive margin responses (e.g., changes in wealth accumulation and allocation as in Jakobsen et al. (2020) or Le Guern Herry (2023)), while we want to isolate migration-induced effects. Our two-step procedure's homogeneity assumption is more straightforward to test compared to the exclusion restriction assumption required for the alternative IV approach.

size of the wealthy population by at most 2% in the long run, with an induced impact on aggregate employment and total investment in the economy of .03% and .04%, respectively. How can we explain such small effects? This boils down to a simple fact: our data allow us to show that the overall migration flows at the top of the wealth distribution are remarkably small, and this is what mostly determines the small magnitude of aggregate effects from tax-induced migration.

Our paper provides a validation of the robustness of our estimates using results from two different wealth tax environments, Sweden and Denmark. While this validation is useful, these two countries are relatively similar. Both countries are small open economies where migration responses may be stronger: migration elasticities are typically found to decrease with country size and increase with the degree of openness of the economy. It would also be interesting to explore the portability of our results to a context with stronger effective taxation of business assets.

This paper focuses entirely on extensive margin responses to wealth taxation through migration. But we note that the data assembled here—linking individuals to all their assets, including the business assets they control directly or indirectly—offer a unique opportunity to further investigate the impact of capital taxation on entrepreneurship, investment, innovation, and firm growth at the intensive margin. We leave this important agenda for future work.

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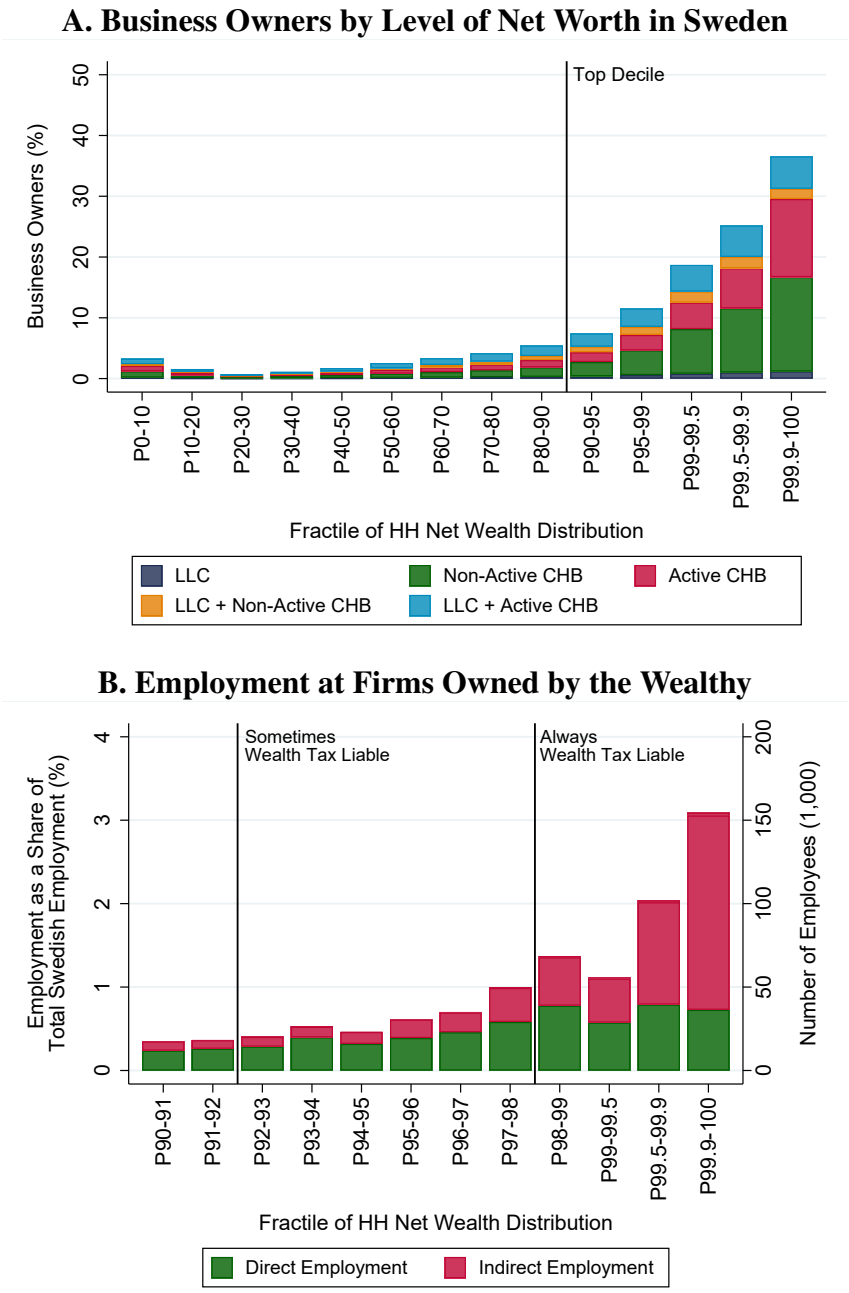
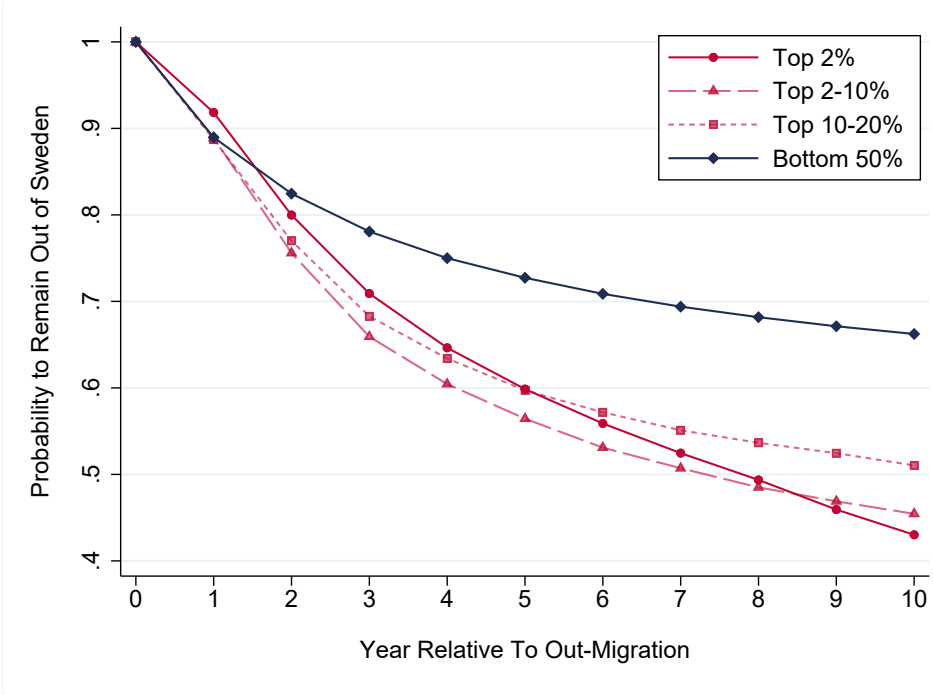
Figures and Tables

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



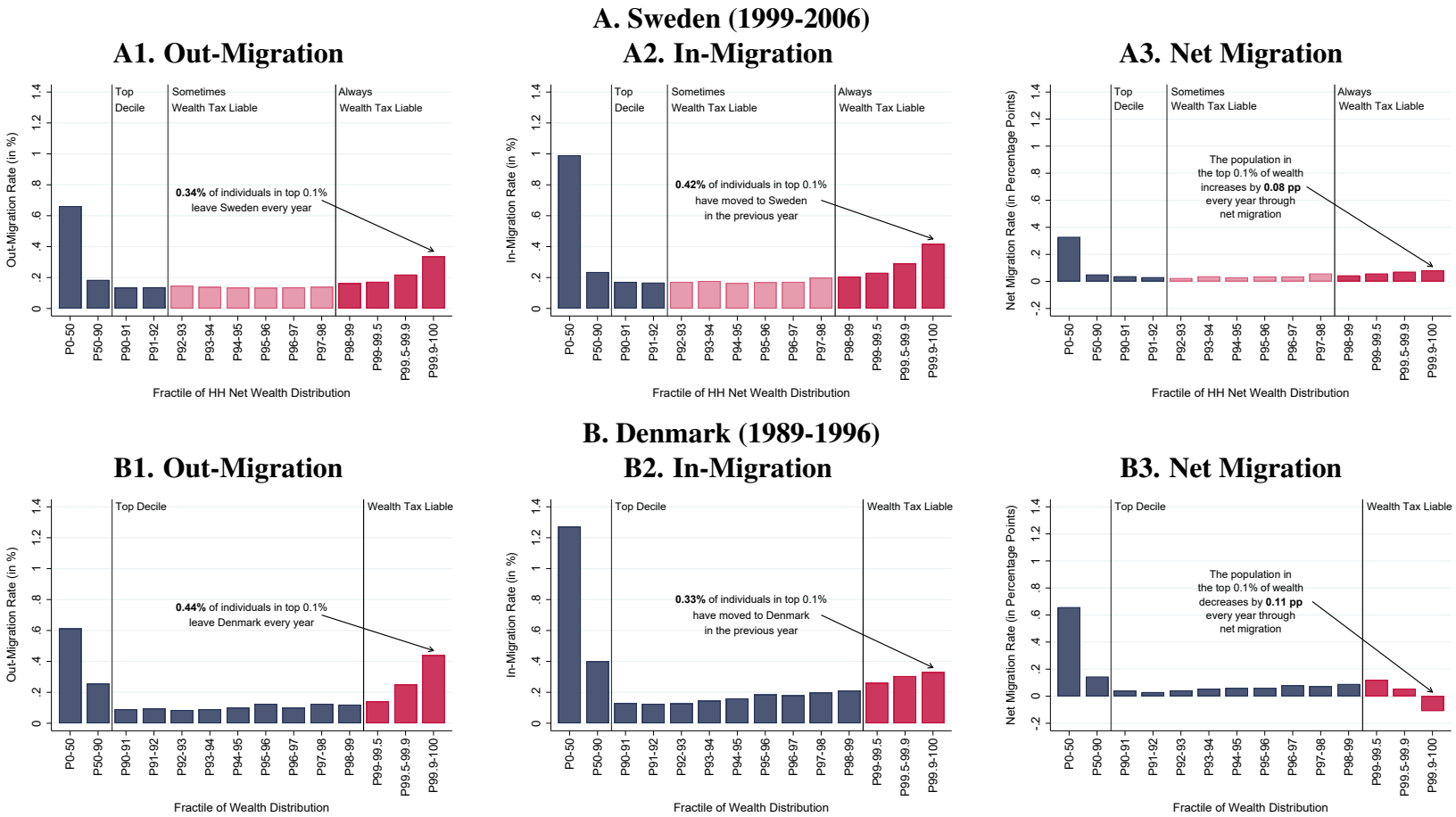
Notes: This figure describes firm ownership in Sweden during the period 2000-2006, when a wealth tax was still in place in Sweden. Each year, we rank households by their net wealth level and assign this rank to the individuals in the household. Panel A shows the share of individuals owning at least one 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 closely-held businesses employing at least one person beyond the owners of the firm. In Panel B, we show the share of 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: **Probability to Remain Out of Sweden After an Out-Migration Event**



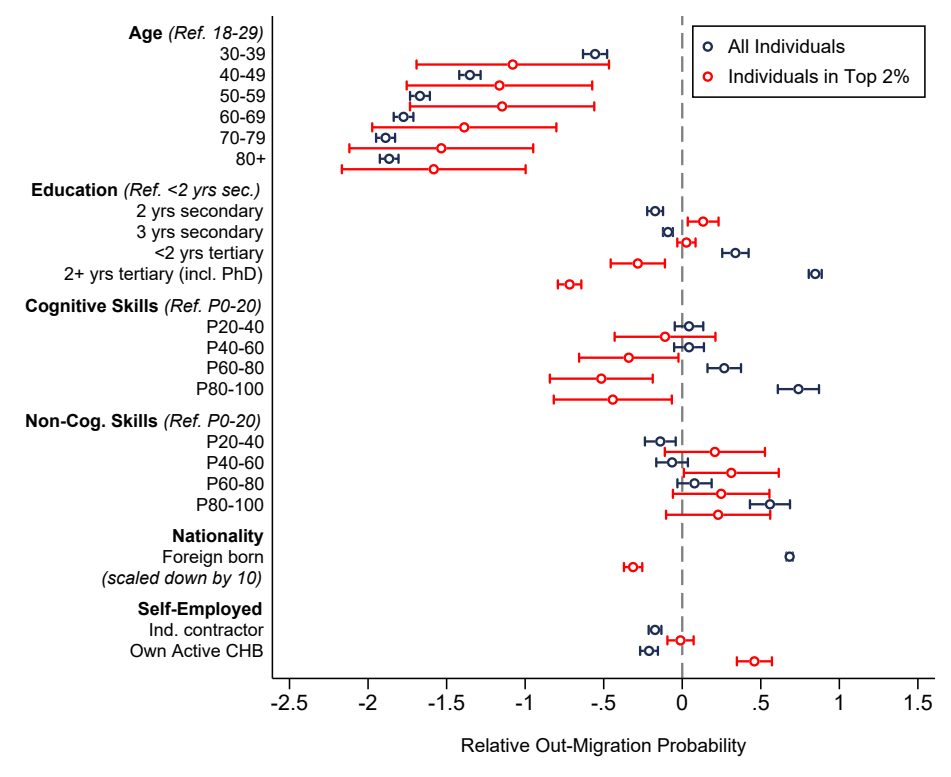
Notes: This figure shows the probability to remain out of Sweden after an out-migration event. The out-migration events considered for this plot cover the years 2000-2007. The wealth groups are based on the net wealth of individuals' household in the year before their out-migration date. Each year that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

Figure 3: **Out-Migration at the Top of the Wealth Distribution in Scandinavia**



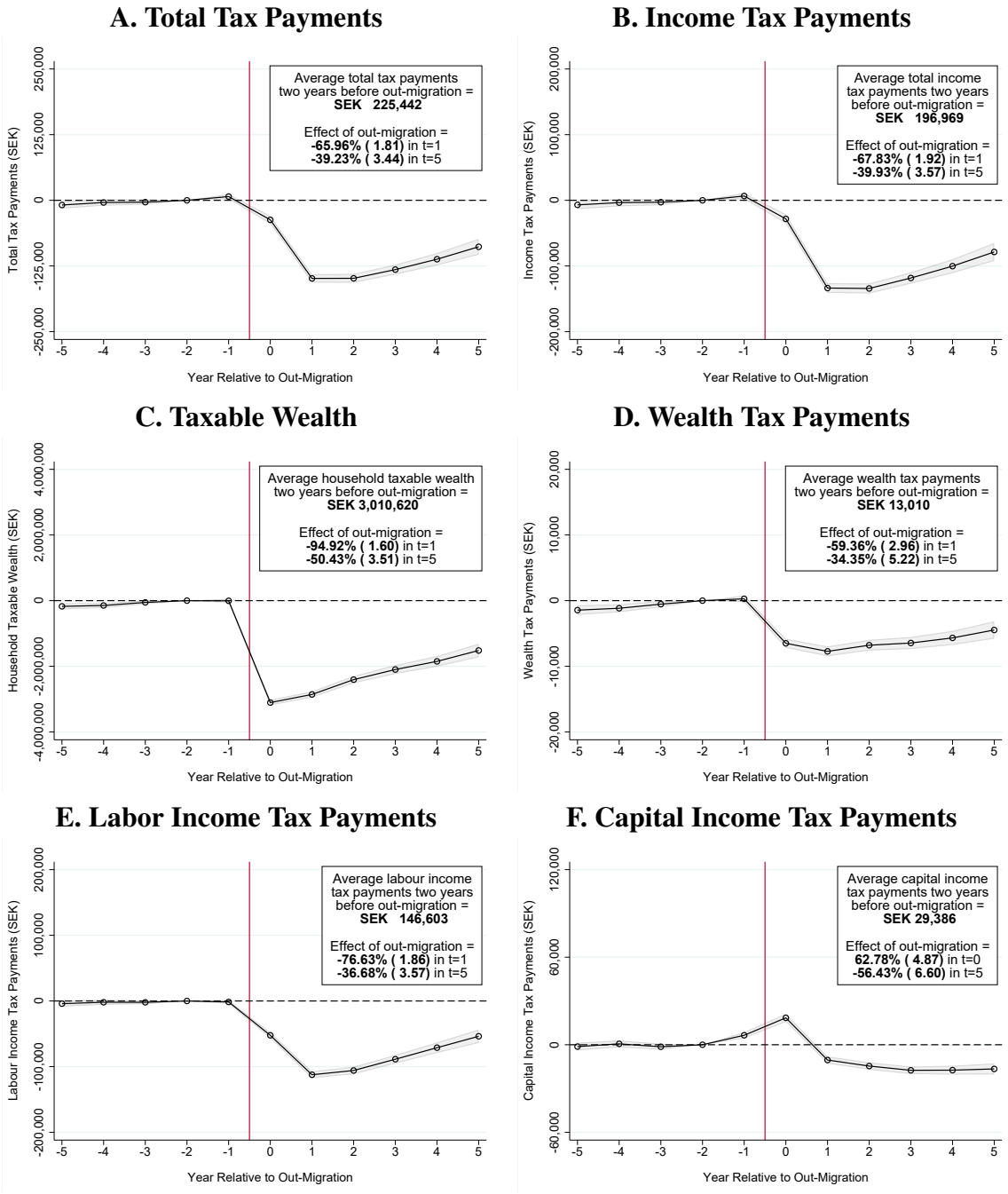
Notes: This figure shows in-migration, out-migration and net-migration rates by level of total household net wealth in Sweden (Panel A) and total household net wealth in Denmark (Panel B). We compute those statistics during the years in which the wealth tax was still in place in each country, which corresponds 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 4: **Selection into Out-Migration from Sweden**



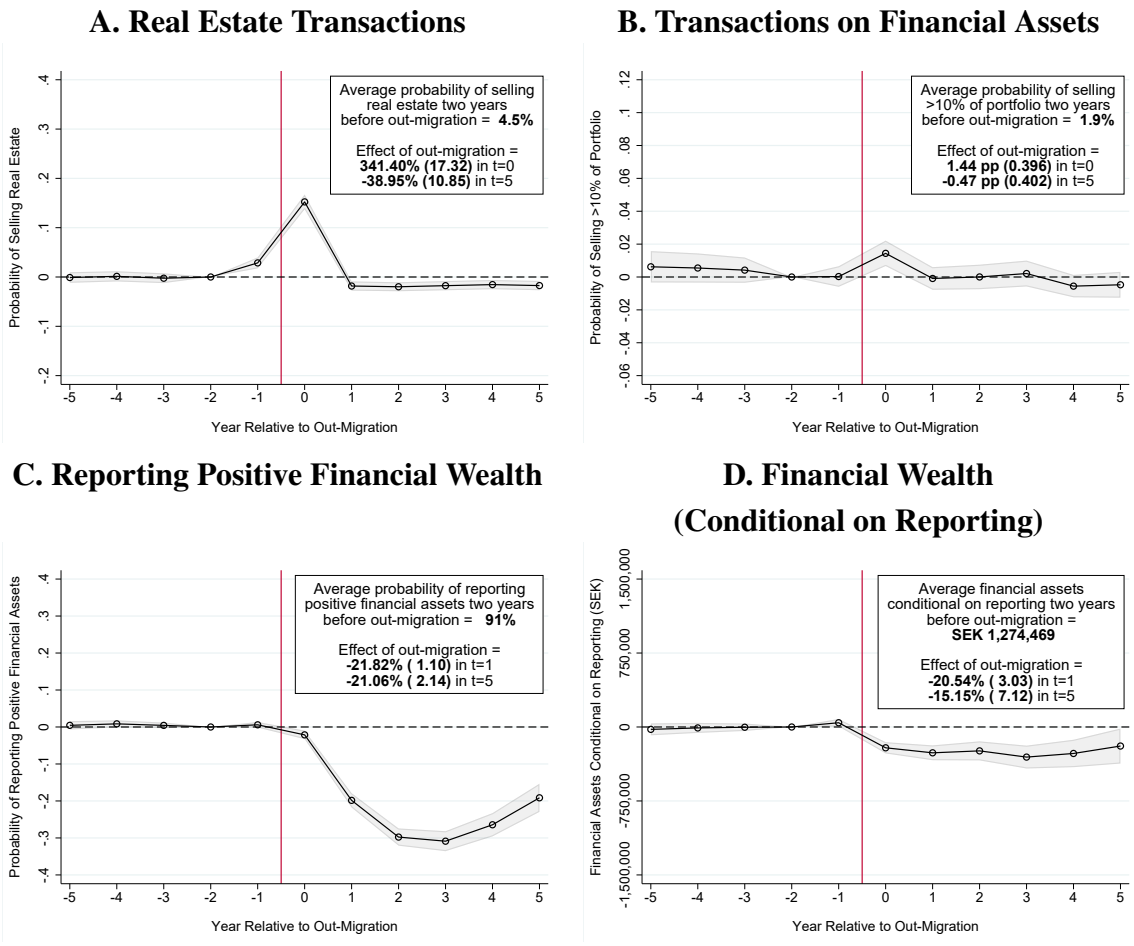
Notes: This figure describes selection into out-migration from Sweden during the period 2000-2007. We estimate Equation 1 and plot the estimated vector of coefficients β and their confidence intervals in blue. We estimate Equation 2 and plot the estimated vector of coefficients β_w and their confidence 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 5: **Effects of Wealthy Out-Migration on Tax Payments, Top 2%**



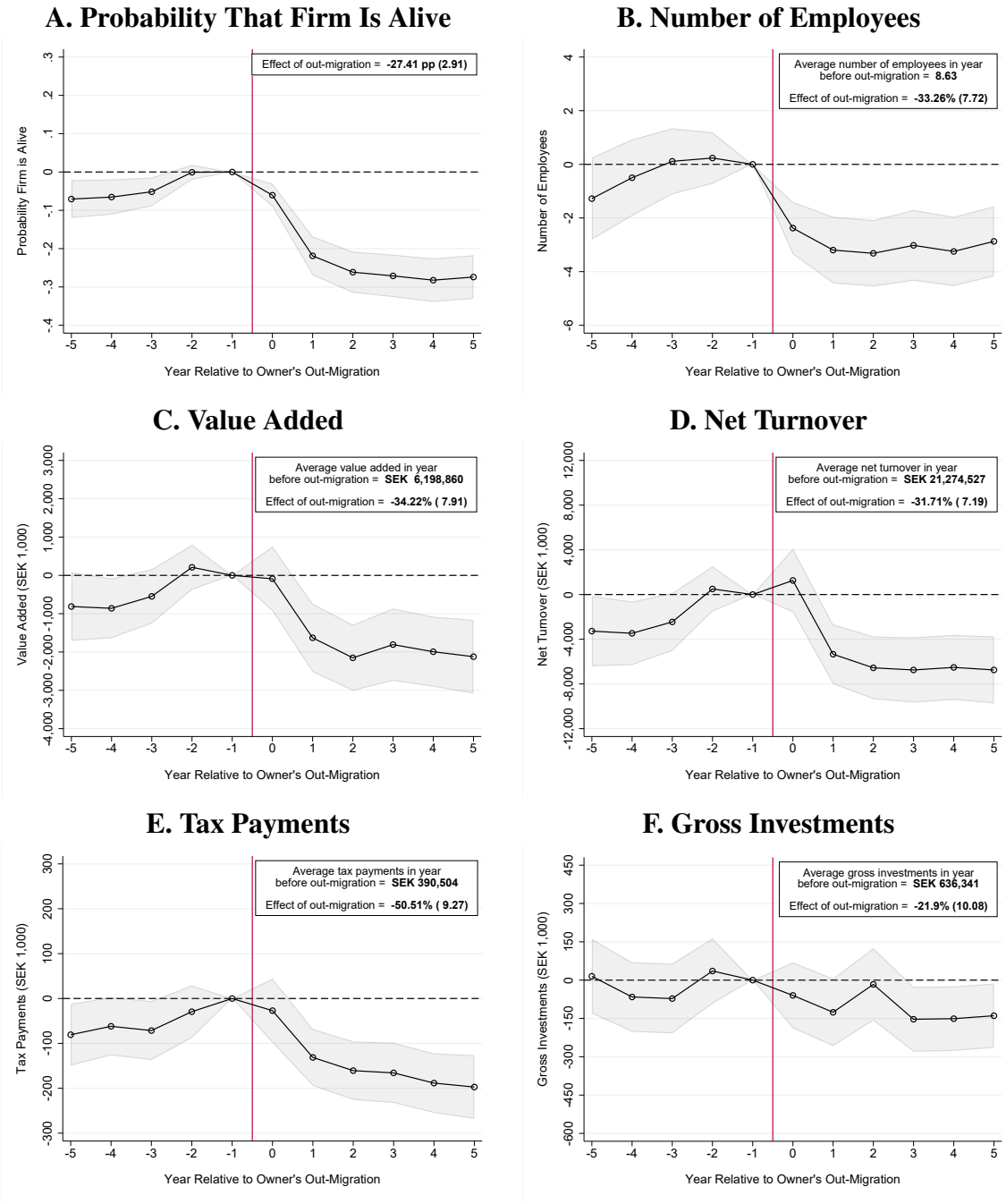
Notes: This figure describes the evolution of wealthy individuals' outcomes before and after they leave Sweden, compared to control wealthy individuals who do not move that same year. The sample includes individuals who were in the top 2% of the household net worth distribution in Sweden for at least one year before their true or placebo out-migration date. We focus on out-migration events occuring between 2000 and 2007, with wealth ranks drawn from 1999-2006, when the wealth tax was in place. We winsorised the bottom 1% and top 5% of all outcomes. We plot the estimates β_j from Equation 3. 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 6: **Effects of Wealthy Out-Migration on Portfolio Reallocation, Top 2%**



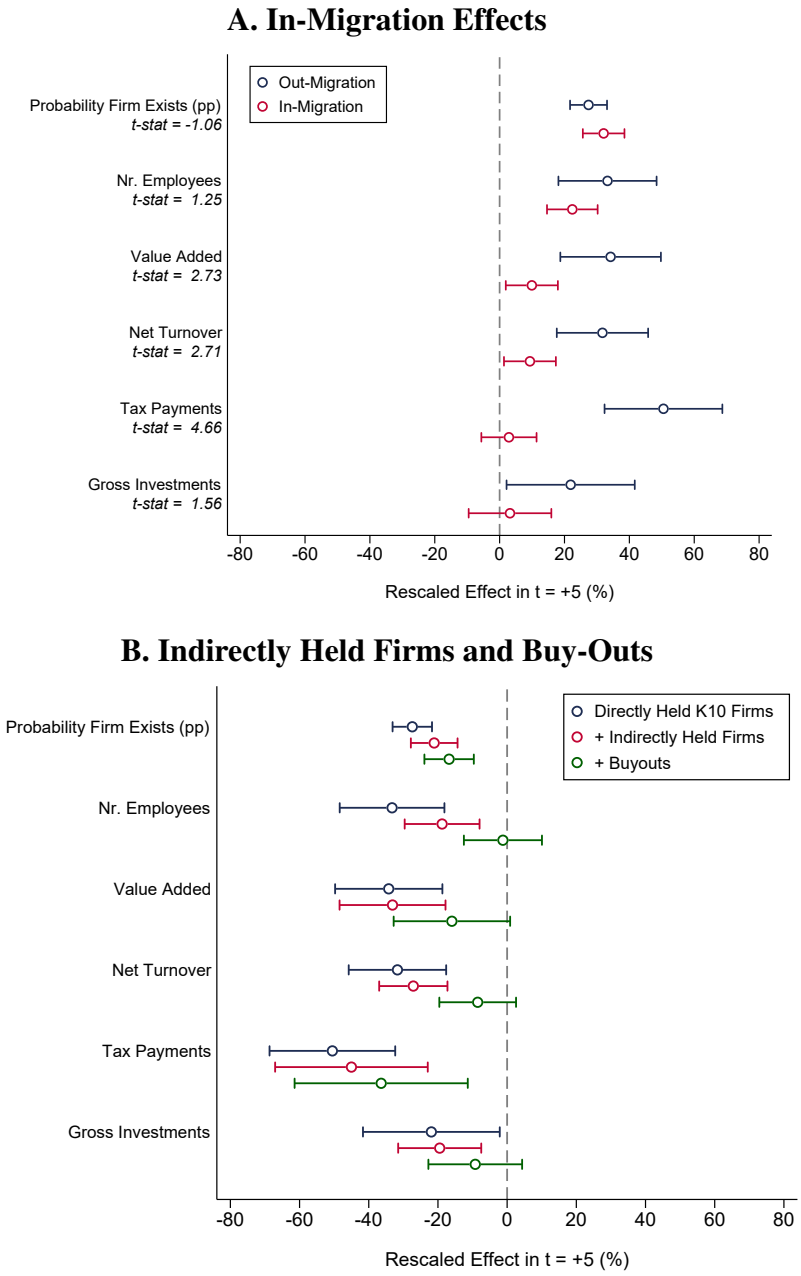
Notes: This figure describes the evolution of wealthy individuals' outcomes before and after they leave Sweden, compared to control wealthy individuals who do not move that same year. The sample includes individuals who were in the top 2% of the household net worth distribution in Sweden for at least one year before their true or placebo out-migration date. We winsorised the bottom 1% and top 5% of all outcomes. We plot the estimates β_j from Equation 3. 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 Outcomes, Top 2%**



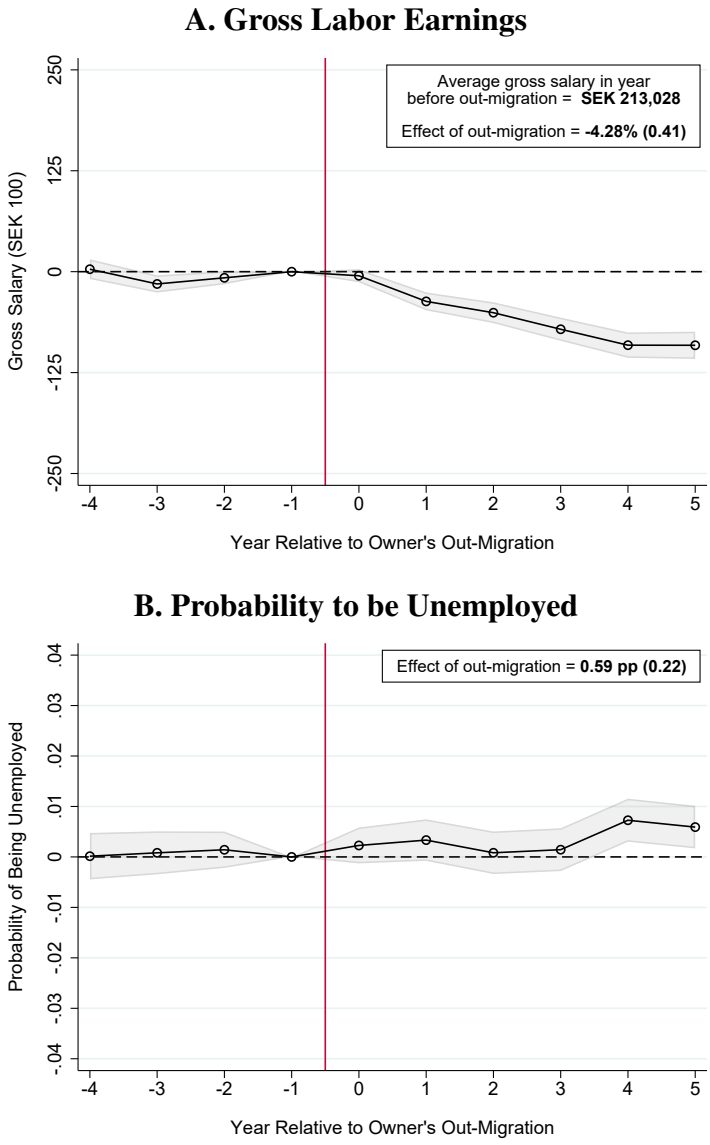
Notes: This figure shows the effects of wealthy owners' out-migration events on firm-level outcomes. We focus on out-migration events occuring between 2001 and 2007, with wealth ranks drawn from 2000-2006, when the wealth tax was still in place in Sweden. The sample includes active closely-held businesses controlled by wealthy individuals in the year $t-1$, with (real or placebo) out-migration events occurring in the subsequent year t. We plot the estimates of β_j and their confidence intervals estimated from Equation 4. 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**



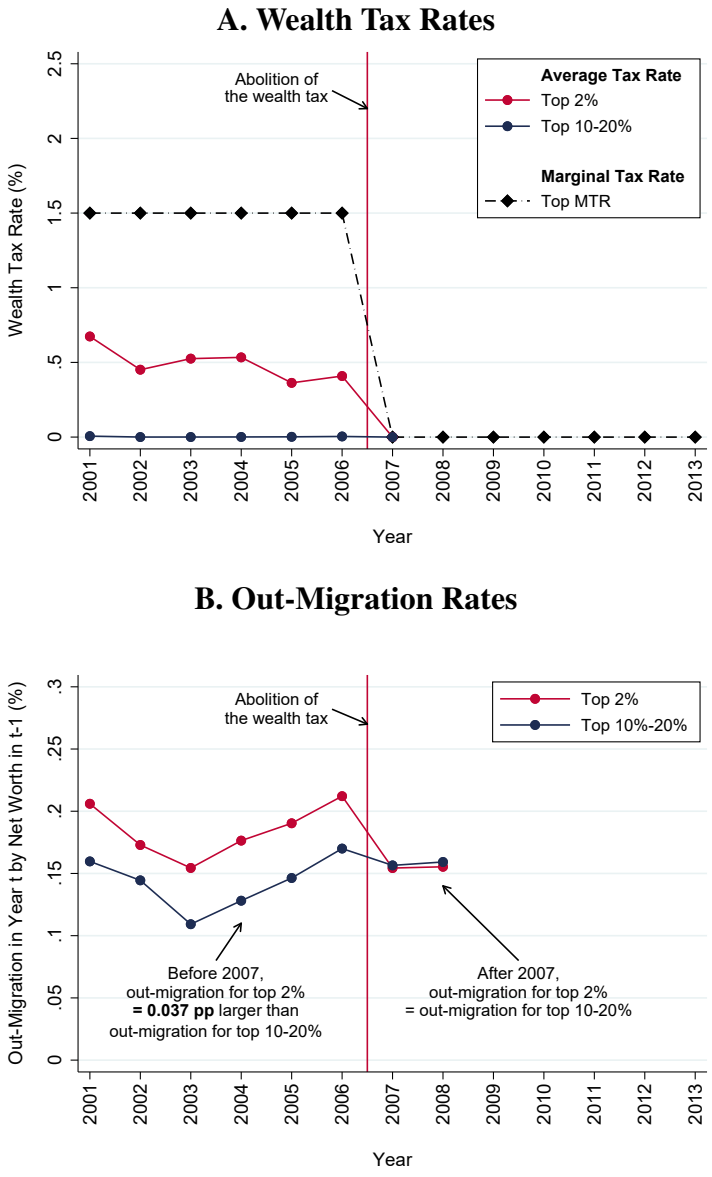
Notes: In Panel A, we study the effects of wealthy owners' in-migration events on firm-level outcomes. We study in-migration events during the period 1999-2005, when a wealth tax was still in place in Sweden. The sample includes active closely-held businesses directly owned by individuals whose real or placebo in-migration happened in year t and who were in the top 2% of the household net worth distribution in Sweden for at least one year after t. We rescaled our coefficients β_5 from Equation 4 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 $t-1$. 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' buy-outs after closure (green dots).

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



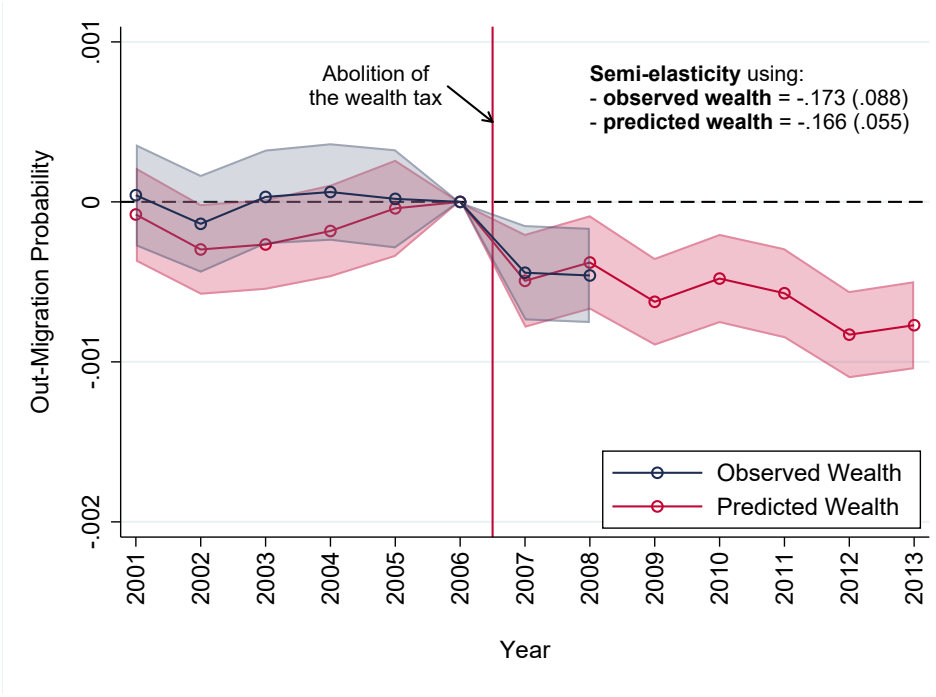
Notes: This figure shows the effects of wealthy owners' out-migration events on worker-level labor market trajectories. We focus on workers employed at firms controlled directly or indirectly by wealthy entrepreneurs in the year before the (real or placebo) out-migration event. Panel A uses gross labor 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 4 and we report β_5 rescaled by the average outcome for the treatment group in year $t-1$ in the text boxes.

Figure 10: **Effect of the Swedish Wealth Tax Reform on Out-Migration Flows**



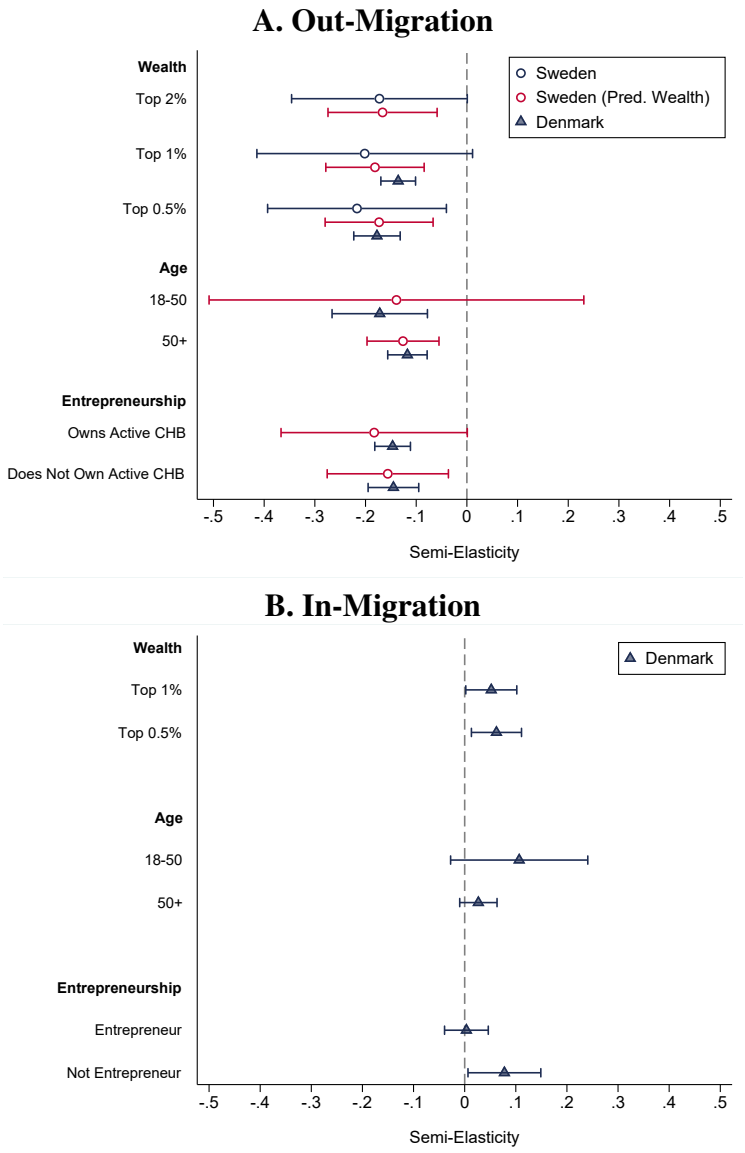
Notes: Panel A reports the evolution of wealth tax rates before and after the repeal of the wealth tax in Sweden. The dotted black line displays the evolution of the statutory marginal tax rate on wealth above the exemption threshold in Sweden between 2001 and 2013. We show the corresponding evolution of the average effective tax rate (defined as total wealth tax payments over total household taxable 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 household net wealth distribution (control group, blue series). Panel B reports out-migration rates for individuals with wealth above the wealth tax exemption threshold (treatment group, red series) and individuals just below the exemption threshold (control group, blue series), from 2001 to 2008. The vertical red line at year 2007 in both panels denotes the year the wealth tax was repealed by the new Swedish government.

Figure 11: **Out-Migration Responses to the Wealth Tax Repeal: Difference-in-Differences**



Notes: This figure shows 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 5 and their 95 percent confidence intervals. The semi-elasticities shown in the plots correspond to ϵ estimated from Equation 6. For example, the semi-elasticity computed using true household net wealth indicates that a 1 percentage point increase in the net-of-tax rate on wealth decreased the out-migration rate of individuals in the top 2% of the household net wealth distribution in Sweden by 0.17 percentage points.

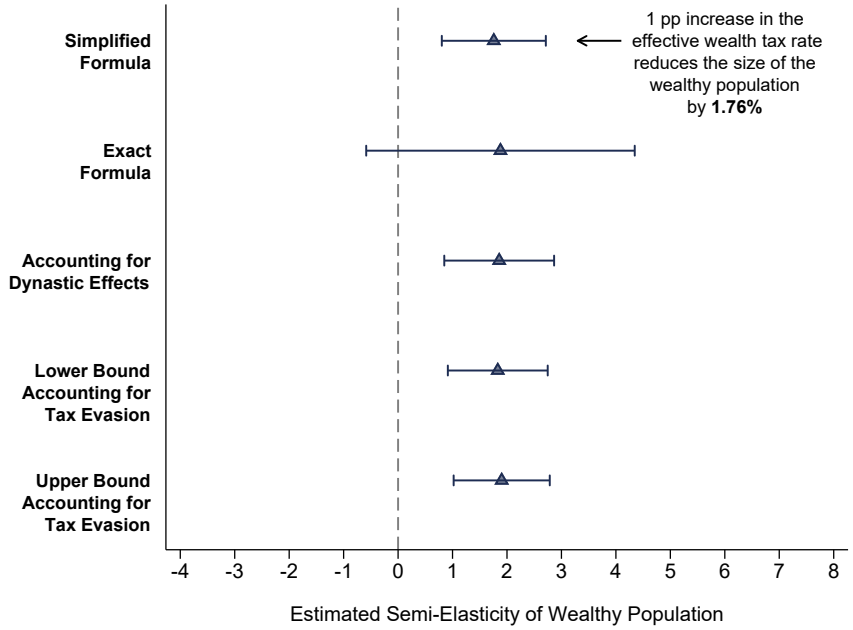
Figure 12: **Semi-Elasticities of International Migration Flows**



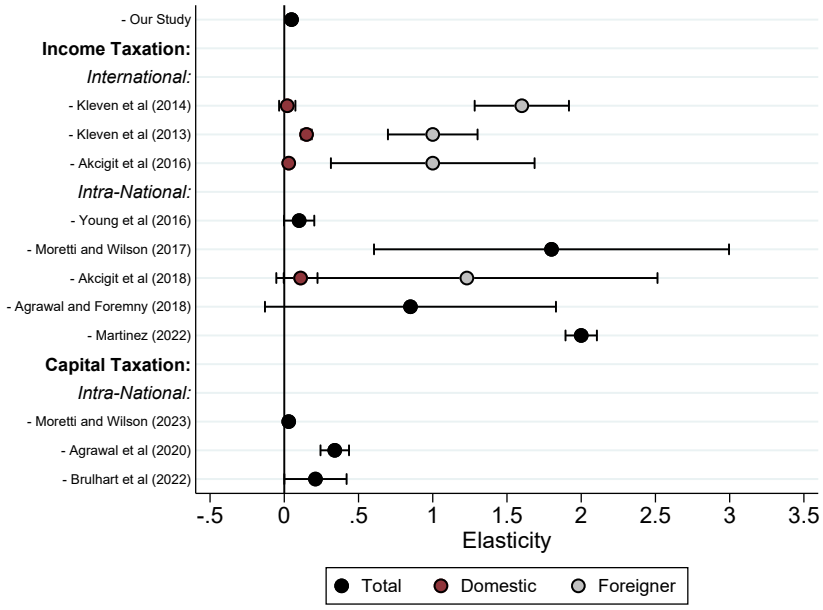
Notes: The semi-elasticities plotted in the figure correspond to ϵ estimated from Equation 6. Each coefficient and its confidence intervals refer to one separate regression. In Panel A, we estimate semi-elasticities of out-migration flows exploiting the repeal of the wealth tax in Sweden (blue and red circles) and two large wealth tax reforms in Denmark (blue triangles). In Panel B, we estimate semi-elasticities of in-migration flows exploiting two large wealth tax reforms in Denmark (blue triangles).

Figure 13: **Elasticities of the Size of the Wealthy Population With Respect To Taxes**

A. Semi-Elasticity With Respect To the Net-of-Tax Rate on Wealth

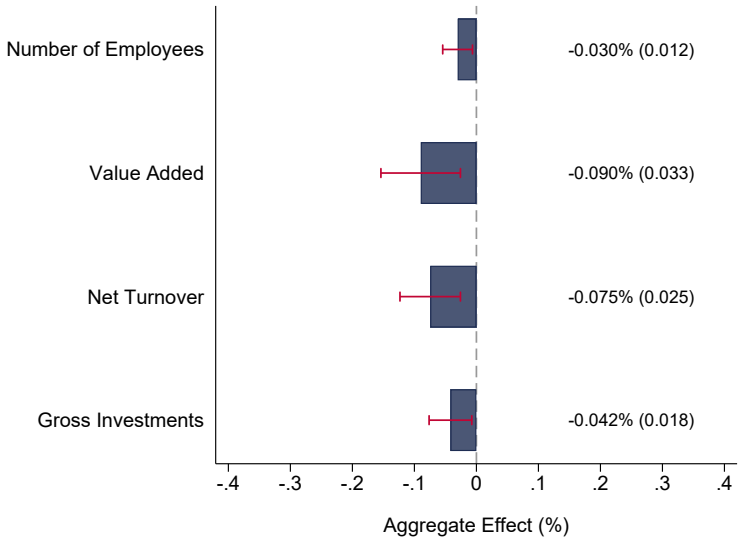


B. Elasticity With Respect To the Net-of-Tax Rate on Income



Notes: This figure 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 12 over time building on the model detailed in Appendix VII. Panel B compares our estimates of migration elasticities with respect to the net-of-tax rate on wealth to (converting taxes on stock to taxes on flows as explained in the text) to comparable estimates in the literature, focusing on income or capital taxation.

Figure 14: **Aggregate Migration Implications of a 1 p.p. Increase in the Effective Tax Rate on Wealth**



Notes: This figure presents the outcomes of our quantification exercise, which evaluates the aggregate economic effects of migration responses to a one-percentage-point increase in the effective tax rate on wealth. This exercise is described in details in Appendix VII and in the main text. We combine our estimates of the migration stock elasticities presented in Figure 13 and the reduced-form effects of out-migration and in-migration events on firm-level outcomes presented in Panel A of Figure 8.

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

Variable	Mean	Median	Std. Dev.	Obs.	% of Swedish Aggregates
Panel A. All Active CHBs					
Nr. of Owners	1.78	1	7.05	589,788	
Nr. 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%
Tax Payments	138	21	4,708	541,097	27.64%
Gross Investments	534	55	4,661	541,097	17.88%
Panel B. Active CHBs with at least one owner in the top 2% of net worth					
Nr. of Owners	2.44	2	17.91	89,485	
Nr. 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%
Tax Payments	386	56	3,653	82,473	10.68%
Gross Investments	1,271	100	10,940	82,473	6.41%
Panel C. Active firms with at least one direct or indirect owner in the top 2% of net worth					
Nr. of Owners	5.61	2	72.38	138,067	
Nr. 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%
Tax Payments	502	58	5,157	128,602	18.98%
Gross Investments	1,646	118	16,418	128,602	12.22%

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, tax payments, and gross investments is SEK 1,000. Value added, net turnover, tax payments, gross investments as percentages of Swedish aggregates (last column) are obtained by dividing total value added, net turnover, tax payments, gross investments 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.

APPENDIX

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

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

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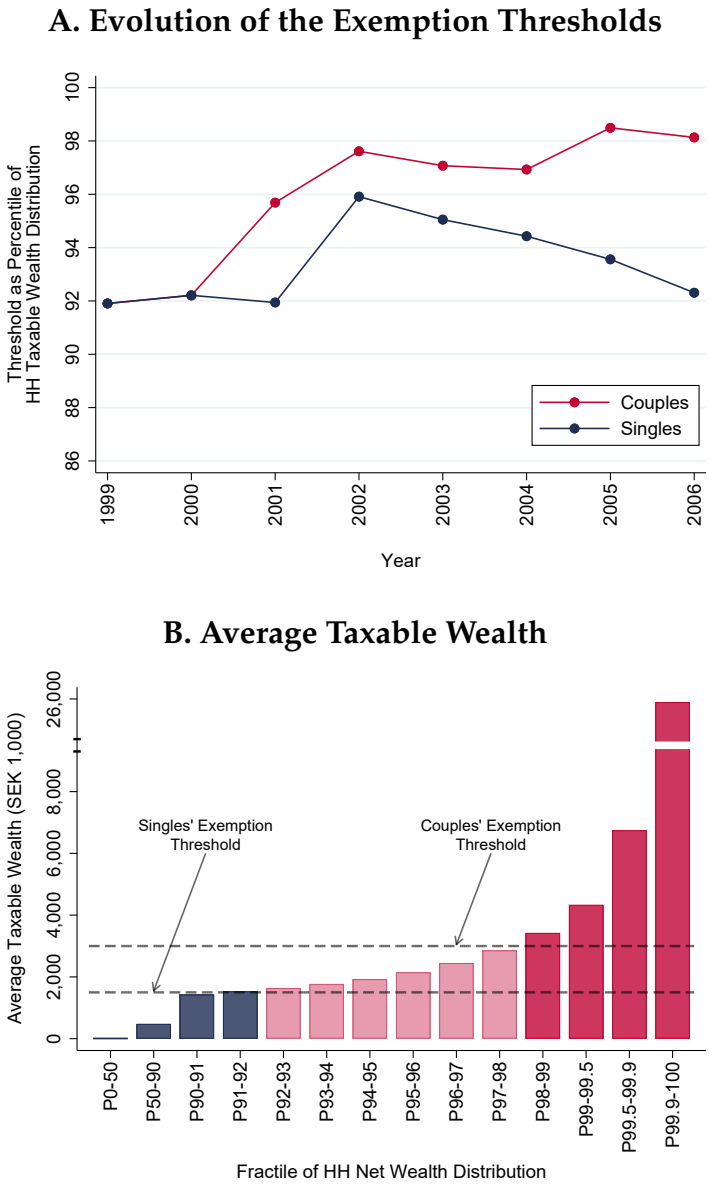
I. Additional Institutional Details

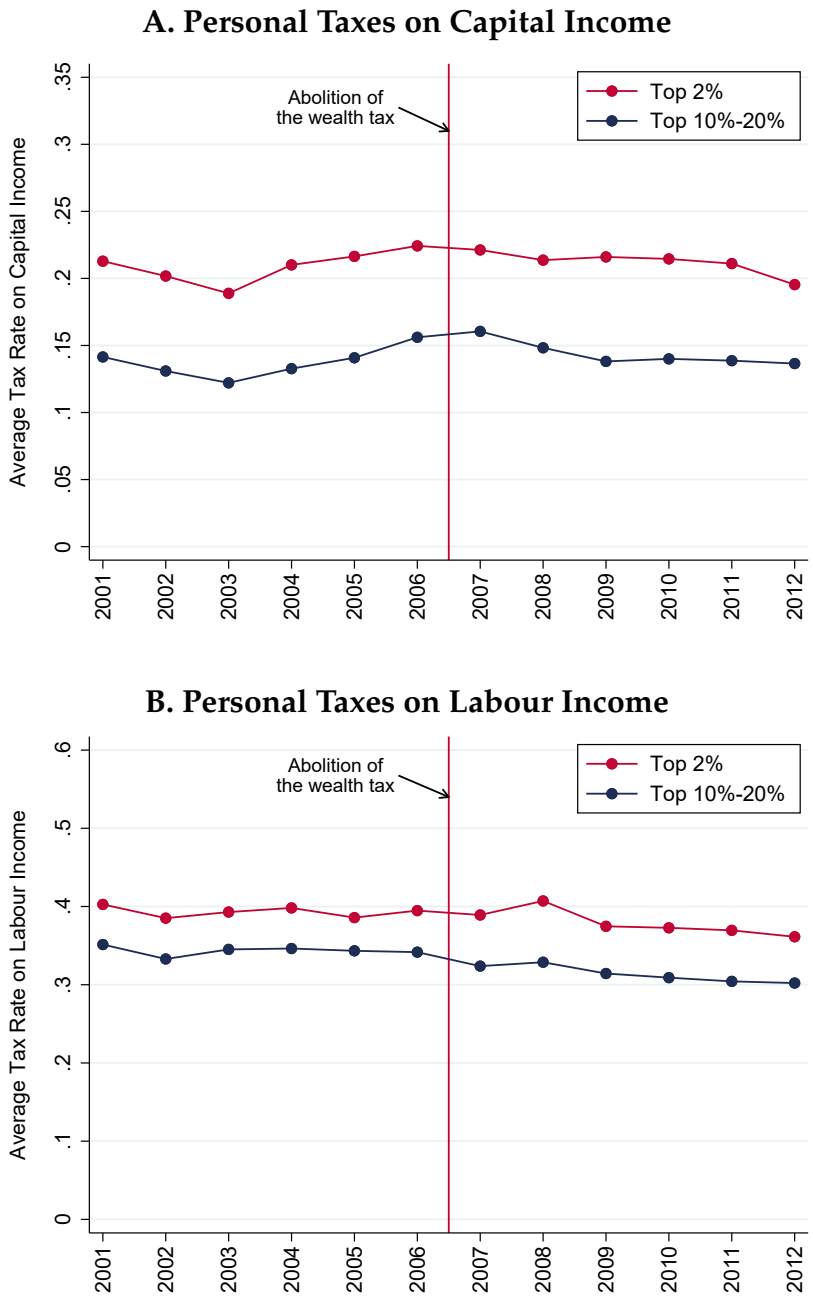
I.1. Additional Institutional Details: Sweden

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



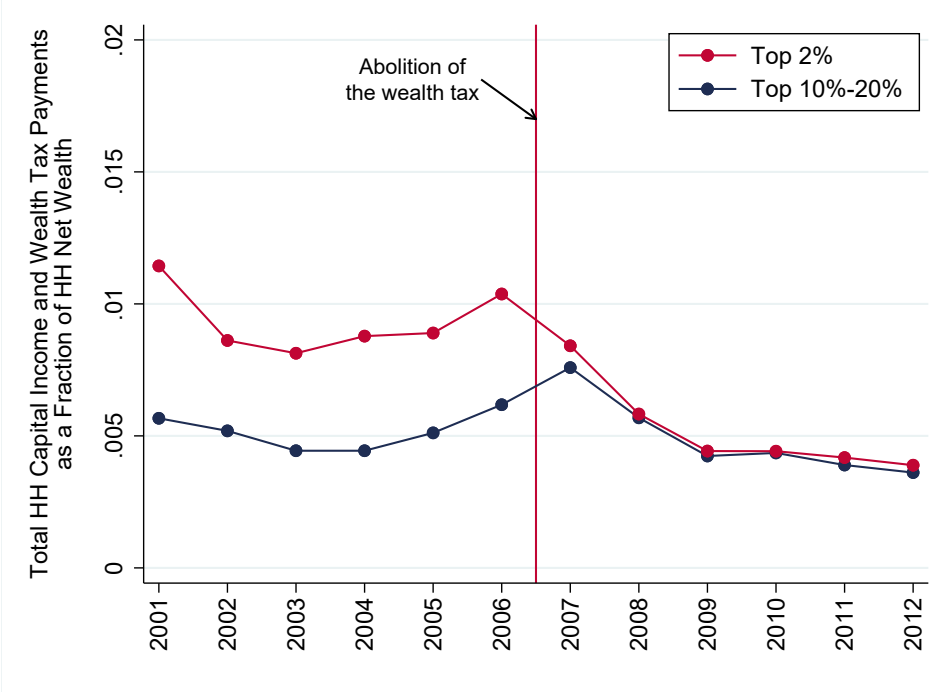
Notes: Panel A shows the evolution of the exemption threshold in the wealth tax for Sweden as a function of the household taxable wealth distribution. The red line refers to the wealth tax exemption threshold set for couples, while the dark blue line refers to the wealth tax exemption threshold set for 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 winsorised. 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 winsorised. 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 showed 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%. In addition to these reliefs, there was a wealth tax exemption for owners of closely-held corporations (*hovedaktionærnedslag*).

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

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

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 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).

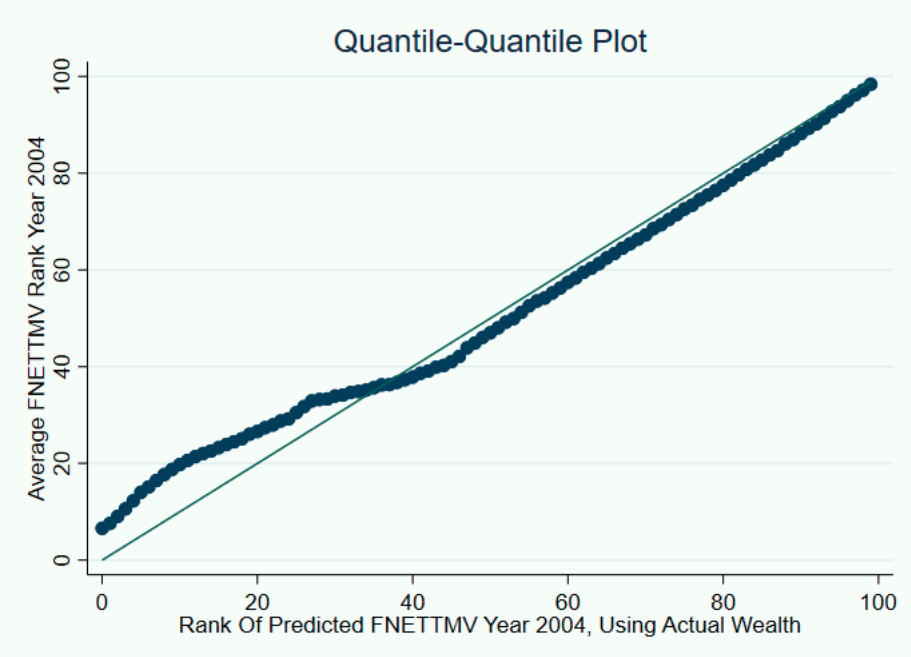
- Furthermore, from equation (3), we know that consumption can be retrieved as residual from all flows of earnings, taxes/transfers and inheritances and active savings and dissavings. In practice we measure throughout the period all active savings and dissavings in asset classes o and we also measure 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 showed 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 (outmigration 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. 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	96th-98th	98th
(percentile of wealth distribution)		
Tax ceiling	Y	Y
Exemptions (100%)	Business assets	Business assets
	Pension savings	Pension savings
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 environment for the wealth tax in Sweden and Denmark.

II.3. 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 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.[2] 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 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.

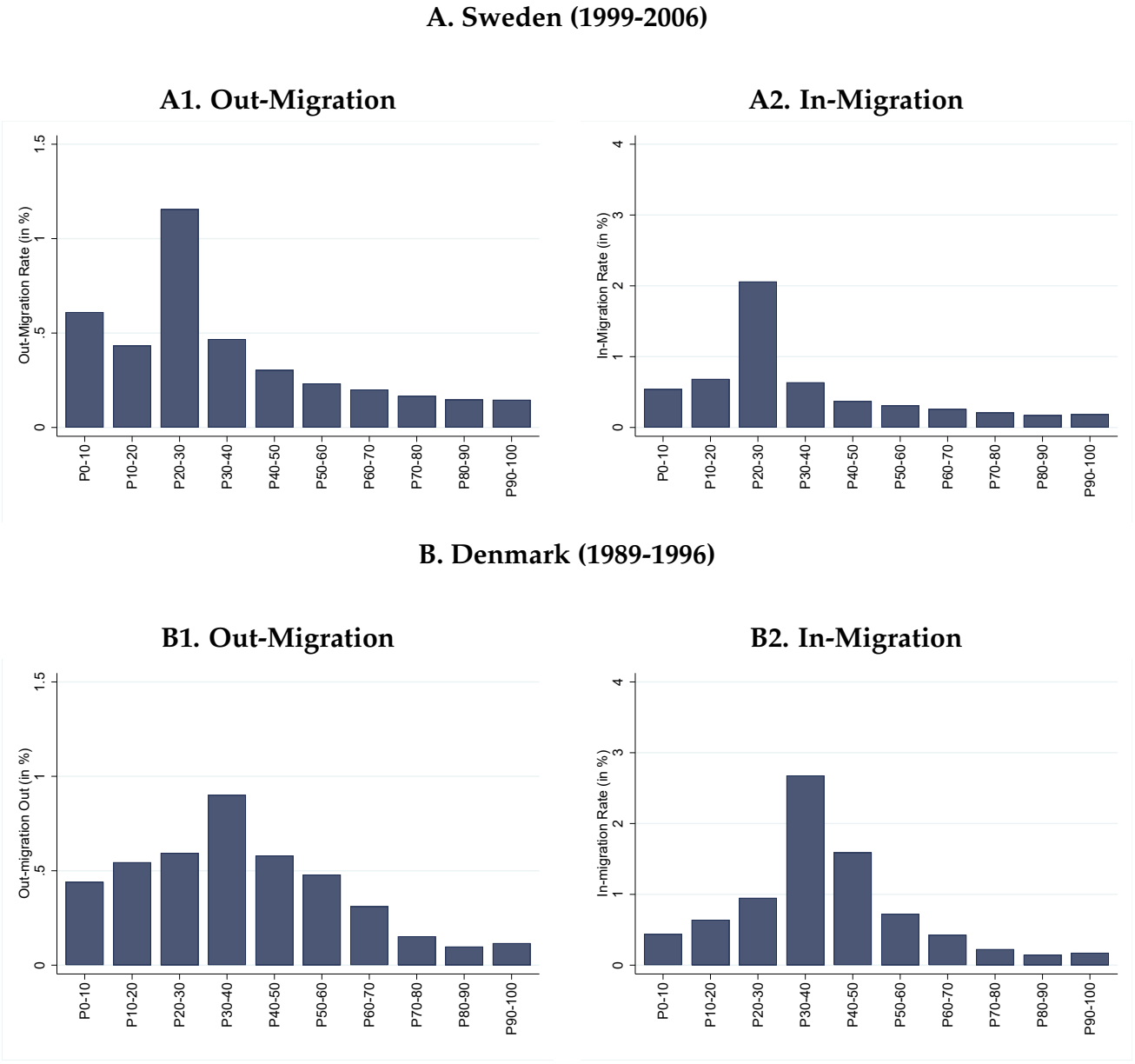
In a second step, we match the parent companies to the registry of closely-held businesses. When a parent company appears in the registry of closely-held businesses, we observe the individuals who own it. We define these individuals as the indirect owners of the subsidiary firm held by their closely-held 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,

[2]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).

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.

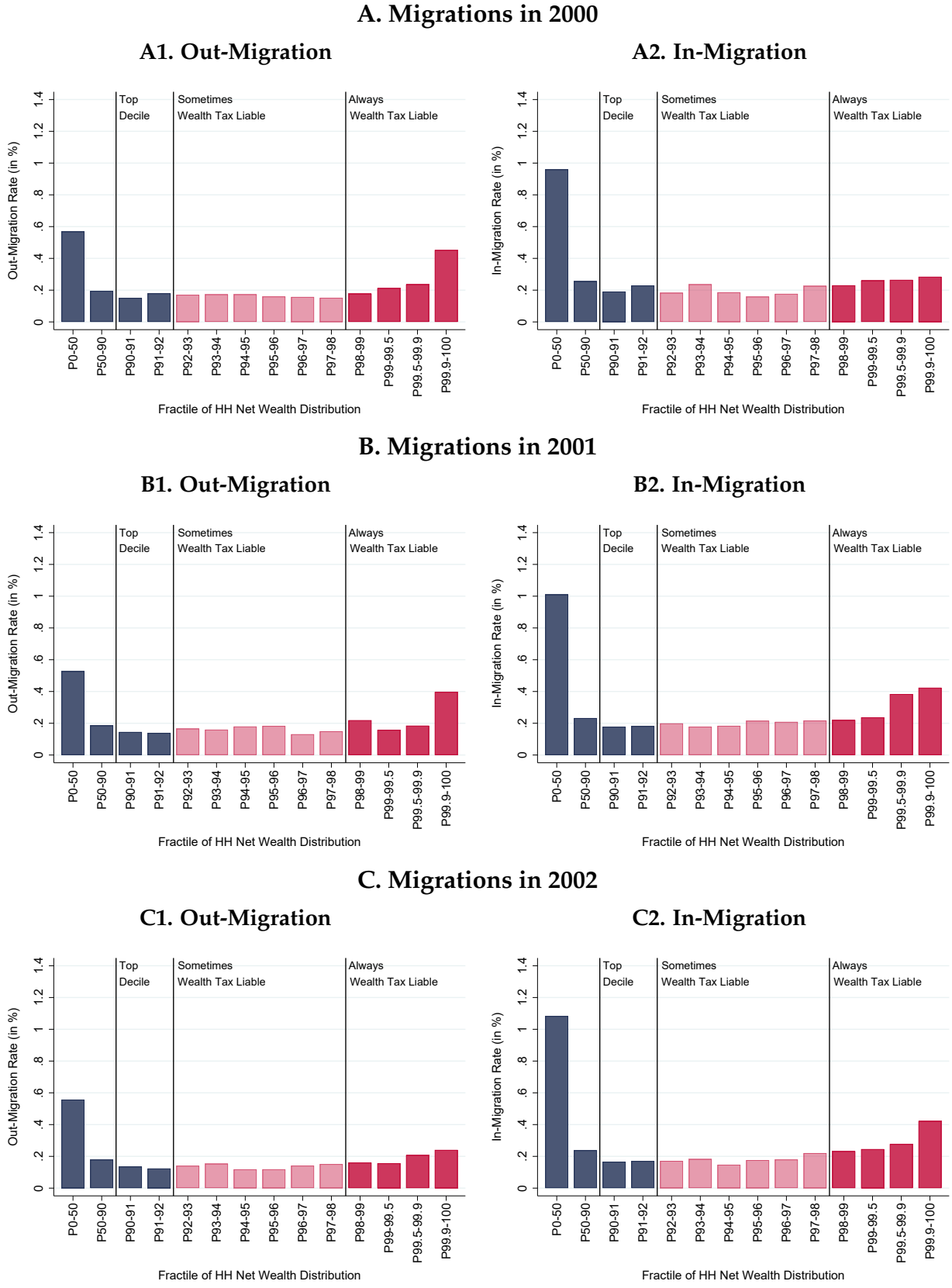
III. Additional Descriptive Evidence on Migration Patterns in Scandinavia

Figure III.1: **Wealth and International Migration Patterns in Scandinavia**



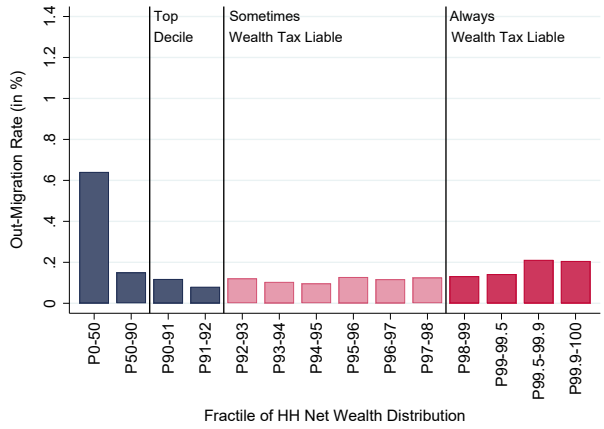
Notes: This figure shows in-migration and out-migration rates by decile of total net worth, in Sweden (Panel A) and Denmark (Panel B). We compute those statistics during the last years when the wealth tax was still in place in each country, which corresponds to the period 1999-2006 in Sweden and the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

Figure III.2: **Wealth and International Migration Patterns in Sweden Between 2000 and 2004**

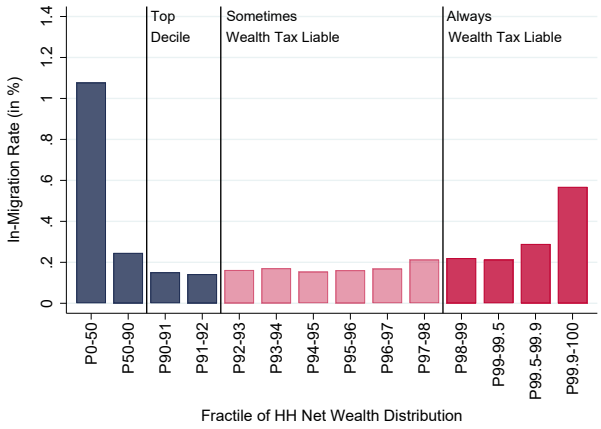


D. Migrations in 2003

D1. Out-Migration

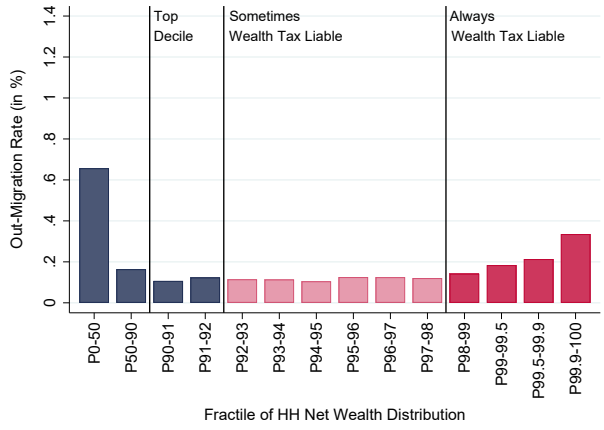


D2. In-Migration

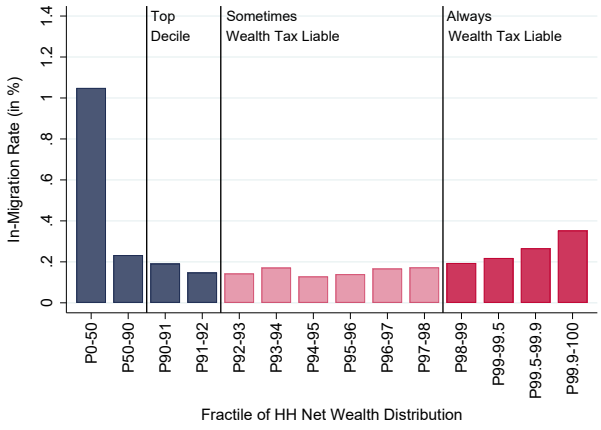


E. Migrations in 2004

E1. Out-Migration

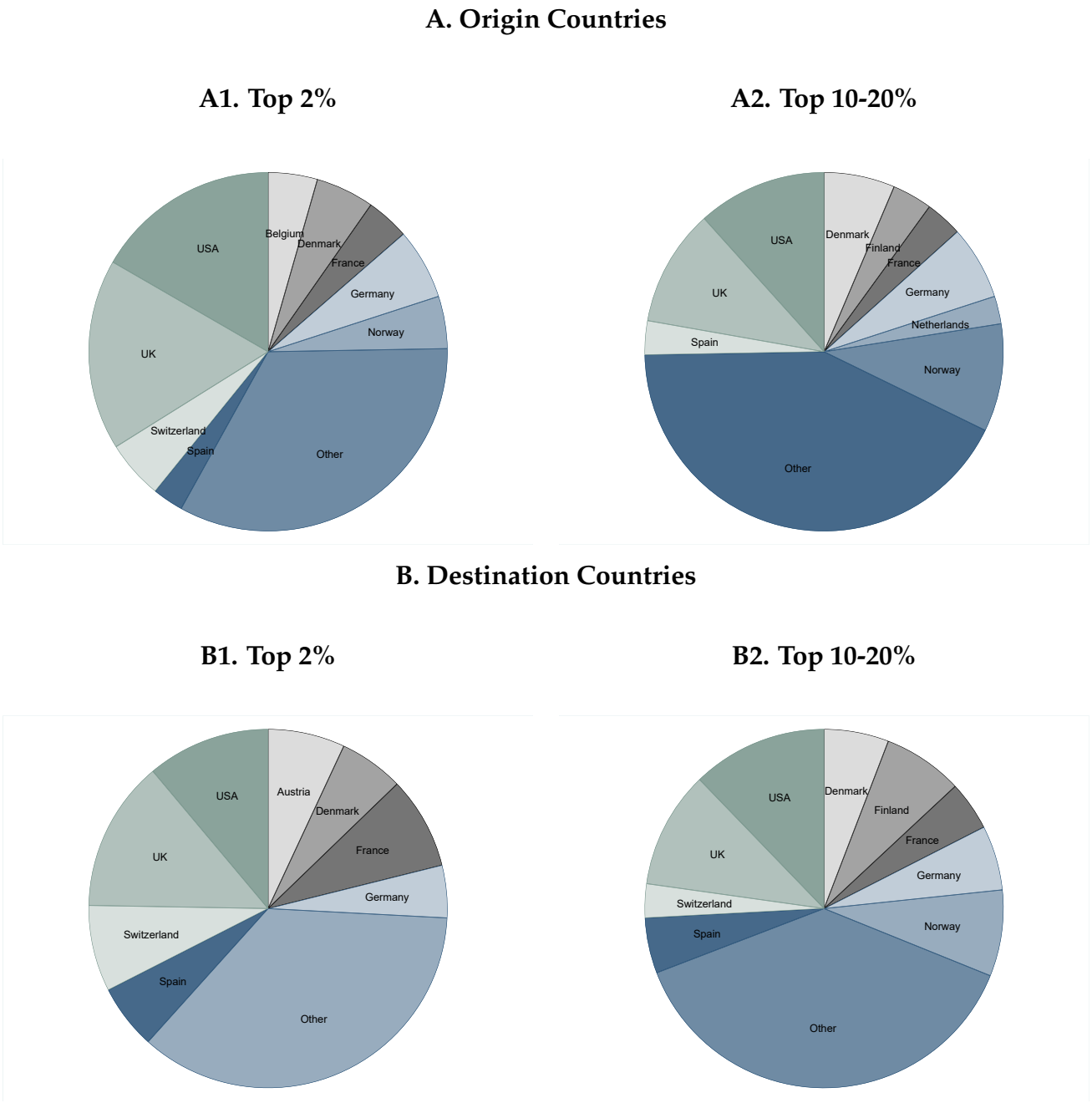


E2. In-Migration



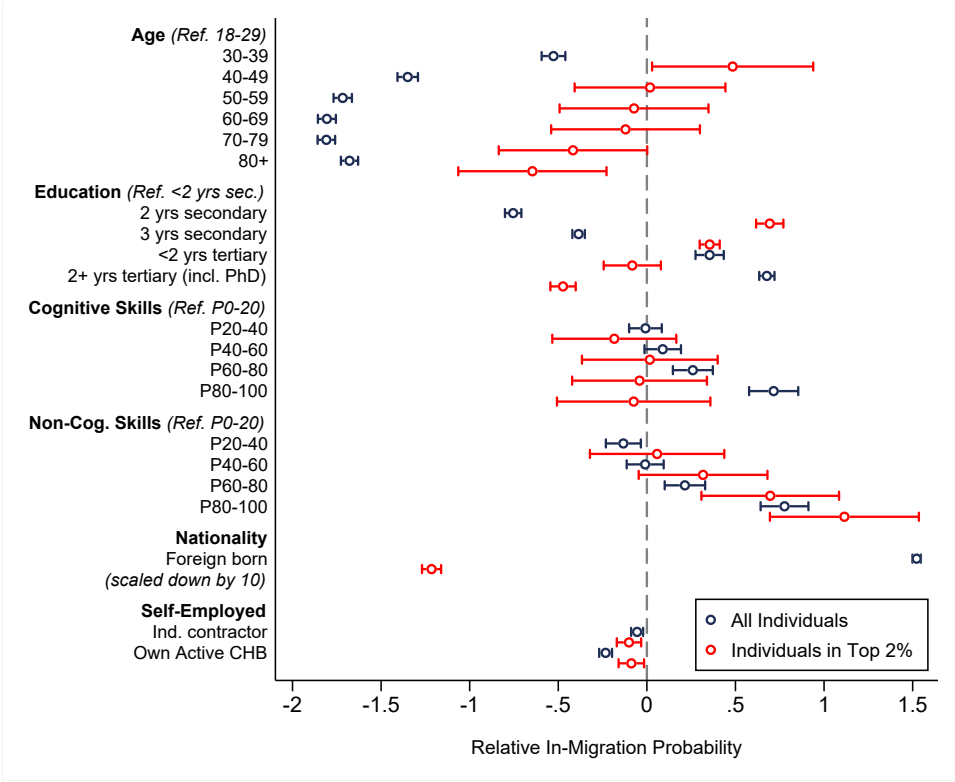
Notes: This figure shows in-migration and out-migration rates by level of total household net worth in Sweden for each year between 2000 and 2004. Each year, we rank individuals based on their household net worth, and show the out-migration and in-migration rates for each fractile of the household net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

Figure III.4: **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.5: **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 1 detailed in the text, while red coefficients are estimated on the interaction term of Equation 2. 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.

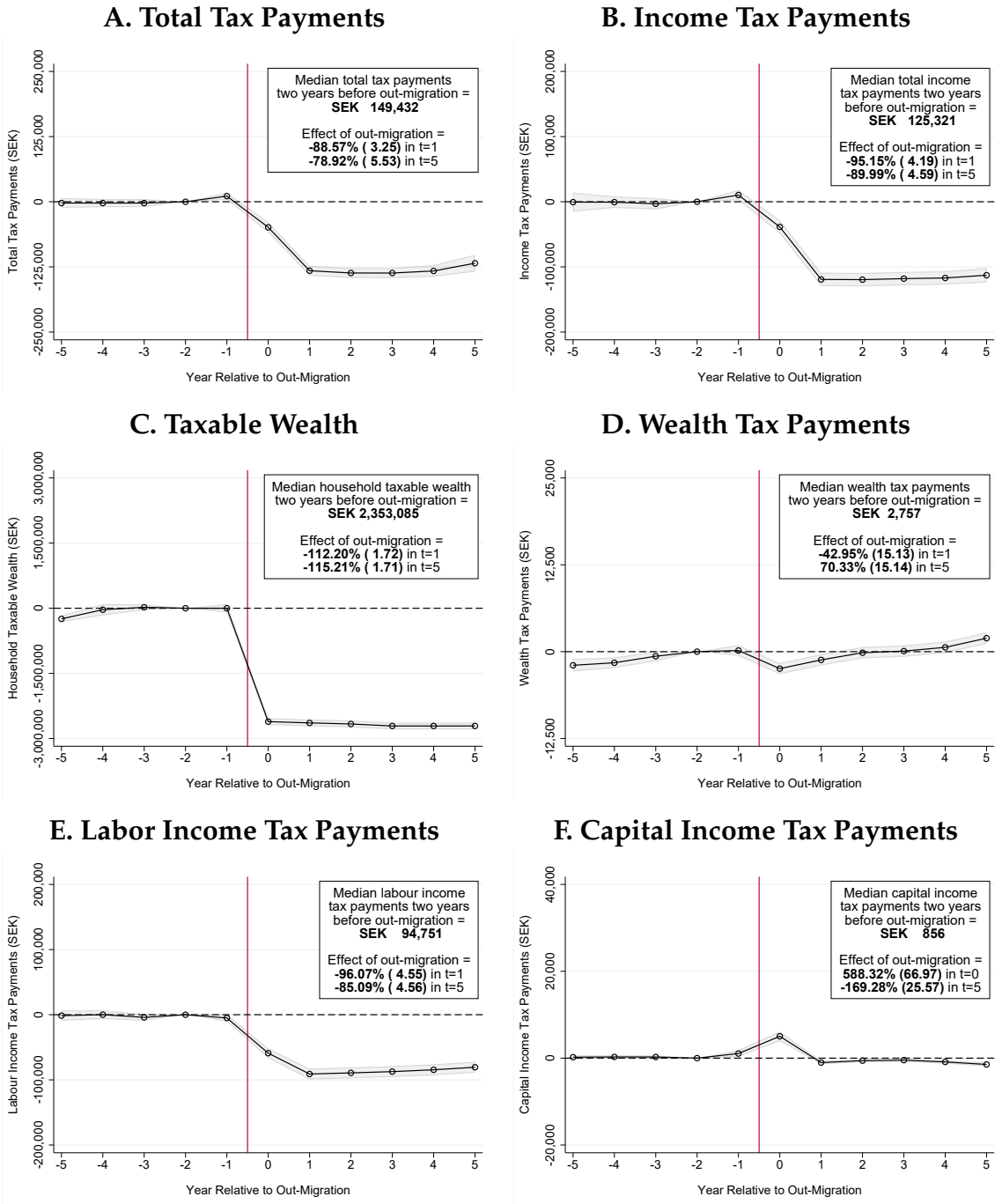
Table III.1: **Selection into Out-Migration from Sweden for Owners of Fast-Growing Active Closely-Held Businesses**

	All Individuals	Individuals in Top 2% of Net Wealth
Value Added Growth		
Year-on-Year, Top 25%	-20.21%	65.81%
	(6.52)	(12.46)
Past 3 Years, Top 25%	-20.08%	95.06%
	(7.06)	(13.73)
Past 5 Years, Top 25%	-31.75%	84.84%
	(6.61)	(13.11)
Year-on-Year, Top 50%	-25.45%	51.91%
	(4.09)	(7.85)
Past 3 Years, Top 50%	-28.41%	54.91%
	(4.10)	(7.94)
Past 5 Years, Top 50%	-28.79%	51.80%
	(4.16)	(8.13)
Employment Growth		
Year-on-Year, Top 25%	-30.13%	43.72%
	(6.32)	(12.48)
Year-on-Year, Top 50%	-27.29%	37.92%
	(4.98)	(9.82)

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 1 and Equation 2, 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 1. The second column displays the estimated coefficients for each ownership indicator interacted with a dummy for being in the top 2% of net wealth from Equation 2. 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.

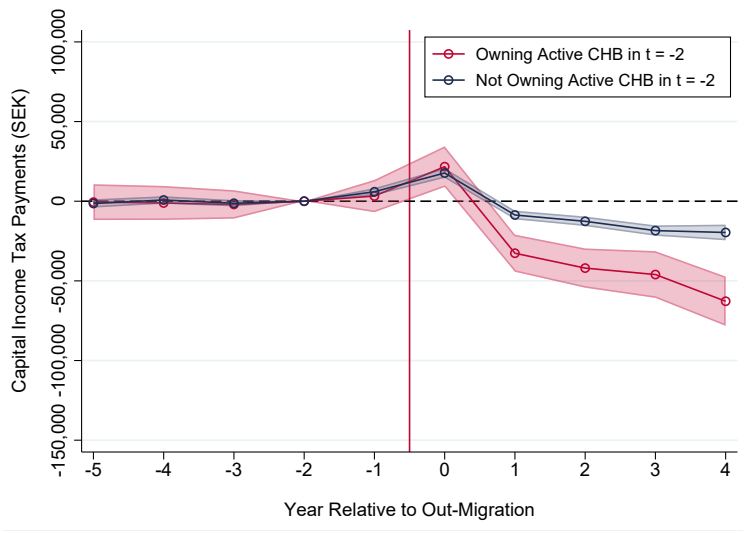
IV. Impact of Migration on Individual and Firm Outcomes: Additional Event Study Results

Figure IV.1: **Median Effects of Wealthy Out-Migration on Tax Payments, Top 2%**



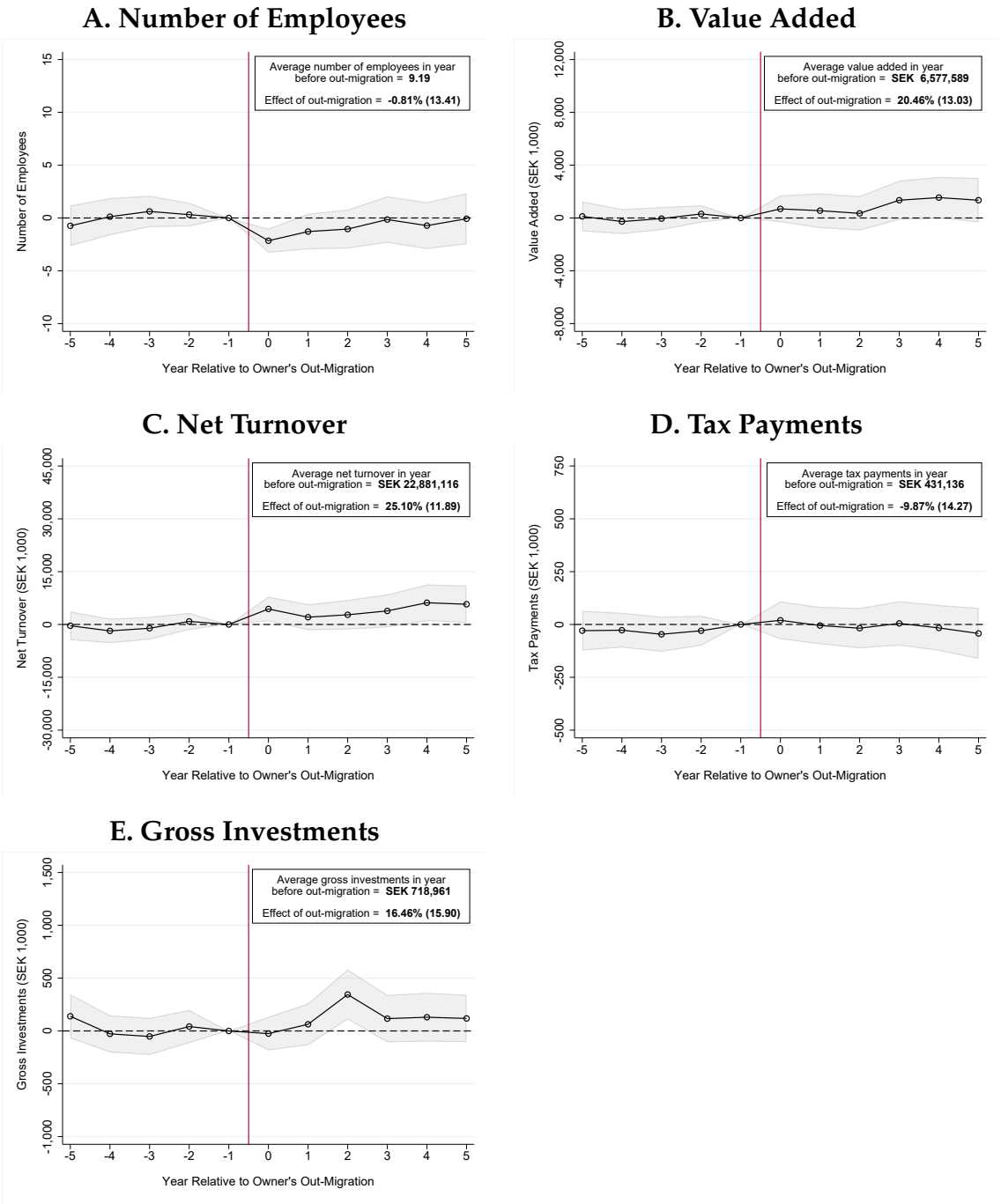
Notes: This figure describes the evolution of wealthy individuals' outcomes before and after they leave Sweden, compared to control wealthy individuals who do not move that same year. The sample includes individuals whose household net wealth was in the top 2% of the household net worth distribution in Sweden for at least one year before their true or placebo out-migration date. We plot the estimates β_j from Equation 3 estimated via median regressions.

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



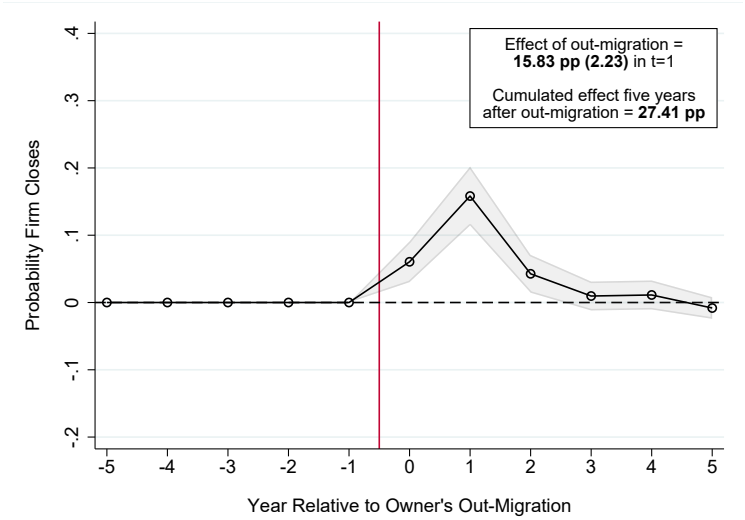
Notes: This figure describes the evolution of wealthy individuals' capital income tax payments before and after they leave Sweden, compared to control wealthy individuals who do not move that same year. We focus on individuals in the top 2% of the household net worth distribution in Sweden for at least one year before their true or placebo out-migration date. We plot the estimates β_j from Equation 3. 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.3: **Effects of Wealthy Out-Migration on Closely-Held Businesses Outcomes Conditional on Existence of Firm, Top 2%**



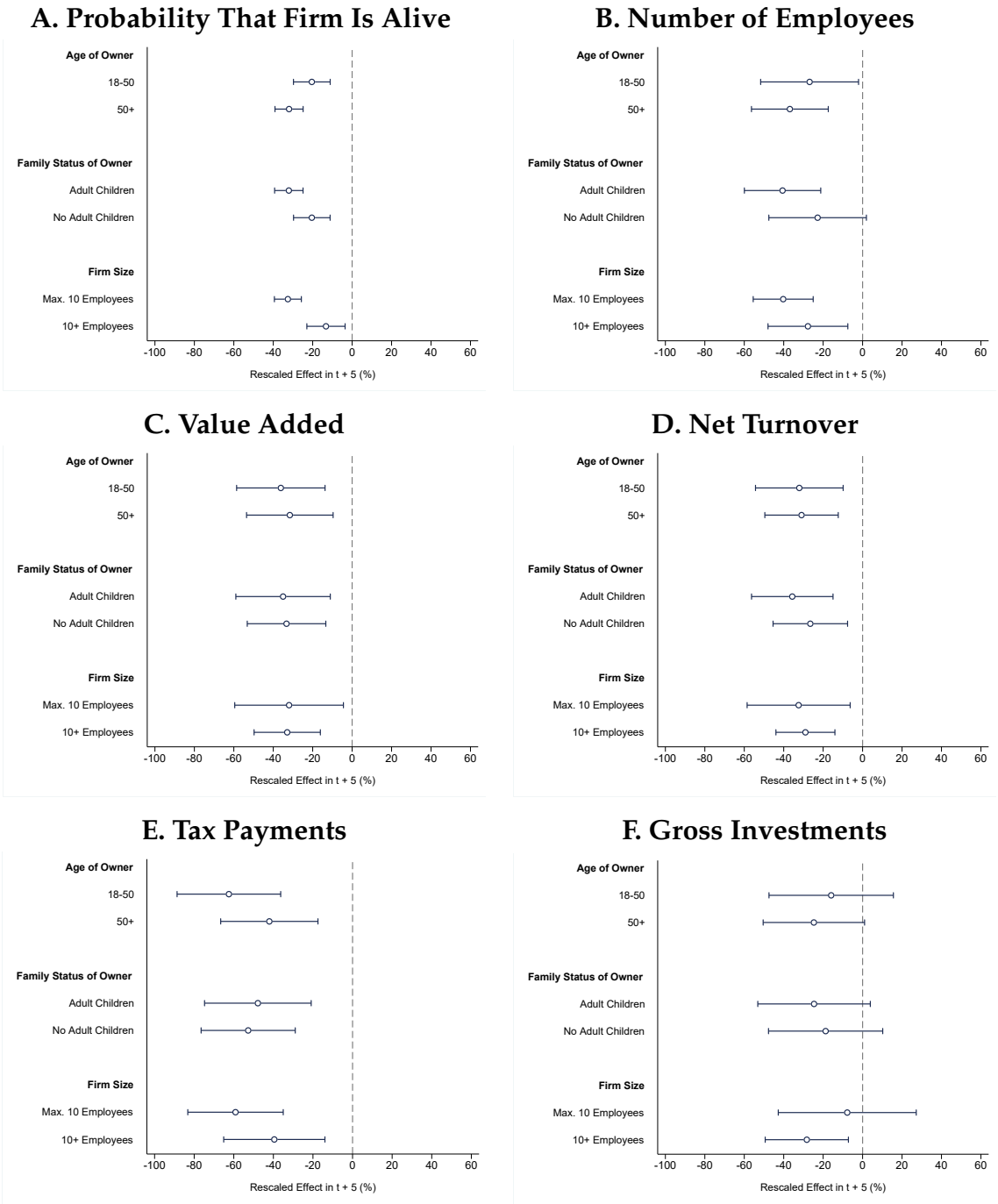
Notes: This figure shows the effects of wealthy owners' out-migration events on active firms controlled by individuals who were in the top 2% of the household net worth distribution in Sweden for at least one year before their migration event. We plot the estimates of β_j from Equation 4. We restrict the analysis to firms that do keep being active in Sweden throughout the period.

Figure IV.4: **Effect of Wealthy Out-Migration on Closure of Closely-Held Businesses, Top 2%**



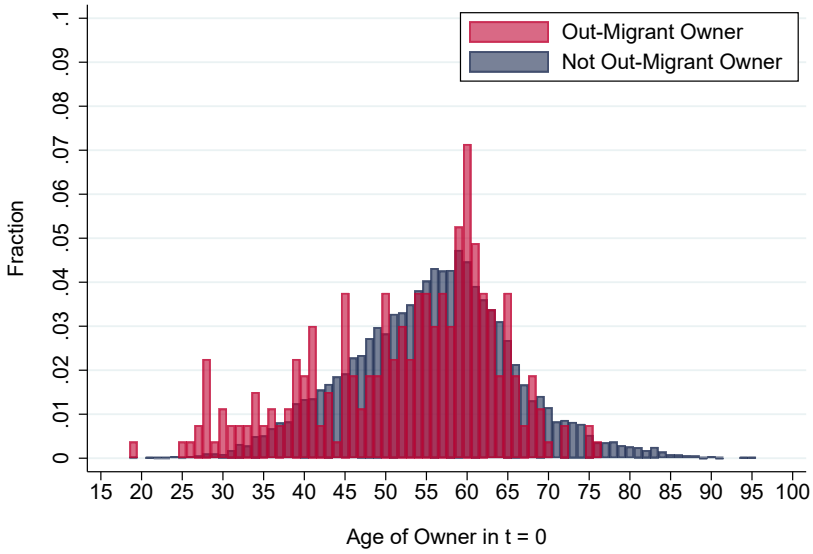
Notes: This figure shows the effects of wealthy owners' out-migration events on the probability that the firm they control before the out-migration event closes. The sample includes firms that in $t = -1$ were active closely-held businesses owned by individuals whose real or placebo out-migration happened 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 4. More details are provided in the main text.

Figure IV.5: **Effects of Wealthy Out-Migration on Closely-Held Businesses Outcomes by Wealthy Owners' Characteristics**



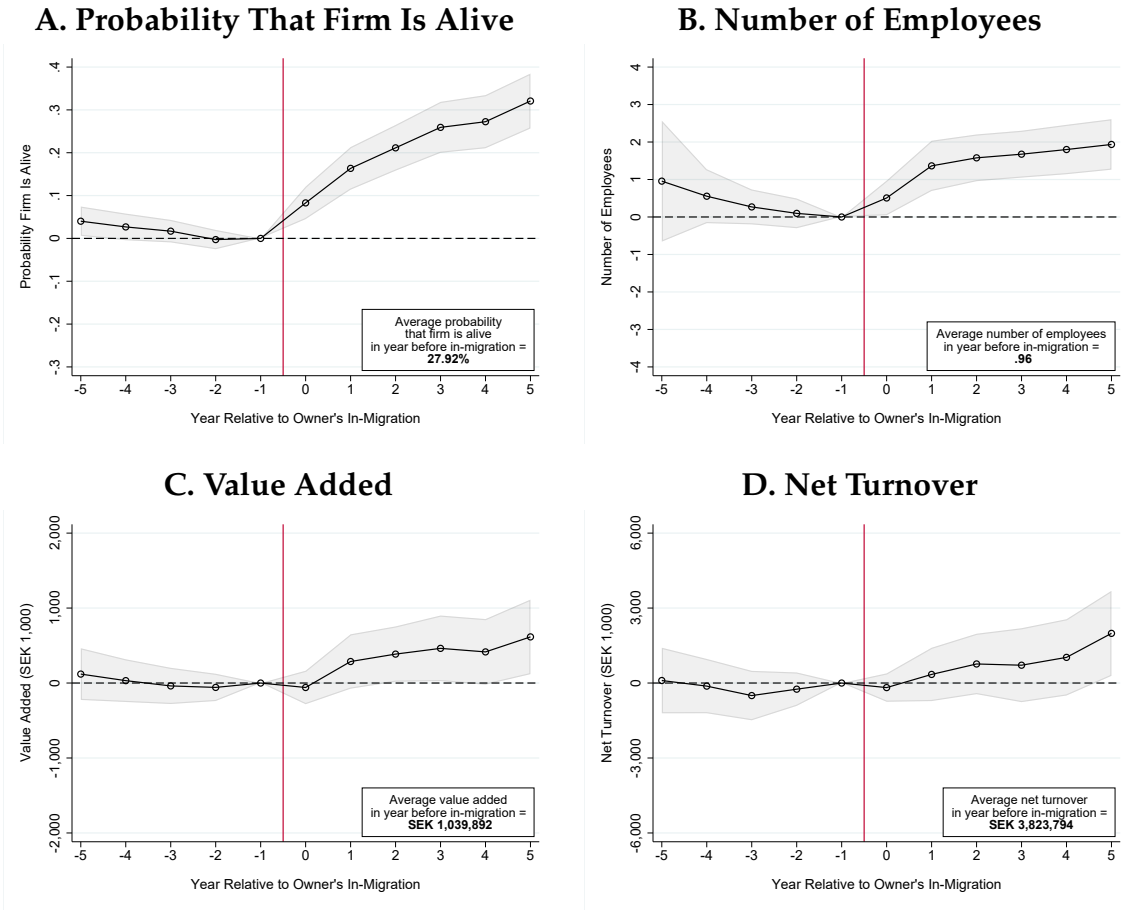
Notes: This figure shows the effects of wealthy owners' out-migration events on firm-level outcomes by wealthy owners' characteristics. Each coefficient and its confidence intervals refer to one separate regression. We focus on out-migration events occuring between 2001 and 2007. The sample includes 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. We plot the estimates of β_j and their confidence intervals estimated from Equation 4. 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 IV.6: **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 rank for at least one year before their observed or placebo out-migration event.

Figure IV.7: **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 winsorised the bottom 1% and top 5% of all outcomes except for the number of employees, for which we winsorised only the top 5%. We plot the estimates of β_j from the in-migration counterpart of Equation 4.

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

Sameple 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
Tax Payments (SEK 1,000)	-197.26 (36.18)	11.22 (16.96)	-4.66
Gross Investments (SEK 1,000)	-139.37 (64.17)	20.36 (41.38)	-1.56

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 4, 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 Unbalanced, Balanced, and Winsorised Characteristics of Closely-Held Businesses in the Event Study Samples**

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

	Unbalanced			Balanced			Winsorised		
Variable	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**
Nr. of Employees	10.65	3.00	50.69	8.75	2.00	46.12	5.67	2.00	9.41
Value Added	8,649	2,377	46,382	6,993	1,584	41,843	4,331	1,584	6,629
Net Turnover	30,310	6,461	237,824	24,505	4,143	214,172	14,087	4,143	23,397
Tax Payments	466	56	3,272	377	21	2,947	199	21	382
Gross Investments	1,280	70	8,835	1,026	21	7,927	430	21	916

Notes: This table reports descriptive statistics for the sample of closely-held businesses in our event-study sample. We study private firms controlled by individuals in the top 2% of net worth distribution in Sweden at least one year before their true or placebo first out-migration event. We also focus on firms that have at least one employee that is not the owner in the year before the (true or placebo) out-migration event. The unit of measure for value added, net turnover, tax payments, and gross investments is SEK 1,000. Employment is defined as total private employment at the firm minus employment of the owner. The unbalanced sample only includes firms in activity in Sweden. The balanced sample attributes zero outcomes to firms that have not yet started or have stopped their activity in Sweden. The winsorized sample is built after the balancing of outcomes. For the number of employees (than can never be lower than zero), we winsorize the top 5% of outcomes. For all other outcomes (that can take negative values), the top 5% and the bottom 1% were winsorised.

Table IV.3: **Descriptive Statistics on the Characteristics of Closely-Held Businesses in the Event Study Samples**

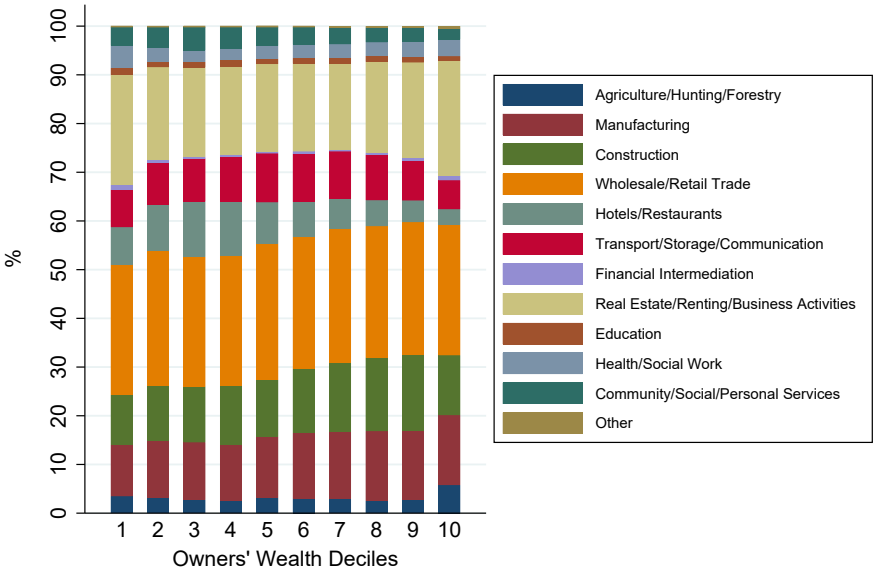
Sample of CHBs with owner in the top 2% of net worth

	Control CHBs				Treated CHBs					
Variable	**Mean**	**Median**	**Std. Dev.**	**Obs.**	**Mean**	**Median**	**Std. Dev.**	**Obs.**	**% of Swedish Aggregates**	**% of Active CHBs**
Nr. of Owners	1.86	1	2.23	187,047	4.24	2	14.39	2,424		
Nr. of Employees	10.55	3	50.12	187,093	18.47	2	83.42	2,426	0.02%	0.13%
Value Added	8,290	2,366	40,902	210,706	32,020	3,596	180,730	3,245	0.04%	0.19%
Net Turnover	29,381	6,425	233,544	210,706	90,627	10358	428,875	3,245	0.04%	0.21%
Tax Payments	448	56	3,143	210,706	1,664	88	7,915	3,245	0.06%	0.28%
Gross Investments	1,264	70	8,811	201,686	2,288	88	10,195	3,202	0.02%	0.09%

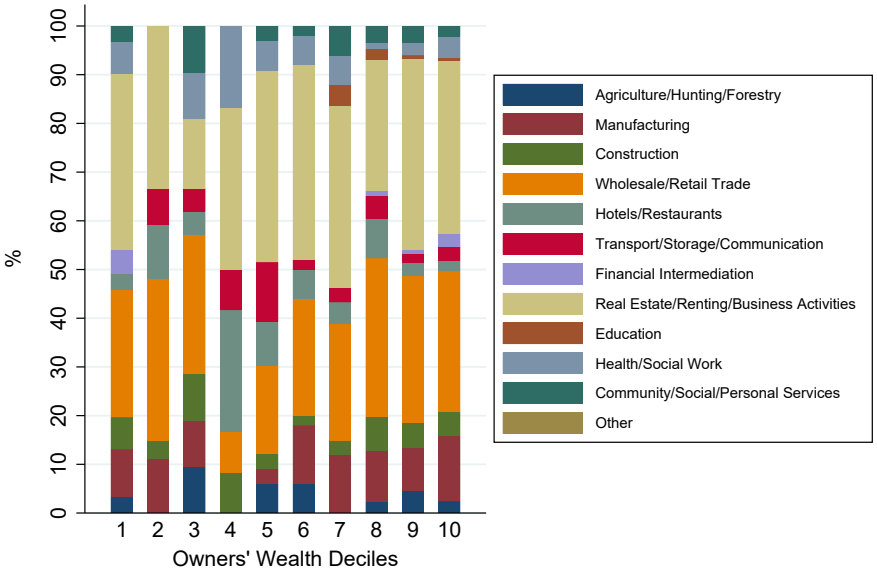
Notes: This table reports descriptive statistics for the sample of closely-held businesses in our event-study sample. We study closely-held businesses ("CHBs") controlled by individuals in the top 2% of net worth distribution in Sweden at least one year before their true or placebo first out-migration event. We also focus on firms that have at least one employee that is not the owner in the year before the (true or placebo) out-migration event. The unit of measure for value added, net turnover, tax payments, and gross investments is SEK 1,000. We winsorize each outcome to handle outliers. For the number of employees (than can never be lower than zero), we winsorize the top 5% of outcomes. For all other outcomes (that can take negative values), the top 5% and the bottom 1% were winsorised. Treated CHBs are closely-held businesses with at least one wealthy owner who left Sweden during the period 2001-2007. Control CHBs are closely-held businesses with wealthy owners who never left Sweden over the same period.

Figure IV.8: **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).

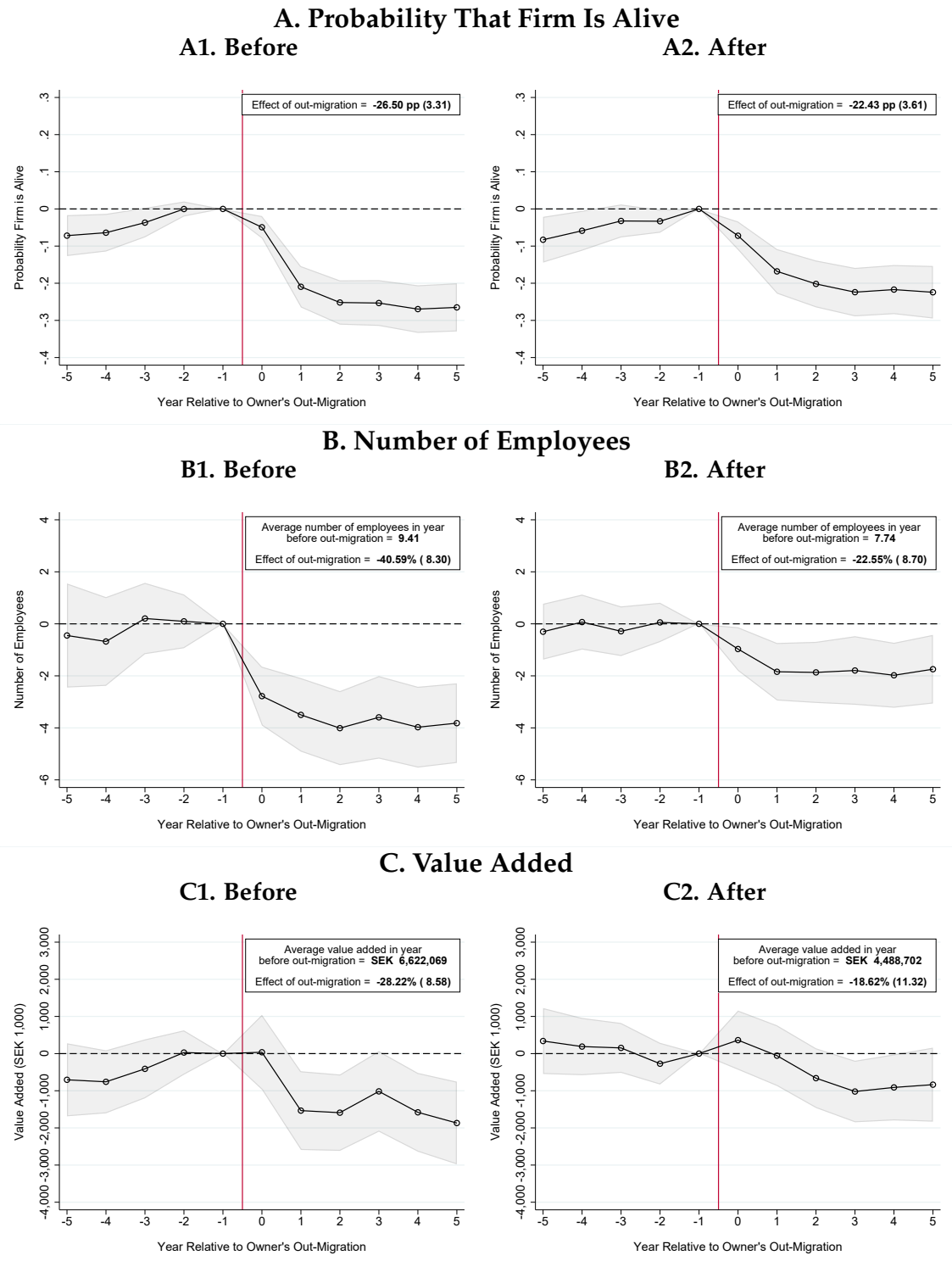
Table IV.4: **Effects of Wealthy Out-Migration on Closely-Held Business Outcomes in t+5 by Winsorization Method**

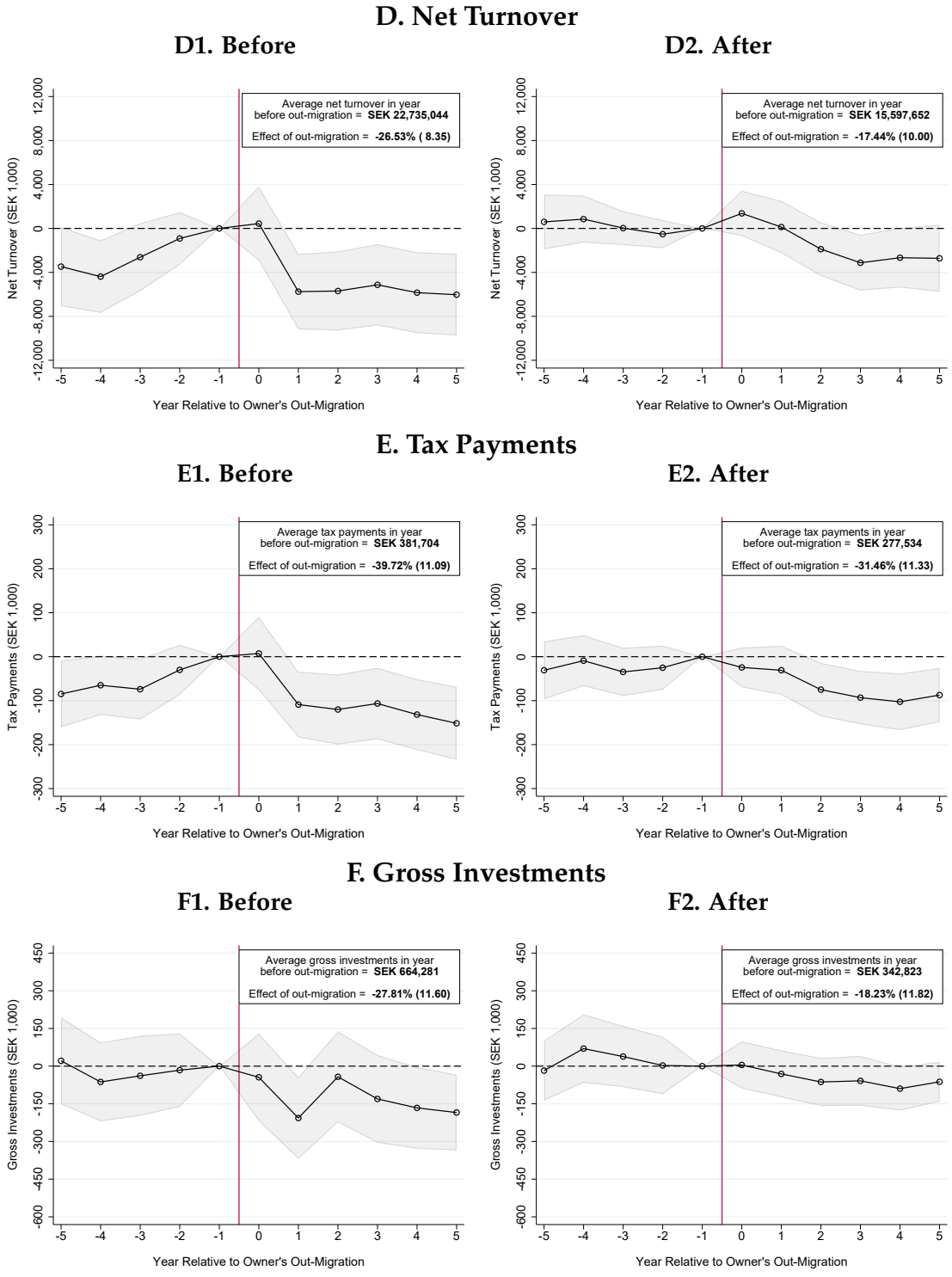
Sample of CHBs with owner in the top 2% of net worth

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: The table reports the estimated β_j in $t = 5$ from Equation 4 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.9: **Effects of Wealthy Out-Migration on Closely-Held Business Outcomes Before and After the Repeal of the Wealth Tax, Top 2%**





Notes: This figure reports β_j (and their confidence intervals) estimated from Equation 4, using out-migration events between 2001 and 2006 (before the repeal of the wealth tax, left panels) and using out-migrations between 2007 and 2013 (after the repeal of the wealth tax, right panels). We use predicted level of net wealth so that we can select firms controlled by wealthy owners even after 2007.

Table IV.5: **Test of Equality of Out-Migration Effects in Firm-Level Event Studies Estimated Before and After the Repeal of the Wealth Tax**

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

	Effects In Levels			**Percentage Effects**		
Outcome	**Out-Migration Years: [2001, 2006]** $t_{pre} = +5$	**[2007, 2013]** $t_{post} = +5$	**T-Statistic** $t_{pre} = t_{post}$	**Out-Migration Years: [2001, 2006]** $t_{pre} = +5$	**[2007, 2013]** $t_{post} = +5$	**T-Statistic** $t_{pre} = t_{post}$
Prob. Firm Is Alive (pp)	-26.50	-22.43	-0.83	-26.50%	-22.43%	-0.83
	(3.31)	(3.61)		(3.31)	(3.61)	
Number of Employees	-3.82	-1.74	-2.01	-40.59%	-22.55%	-1.50
	(0.78)	(0.67)		(8.30)	(8.70)	
Value Added (SEK 1,000)	-1,868.48	-835.90	-1.35	-28.22%	-18.62%	-0.68
	(568.21)	(508.10)		(8.58)	(11.32)	
Net Turnover (SEK 1,000)	-6,031.67	-2,720.04	-1.35	-26.53%	-17.44%	-0.70
	(1,898.03)	(1,559.84)		(8.35)	(10.00)	
Tax Payments (SEK 1,000)	-151.61	-87.31	-1.22	-39.72%	-31.46%	-0.52
	(42.35)	(31.44)		(11.09)	(11.33)	
Gross Investments (SEK 1,000)	-184.75	-62.49	-1.40	-27.81%	-18.23%	-0.58
	(77.08)	(40.52)		(11.60)	(11.82)	

Notes: This table compares the estimates of firm-level responses to out-migration events of wealthy entrepreneurs (in the top 2%) in Sweden, before and after the repeal of the wealth tax. For each firm-level outcome in each row, we present β_5 estimated from Equation 4, focusing on out-migration events between 2001 and 2006 (before the repeal of the wealth tax, Column (1) and Column (4)) and out-migration events between 2007 and 2013 (after the repeal of the wealth tax, Column (2) and Column (5)). Column (3) and Column (6) report the t-statistic of a test of equality for the estimates of out-migration effects before and after the repeal of the wealth tax.

V. Tracking Employees After Firm Closure

We start from our firm-level event study samples. We select the treated firms whose last recorded year of activity in the firm registries (year t) falls between the year before the owner's out-migration event date (inclusive) and 2016. Given that the firm-level data are available until 2017, we can only identify firm closures until 2016.

We then move to our individual-level panel dataset (LISA). We remove individuals who are direct owners of closely-held businesses and are employed in those same firms, so that we are left with pure firm employees.[3] Among the remaining workers, we keep those who were employed in one of the selected firms in its last year of activity (year t).

Having defined a sample of workers employed in a treated firm in its last year of activity (origin firm, year t), we then track their employment status in year $t+1$. An individual may be associated with a new firm (destination firm) or she may be unemployed. For each origin firm we compute its number of employees (excluding owners when possible) in year t. We also compute the number of employees in year $t+1$ in each destination firm coming from the same origin firm. For each origin-destination pair, we are thus able to compute the share of employees moving from the same origin firm in year t to the same destination firm in year $t+1$, relative to the total number of employees in the origin firm in year t. We repeat the same exercise using the control firms in our event study samples.

Table V.1 shows the number of treated and control firms in our firm-level event study samples with at least five employees in their last year of activity (year t). It also displays the share of these firms where at least 50% of the employees end up in the same firm in year $t+1$. The event study samples we consider are the one including only directly held closely-held businesses and the one including both directly and indirectly held businesses, with owners in the top 2% of household net wealth.

[3] We are only able to identify owners of closely-held businesses. Therefore, if a firm closes as a non-CHB we cannot eliminate its direct owners from the list of workers associated to the firm in its last year of activity.

Table V.1: **Tracking Employees After Firm Closure**

	Firms with $\geq$ 5 employees in last year	**% of firms such that $\geq$ 50% of employees move to the same firm**
Panel A: Sample with directly held CHBs		
Treated firms	22	54.6%
Control firms	690	58.0%
Panel A: Sample with directly and indirectly held firms		
Treated firms	137	67.9%
Control firms	2,264	65.6%

Notes: This table shows the number of firms closing with at least five employees by treatment group and event study sample. It also shows the share of these firms that send at least 50% of their employees to the same destination firm in the year after they stop their operations, measured as the first year in which they do not appear in the firm registries.

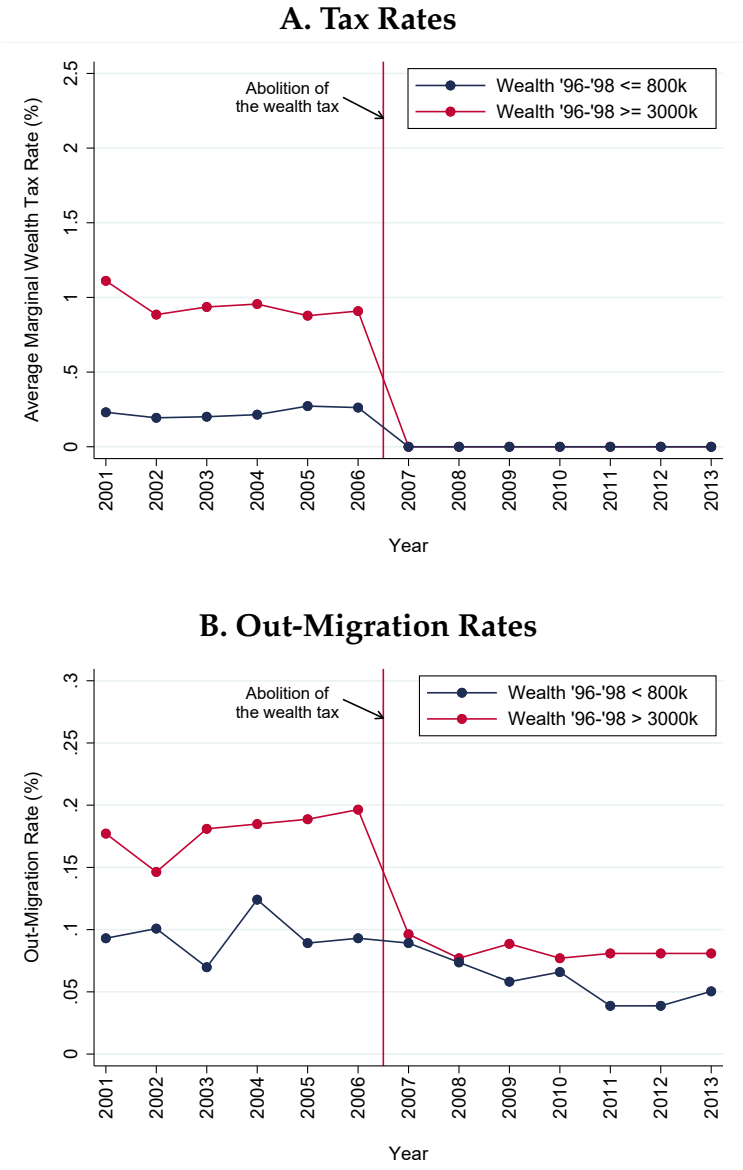
VI. Migration Elasticity Estimates

VI.1. Additional Evidence: Sweden

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).

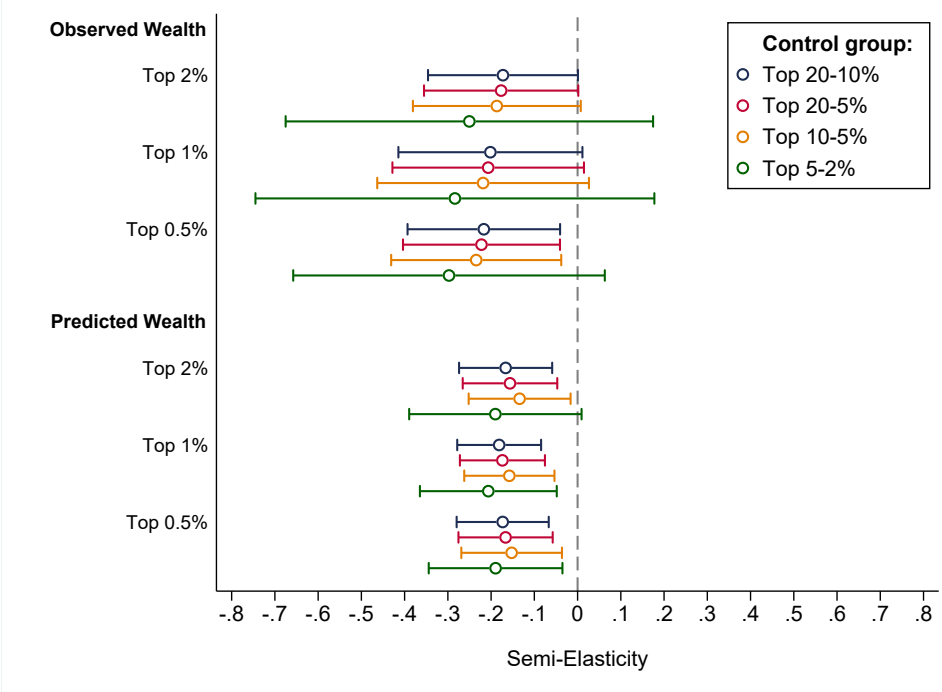
Figure VI.1 illustrates this empirical strategy: we use 1996-1998 wealth levels as the simplest predictors of wealth levels at the time of the reform. We define individuals with household taxable wealth above SEK 3,000K in 1996-1998 as high exposure to the reform, since they were already above the highest 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 10, 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.

Figure VI.1: **Out-Migration Rates by Predicted Exposure to the Wealth Tax Reform**



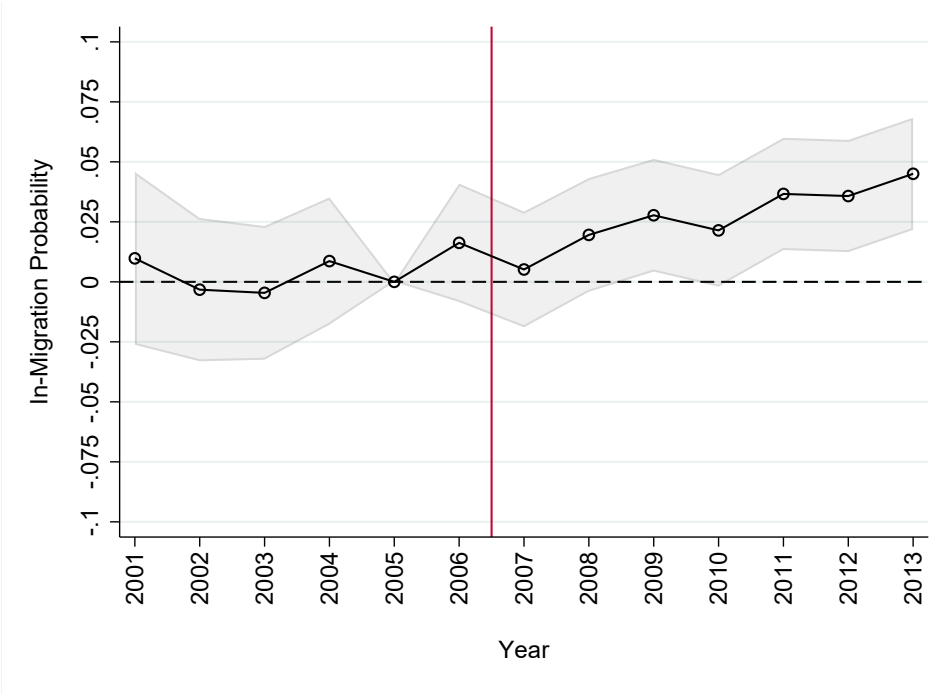
Notes: This figure shows the evolution of wealth tax rates (Panel A) and out-migration rates (Panel B) by exposure to the repeal of the wealth tax in Sweden, denoted by the vertical red line. We predict exposure to the reform by level of wealth in 1996-1998, comparing individuals above the highest exemption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

Figure VI.2: **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 6. 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, 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 showed in blue.

Figure VI.3: **Return Migration Rate Response To The Abolition Of The Wealth Tax: Difference-In-Difference Estimates**



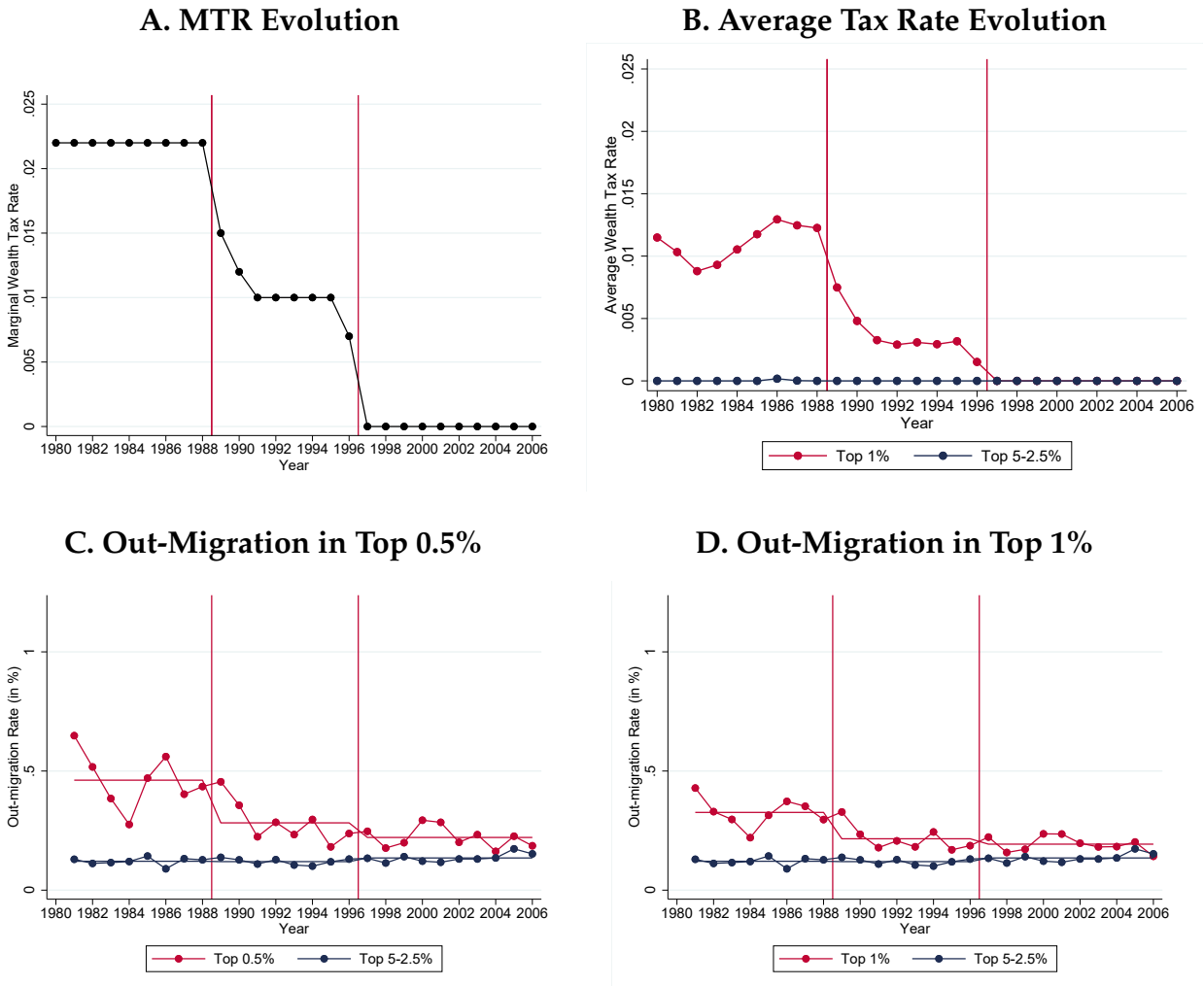
Notes: This figure shows the differential effects of the repeal of the wealth tax on the in-migration probability of treated (top 2% of the household net wealth distribution, subject to the wealth tax) and control (top 10-20%, not subject to the wealth tax) individuals. We define the treatment and control 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 5 and their 95 percent confidence intervals.

VI.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 showed 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 than in 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 showed in Panel A and B of Figure VI.4. The evolution of out-migration patterns around those changes for treated (red series) and control (blue series) individuals are showed 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. The graphical evidence in Figure VI.4 is somewhat noisier because the tax changes in Denmark were more gradual, and we have less wealthy treated by the reform in Denmark than in Sweden. 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 VI.4: **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.

VII. Quantification of Aggregate Implications

In section 6 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.

VII.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. 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 period t, the total population N_t is composed of individuals of different "age" k, that is, of individuals who have been in the wealthy population for a different number of periods.

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

where α_t^k is the net outmigration 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 $t-k$.

The total local stock of population of wealthy individuals is simply $N_t = \sum_{k=0}^{T} N_t^k$.

VII.2. Microfoundations of Migration Responses

We are interested in how wealth taxation affects migration flows 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. An individual's utility in location l depends on her consumption opportunities in l, as well as some idiosyncratic location taste shock μ_l. Consumption opportunities are simply her net-of-tax labor income and net-of-tax wealth in location l.

$$\mathbb{U}_l = \underbrace{u((1-t_l)y_l + (1-\tau_l)W_l)}_{u_l} + \mu_l$$

where y_l and t_l are respectively earnings and taxes on earnings in location l; W is wealth, and τ is the effective tax rate on wealth. We assume that individuals receive independent idiosyncratic moving shocks $\Delta\mu = \mu_S - \mu_O$, taken from the same distribution $F(.)$ every period. These shocks generate constant flows in and out of Sweden: e.g. individuals who experience a shock $\Delta\mu < \Delta u = u_O - u_S$ 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 below which individuals decide to leave Sweden.

VII.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 ($B_t = B, \ \forall t$). Net outmigration flow rates are also constant over time ($\alpha_t^k = \alpha_{t-1}^k, \ \forall k, t$).

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

$$N = B \cdot \sum_{k=0}^{T} \prod_{j=0}^{k} (1 - \alpha^{k-j}) = B \cdot \sum_{k=0}^{T} S(k) = B \cdot D \tag{6}$$

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

The percentage impact on the population stock N of a change in the net-of-tax rate on wealth in Sweden $d(1-\tau)$ is:

$$\frac{dN/N}{d(1-\tau)} = \frac{B}{N} \cdot \sum_{k=0}^{T} \frac{\partial(\prod_{j=0}^{k}(1-\alpha^{k-j}))}{\partial(1-\tau)} \tag{7}$$

This impact can be easily decomposed into the effect of the tax change of the population at each age k:

$$
\begin{aligned}
\text{Percentage effect on population of age 0} \quad & \frac{dN^0/N^0}{d(1-\tau)} &&= \quad 0 \\
\text{Percentage effect on population of age 1} \quad & \frac{dN^1/N^1}{d(1-\tau)} &&= \quad -\frac{\partial \alpha^1}{\partial(1-\tau)} \cdot \frac{1}{1-\alpha^1} = \varepsilon^1 \\
\text{Percentage effect on population of age 2} \quad & \frac{dN^2/N^2}{d(1-\tau)} &&= \quad \varepsilon^1 + \varepsilon^2 \\
& \dots \\
\text{Percentage effect on population of age k} \quad & \frac{dN^k/N^k}{d(1-\tau)} &&= \quad \sum_{j=1}^{k} \varepsilon^j
\end{aligned}
$$

This allows us to rewrite equation (7) as:

$$
\frac{dN/N}{d(1-\tau)} = \sum_{k=0}^{T} \frac{S(k)}{D} \cdot \sum_{j=1}^{k} \varepsilon^j \tag{8}
$$

Discussion Equation (8) 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 ε^1, the percentage effect on the population of age 2 is $\varepsilon^1 + \varepsilon^2$, etc. This is quite intuitive: if individuals leave in t, 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 outmigration 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 (8) is that it connects easily to our data and empirical setting. It provides a direct estimate of the percentage effect of change in the wealth tax on the population stock based solely on our estimates of the effects of a change in the tax rate on the net outmigration flows ε^k at age k, and on measures of D and S which we can observe in the data. Note that because τ and α^k are small, $\varepsilon^k \approx \frac{d\alpha^k}{dln(1-\tau)}$. In other words, ε^k corresponds to our estimates of the semi-elasticity of the net-migration rate with respect to the net-of-tax rate on wealth from specification (6) in section 6 of the paper.

Simplified Formula and Intuition Results in section 6 of the paper suggest that semi-elasticities ε^k are approximately constant. That is, the semi-elasticity 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:

$$\frac{dN/N}{d(1-\tau)} \approx \varepsilon \cdot \frac{(T+1)}{2} \tag{9}$$

This formula can be implemented simply with our estimate of the average semi-elasticity of net outmigration flows 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's 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 (9) has a simple interpretation: to get an estimate of the effect on the population stock N, 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 outmigration flows), we take the sum of the out-migration and in-migration semi-elasticities from Figure 12 in the main text.[4]

Simplified Formula: Calibration 1 *The estimated semi-elasticity of the stock of the population*

[4]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.

of wealthy individuals with respect to the net-of-tax rate on wealth is:

$$\frac{dN/N}{d(1-\tau)} = 1.76 \ \ (0.49)$$

This is the percentage change in the size of the wealthy population when the effective tax rate on wealth is increased by 1 percentage point. Because τ is very small, this semi-elasticity can also be interpreted as the elasticity of the stock of the population with respect to the net-of-tax rate on wealth (i.e. $d(1-\tau) \approx d(1-\tau)/(1-\tau)$).

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 1.76 % 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 .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 (8) using estimates of semi-elasticity of migration flows by age. We find an estimated semi-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, outmigration 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:

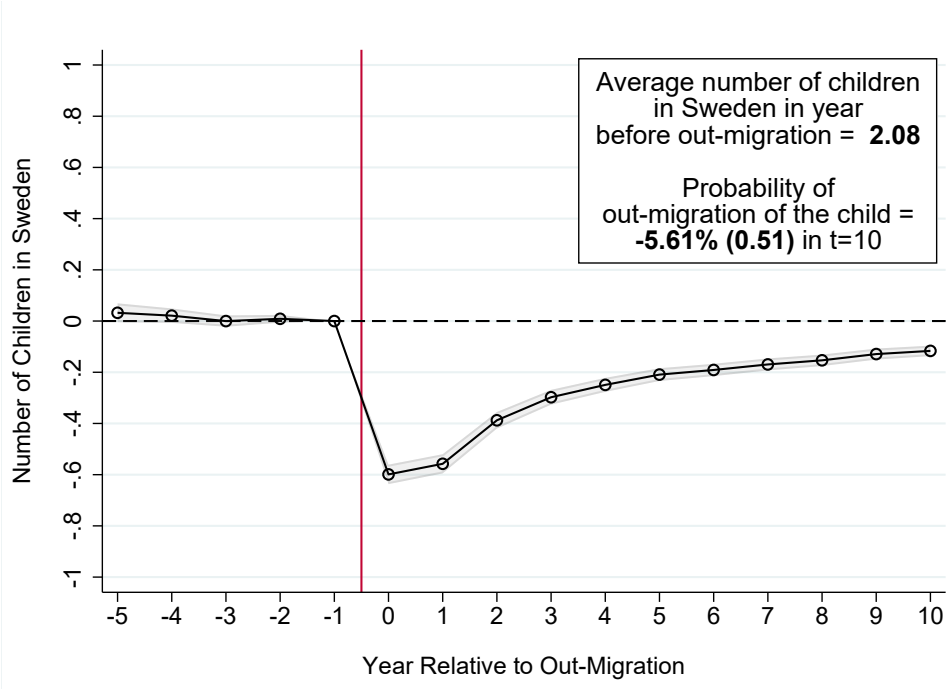
$$\frac{dN/N}{d(1-\tau)} = \frac{\partial B}{\partial(1-\tau)} \cdot \frac{1}{B} + \sum_{k=0}^{T} \frac{S(k)}{D} \cdot \sum_{j=1}^{k} \varepsilon^j \tag{10}$$

where, compared to our baseline formula, we now need to add the semi-elasticity of the birth rate into the wealthy population with respect to the net-of-tax rate on wealth.

The size of this semi-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 (10) 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 VII.1 below, we report the results of an event study similar to specification (3) 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 outmigration 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 VII.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 3, 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 $\frac{\partial B}{\partial(1-\tau)} \cdot \frac{1}{B} = .001$. And calibrating formula (10), we find an estimated semi-elasticity of population size of 1.86 (.51), 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. Alstadsæter et al. (2019)). 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 $(1-\tilde{\tau})$:

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

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 Alstadsæter et al. (2019). 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.[5] 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 13 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 1.9 and the lower bound elasticity is 1.83, 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.[6]

[5] 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.

[6] 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

Elasticity w.r.t Net-of-Tax Rate on Capital Income While our estimated semi-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:

$$\varepsilon_{N,1-t} = \varepsilon_{N,1-\tau} \cdot \frac{d\ln(1-\tau)}{d\ln(1-t)} \approx .049 \ \ (.013) \tag{11}$$

When appropriately rescaled, the implied migration elasticity is thus small. Panel B of Figure 13 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.

VII.4. Aggregate Tax Revenue Implications

How much constraints do these behavioral migration responses impose on tax policy? A natural way to answer this question is to measure the aggregate tax revenue implications of these migration responses when increasing the tax rate on wealth, accounting for all tax externalities generated by the wealthy when they migrate. When focusing solely on

one's wealth rather than residence to avoid the burden of taxation.

these extensive migration responses, the impact of a change in wealth tax τ on total tax revenues ($dGBC$) is the sum of a mechanical effect $N \cdot W \cdot d\tau$, and of a behavioral effect $dN \cdot \mathbb{T}$, where N is the size of the wealthy population, W is average wealth among the wealthy and $\mathbb{T} = \mathbf{t} + \tau W$ are total taxes paid by the wealthy, including all non-wealth taxes $\mathbf{t}$.

The overall distortions imposed by migration responses are simply determined by the ratio of the behavioral to mechanical effects of a change in the tax rate on wealth:

$$dGBC/d\tau = N.W(1 - \frac{dN/N}{d(1-\tau)}(\mathbf{t}/W + \tau))$$

To measure the behavioral revenue effect, we use the average $\tau = .006$ for the top 2% of wealthy individuals observed over the period 1999-2006 in Sweden. And we find that $\mathbf{t}/W = 13.1\%$.[7] Using our semi-elasticity estimate $\frac{dN/N}{d(1-\tau)} = 1.76$, this means that the behavioral revenue effect is equal to .24. In other words, for every additional Swedish Kr of tax revenues levied mechanically by raising the wealth tax rate by $d\tau$, only .24 Kr are lost due to behavioral migration responses. This clearly outlines that migration responses alone, even accounting for all tax externalities, are too small to suggest that the Swedish wealth tax rate was anywhere near the Laffer rate before 2007.

VII.5. Aggregate External Effects on Employment and Economic Activity

Our results suggest that the fiscal implications of migration responses to the wealth tax are very small. But in policy debates, the focus is often shifted to the negative externalities on aggregate employment, investment and business dynamism that may be exerted by outmigration of the very wealthy.

We now combine our estimates of migration responses to the wealth tax with our estimates of the persistent impact of migration on firms owned by the wealthy to quantify these aggregate external effects of wealth taxation.

For any aggregate outcome Y such as employment, investment or value-added, the migration-induced aggregate effect on Y of an increase in the wealth tax rate is simply given by:

[7] Note that we measure $\mathbf{t}$ as total non-wealth tax payments paid by the wealthy in Figure 5 plus the total change in corporate tax payments made by firms directly or indirectly owned following a migration event from Figure 7.

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

where Y^w is the average outcome (i.e. employment, investement, etc.) generated by a wealthy entrepreneur. In other words, we can measure these external effects by simply combining our estimate of the semi-elasticity of population w.r.t. net-of-tax rate on wealth, with our event-study estimates of the impact of migration on firms outcomes from Figure 8, and multiplying this by the share of aggregate Y controlled by the very wealthy that we have measured in Table 1.

A couple of points are worth noting about our calibration. First, we use the total net migration elasticity (which combines the effects on out-migration and in-migration flows) for $\frac{\partial N}{\partial(1-\tau)} \cdot \frac{1}{N}$ For the migration impact $\frac{\partial Y^w}{\partial N} \cdot \frac{1}{Y^w}$ we use our estimates of the ES for outmigration from Figure 8. In practice, this calibration therefore assumes perfect symmetry in the effect of in- and out-migration on firms' outcomes. We have however noticed in Figure 8 that in-migration effects are somewhat smaller than out-migration effects for a few outcomes like investment or value-added. This in turn means that our estimate of the aggregate external effect is an upper-bound on the true effect.

Second, we note that for the migration impact $\frac{\partial Y^w}{\partial N} \cdot \frac{1}{Y^w}$, we take estimates controlling for buyouts to account for reallocation effects.

Results are reported in Figure 14. To understand these estimates, we provide as an example the decomposition of the aggregate effect for employment. We have estimated that a 1 percentage point increase in the effective average tax rate on wealth causally decreases the steady-state stock of wealthy entrepreneurs by 1.76%. These entrepreneurs control (directly or indirectly) firms that represent 9.2% of total employment in the Swedish economy (Table 1 panel C). And their migration causes a 18.7% reduction in employment in the firms they own directly or indirectly (Figure 8 panel B). As a consequence, we estimate that a one percentage point increase in the effective average tax rate on wealth causally decreases aggregate employment by $(9.2\% * 18.7\% * 1.76\%) = .030\%$ through tax-induced migration of the wealthy.

The main insight from Figure 14 is that the migration-induced effects of wealth taxes on overall economic activity are extremely limited. A one p.p. increase in the effective tax rate on wealth of the top 2% of wealthiest taxpayers is found to decrease total employment by .03%, total investment by .04% and total value-added by only .09%. This is despite the fact that wealthy entrepreneurs account for a substantial share of overall eco-

nomic activity through the firms that they control directly and indirectly. The main reason for such small effects lies in the very small migration elasticity, which itself is largely due to the fact that migration flows at the top of the wealth distribution are actually remarkably tiny.

We should also stress that our results are calibrated based on a 1 percentage point change in the effective tax rate on wealth. Such a variation is sizeable: it is twice the size of the variation induced by the abolition of the Swedish wealth tax for the top 2% of wealthiest taxpayers. This implies that the long run effects on economic activity of the abolition of the Swedish wealth tax are twice smaller than the ones reported in Figure 14. Another way to present these effects is to compute the implied fiscal cost per job "created" by migration responses induced by the abolition of the wealth tax in Sweden. We find that each job created cost about SEK 2,341,000 Swedish Kr. of tax revenues (i.e. approximately 230,000 euros of public funds per job created), which is ten years of the average wage in Sweden at the time.

Our quantification procedure relies on a few assumptions that are worth highlighting and discussing. First, we have used for the migration elasticity our baseline elasticity for the whole population of wealthy taxpayers. In practice, the elasticity that matters is the migration elasticity for the population of entrepreneurs. Fortunately, our estimates from Figure 12 confirm that the semi-elasticity estimates for in- and out-migration flows for firm owners are almost identical to the estimates for the wealthy population as a whole. While the semi-elasticities of migration flows are similar, the semi-elasticities on the stock may differ if the average birth and death rates of entrepreneurs at the top of the wealth distribution differs from the rest of the wealthy population. The data indeed suggests that the hazard rate out of the population of wealthy is larger for entrepreneurs, which may reflect the fact that they engage in riskier investments than the rest of the wealthy population. This in turn suggest that our estimate is an upper-bound on the migration elasticity of entrepreneurs.

Our two-step procedure, which separately identifies migration elastiticities and migration impacts before combining these two set of estimates, also relies on the implicit assumption of no treatment effect heterogeneity in migration impacts $\partial Y^w/\partial N$. But the LATE estimate for migration impacts that we identify in our event study design might not be the same as the LATE on the individuals who are at the margin of migration with respect to a change in the wealth tax rate (and decide to migrate when the wealth tax is abolished). A worry would therefore be that the repeal of the wealth tax changed the way out-migration events and the out-migrants' firms outcomes co-vary. This could occur, for instance, if the reform changed migration decisions specifically for entrepreneurs

who would exhibit different behavior (in terms of their firms outcomes) when they move. Alternatively, the repeal of the wealth tax could have changed the incentives for wealthy out-migrants to close their firms in Sweden, or to change their firms' activity. To tackle those issues, we investigate potential heterogeneities in the spillover effects of wealthy out-migration events, before and after the repeal of the wealth tax. To do this, we run our baseline analysis separately on all out-migration events that occurred before and after the reform. We show the results in Appendix Table IV.5. The effects of wealthy owners' out-migration are quantitatively and qualitatively similar before and after the repeal of the wealth tax. This means that while the reform affected the rate at which the wealthy out-migrated from Sweden, it did not change the way their out migration decisions interacted with their firms' economic activity in Sweden. This also means that wealth tax-induced migration events (that existed before 2007 but not after) do not have a differential impact on firms' activity compared to other migration events.