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Behavioral Responses to Wealth Taxes: Evidence from Sweden†

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This paper provides an empirical assessment of an annual wealth tax. Using Swedish administrative data, I estimate net-of-tax-rate elasticities of taxable wealth in the range [0.09, 0.27]. Cross-checking self-reported assets against asset data unavailable to the tax agency reveals that around a third of the elasticity estimates are due to underreporting of asset values. Difference-in-difference designs further suggest that the responses reflect evasion and avoidance rather than changes in saving. (*JEL* H24, H26, H31)

In the aftermath of the global financial crisis, economic inequality has assumed center stage in the policy debate. Wealth inequality has attracted particular attention with wealth-to-income ratios doubling in developed countries over the last 40 years (Piketty and Zucman 2014) and a growing dispersion in wealth holdings (Saez and Zucman 2016, Kopczuk and Saez 2004). To prevent increasing capital concentration, Piketty (2014) proposes a global wealth tax, which has spurred a vigorous debate about the efficacy of such taxes.

Wealth taxes are controversial. A common belief is that they are distortionary and discourage savings, which if true, might depress the long-run capital stock and lower economic growth. In addition, many difficulties related to defining the tax base and appraising assets mar the administration of wealth taxation (Boadway, Chamberlain, and Emmerson 2010; Brown 1991; Mirrlees et al. 2011), making wealth taxes susceptible to tax avoidance and tax evasion.

Despite being controversial, wealth taxes do exist, or have existed, in Spain, France, Sweden, and other countries. While the wealth-tax schemes are broadly similar in these countries, Sweden stands out as a unique testing ground. The administrative data, covering the universe of Swedish tax records over the period 2000–2006, comprise financial and nonfinancial wealth holdings, asset-by-asset, for each taxpayer. Moreover, the institutional setting generates useful exogenous variation.

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Sweden has taxed net wealth since 1910. From 1991, when an extensive tax reform was implemented, until the tax was repealed in 2007, the wealth tax was progressive and comprised two brackets. The marginal tax rate was zero below an exemption threshold and 1.5 percent above it, but the threshold was changed a number of times. The design of the tax schedule gives rise to two sources of variation that can be exploited empirically. First, the threshold for taxable wealth creates a kink in the budget set, which induces an incentive for individuals to bunch at the kink point. Second, the change in the threshold over time allows for a difference-in-difference research design.

The Swedish setting is also ideal for studying the nature of responses to a wealth tax. Slemrod (1990, 1995) categorizes the behavioral effects of tax reforms and argues that responses are hierarchical: individuals are likely to first respond in reporting terms and resort to real responses only if avoidance or evasion opportunities prove limited. In the Swedish setting, avoidance and evasion opportunities were abundant. Different assets were taxed at different rates, which enabled tax avoidance by means of strategic rebalancing of portfolios. In addition, the tax base comprised some securities that individuals were supposed to self-report, which set the stage for tax evasion, either by underreporting the value of assets or by concocting liabilities that did not exist.

This study takes advantage of the Swedish data and institutional context to bring new evidence to the debate on wealth taxation. My dataset covers almost 51 million observations in a seven-year panel of taxpayers. I exploit both cross-sectional and panel methods to estimate the effect of an annual wealth tax on reported taxable net wealth. I further decompose the responses into real and reporting effects.

In the first part of the paper, I use the variation in tax rates across brackets to estimate bunching at the kink point, i.e., the excess mass in the distribution at the threshold. When applying the methods proposed by Saez (2010) and Chetty et al. (2011) as well as nonparametric strategies to the data, I find statistically significant evidence of bunching: the implied net-of-tax-rate elasticities lie in the range $[0.09, 0.27]$.[1]

In the second part of the paper, I decompose effects into real and reporting responses using two complementary approaches.

I first exploit the fact that Swedish taxpayers were required to self-report non-third-party reported wealth to the tax authority and assess the extent to which they did so truthfully. To this end, I use administrative data from the car registry, a resource that, surprisingly, was unavailable to the tax authority when the wealth tax was in place. Since cars constituted the lion's share of non-third-party reported wealth, they are a close proxy for the amount individuals were supposed to self-report. I show that the self-reported assets of car owners who bunch at the kink point are systematically lower than the reported assets of car owners with no incentive to underreport. Moreover, bunching estimates are 33 percent lower than in the baseline case when I replace the self-reporting errors of bunchers with errors of those with no incentive to misreport. Close to the threshold, 75–85 percent of

[1] The bunching strategy identifies a local elasticity valid for wealth levels close to the threshold. Even though I find similar elasticities at various locations of the kink, elasticities may vary across the wealth distribution.

taxpayers self-reported assets lower than their true car value—a strategy that eliminated around 70 percent of the tax liability. These findings suggest substantial underreporting to evade the tax.

I then approach real responses from a reduced-form perspective. The detailed information about the wealth composition of each Swedish citizen in my dataset allows me to decompose changes in the market value of individuals' portfolios into active rebalancing (saving) and passive (market-driven) fluctuations. Using difference-in-difference designs around the tax cutoff, I find no effects of the wealth tax on financial savings, realizations of capital gains and losses, taxable income or rebalancing of portfolios to avoid the tax.

Taken together, these results suggest that the estimated elasticities mainly represent reporting responses. This finding is important for two reasons.

First, understanding the nature of responses sheds light on whether the estimated elasticities are representative of long-run effects. If a permanent wealth-tax reform spurs real effects that reduce saving and lower wealth over time, elasticities will differ in the short and long run. However, if taxpayers only respond instantaneously along evasion or avoidance margins, the long-run and short-run elasticities will coincide. Second, real responses generate larger distortions that prevail over time, so that this distinction is crucial for quantifying effects on welfare.

The paper contributes to a public economics literature on the potency of wealth taxation as a redistributive policy tool. While the standard theoretical results that capital and wealth should not be taxed (Atkinson and Stiglitz 1976, Chamley 1986, Judd 1985) have been scrutinized and challenged (Banks and Diamond 2010, Diamond and Saez 2011, Piketty 2014), the potential administrative difficulties involved in wealth taxation have led others to advocate against them (Boadway, Chamberlain, and Emmerson 2010; Brown 1991; Mirrlees et al. 2011). This debate has persisted despite a lack of empirical evidence concerning the alleged distortiveness of wealth taxation as well as of its merits in redistributing resources from the wealthy to the poor, as noted by Kopczuk (2013).

By bringing empirical evidence to this discussion, I can address a number of the unresolved questions in the literature. In particular, I shed light on what are arguably the two main concerns when considering wealth taxation: the twin fears that they may lower savings and that they may trigger avoidance and evasion. Slemrod's (1990, 1995) hierarchy of tax responses suggests that in settings conducive to avoidance and evasion, one should not expect real responses to occur. In the Swedish setting, where such opportunities were ample, I find evidence supporting this claim: the positive elasticities that I find appear to be reporting responses rather than adjustments in saving.

This paper is also related to the voluminous literature that studies the response of savings to changes in capital income taxes. Taxing the stock of wealth is closely related to a tax on the returns to savings and most papers within this strand estimate Euler equations to arrive at the intertemporal elasticity of substitution (IES), without exploiting policy variation directly.[2] In this paper, I estimate the policy-relevant

[2] See, e.g., Bernheim (2002) and Attanasio and Weber (2010) for overviews of the literature.

direct effect of wealth taxes on savings. I do not make statements about the IES, recognizing that the implied IES estimate will be contaminated by the avoidance and evasion opportunities in the environment at hand.

The rest of the paper is organized as follows. Section I presents the institutional setup. Section II describes the data. In Section III, estimates of bunching at kink points along with implied net-of-tax-rate elasticities are presented. A decomposition of effects into real and reporting responses is carried out in Section IV and Section V concludes.

I. Institutional Background

For almost a century, 1910–2007, net wealth was taxed annually in Sweden, according to a progressive tax scheme. In 1991, an extensive tax reform replaced a system with three marginal tax brackets by a two-bracket scheme. The new design was simple: 0 percent marginal tax rates below a threshold of SEK 900,000 (approximately USD 114,000 in 2014) and 1.5 percent above it. The marginal tax rates in the two brackets remained the same until the tax was repealed in 2007, but the threshold was changed several times. The tax was filed at the household level.[3]

Table 1 lists the changes in the threshold during the period considered in the paper, 2000–2006, along with tax revenue and the share of the population who paid the tax. As indicated in the table, from 2001 onwards, the threshold was different for singles and for couples who filed the tax jointly.[4] The threshold for singles was raised in 2001 and 2002, and the threshold for couples in 2001, 2002, and 2005. As expected, government revenue from the tax is negatively correlated with the threshold level. In 2000, almost 8 percent of the population paid wealth taxes and wealth-tax revenue amounted to 1 percent of total tax revenue. In 2006, the gradual increases in the threshold had reduced the share of eligible taxpayers to 3 percent and lowered wealth tax revenue to 0.7 percent. Since the wealth distribution is highly skewed at the top, the tax was not only paid by the extremely wealthy.

To describe the tax base more formally, let q_{ijt} denote the volume of taxable asset j, held by individual i at time t; let p_{jt} denote the price of asset j; and let w_j be a weight, capturing the extent to which the asset was taxed. Similarily, d_{ikt} denotes the value of liability k. Taxable net wealth of the household, W_{it}, is then defined:

$$W_{it} = \sum_{j \in Assets} w_j p_{jt} q_{ijt} - \sum_{k \in Liabilities} w_k d_{ikt}, \tag{1}$$

for all taxable assets $j = 1, \ldots, J$ and liabilities $k = 1, \ldots, K$.

Taxable assets consist of financial securities, real estate, and consumption durables such as cars and boats. The general principle was that assets should be taxed at their market value, so that $w_j = 1$, but there were many exemptions to the rule,

[3] Individuals aged 18 and above living with their parent(s) pay the tax individually and are thus treated as separate tax paying units.

[4] Joint taxation applied to married couples, cohabiting couples who either had been married previously or had children. Children's assets and liabilities were added to the household's taxable wealth. Different exemption levels for couples and singles provided incentives for individuals to get married—a topic left for future work.

Table 1—The Swedish Wealth Tax, 2000–2006

Year	Tax revenue percent	Tax payers percent	Threshold, singles (SEK/USD)	Threshold, couples (SEK/USD)
2000	1.0	7.7	900/129	900/129
2001	0.8	5.3	1,000/143	1,500/214
2002	0.5	2.3	1,500/214	2,000/286
2003	0.7	3.5	1,500/214	2,000/286
2004	0.7	3.6	1,500/214	2,000/286
2005	0.6	2.5	1,500/214	3,000/429
2006	0.7	3.0	1,500/214	3,000/429

Notes: The table shows aggregate statistics of the wealth tax for the period studied. Tax revenue is presented as a percentage of total tax revenue. Monetary values are presented in 1,000 SEK and 1,000 USD.

Source: Statistics Sweden and the Swedish National Financial Management Authority

i.e., assets for which $w_j < 1$, providing households with an opportunity to legally avoid the tax by shifting wealth to assets taxed at lower rates. Specifically, consumption durables, bank deposits, bonds, quoted options, and insurance not tied to retirement were taxed at 100 percent of their market value. Stocks were taxed at either 0 or 80 percent of their market value depending on firm type and ownership structure.[5] Mutual funds were taxed according to the underlying composition. Real estate, including tenant-owned apartments, was taxed at 75 percent of the market value. Ownership in closely held businesses was taxable to some extent, while capital not used in business activities was taxed as if held directly by the owner. Pension savings were completely tax exempt, as were art and jewelry.

Fewer exemptions existed for liabilities. Taxable liabilities were always assessed at market value and debt could only be subtracted from assets if it financed taxable assets, e.g., mortgages. The only exception was student loans, which were deductible although human capital was not taxed.

As a result of these many exemptions, the total value of taxable assets differed from the total market value of household wealth. Applying the balance sheet data on the households' total financial assets, provided by the financial accounts, together with the nonfinancial holdings of households, obtained from the Swedish National Wealth Database, reveals that the fraction of total taxable assets to market-value assets was around 36 percent during this time period, largely because pension wealth was untaxed.

An alternative way of characterizing taxable net wealth, which will prove useful for understanding the nature of responses later in the analysis, is to distinguish between wealth reported to the tax authority by third parties, such as banks and other

[5] Shares in firms on the so-called A-list, comprising well-established firms with many shareholders, were taxed at 80 percent of their market value. Shares in firms on the O-list, which attracted younger firms with its lower requirements for listing, were taxed at 0 percent. To prevent firm flight from the former to the latter, various regulations were implemented, see for instance Swedish Government Offices (2004) and Henrekson and Jakobsson (2002) on the impact of wealth taxes on firm ownership. Since the tax was considered a deterrent to firm enlistment on the stock exchange, majority owners holding shares exceeding 25 percent were made tax exempt (so that w_j might depend on q_{ijt}). Foreign stocks were typically treated as shares in firms on the A-list, i.e., taxed at 80 percent of their market value.

financial institutions, and self-reported wealth that the households themselves were supposed to report, so that:

$$W_{it} = W_{it}^{TR} + W_{it}^{SR}, \tag{2}$$

where W_{it}^{TR} denotes third-party reported net wealth and W_{it}^{SR} denotes self-reported net wealth. To be clear, a response of W_{it}^{SR} to a change in the tax rate does not necessarily reflect changes in the actual self-reported wealth, but could be due to changed self-reporting of assets and liabilities on the tax form.

The timing within the tax cycles was as follows. By December 31 of the fiscal year t, third parties sent detailed information about individuals' financial and real-estate assets and liabilities to the tax authority who then used market prices from stock markets and real-estate neighborhood prices to value those assets. Based on these assessments, pre-populated tax forms were sent out to taxpayers in the beginning of year $t + 1$.[6] Households were then obligated to self-report non-third-party reported wealth holdings and liabilities. This obligation applied to households who were already above the threshold in terms of W_{it}^{TR}, and who were supposed to report additional net wealth, and to households who did not reach the threshold in terms of W_{it}^{TR}, but whose added self-reported wealth pushed them into the higher bracket. These households were supposed to make the necessary adjustments to the pre-populated forms and submit them to the tax authority by May 1 the same year.

The presence of a self-reported component of the tax base is crucial since it provided households with evasion opportunities. As mentioned above, the variability in the rates at which assets were taxed supplied (legal) tax avoidance opportunities by means of asset shifting. The reliance on complementary self-reports additionally made the system vulnerable to (illegal) tax evasion. Households could simply either place their wealth in self-declared items and abstain from reporting them or fabricate liabilities that did not exist.[7] As noted by Kleven et al. (2011), enforcement of self-reporting is notoriously difficult.

The empirical analysis exploits several sources of variation to quantify the effects of the wealth tax. A possible concern when exploiting the shifts in the wealth-tax threshold over time, is that these may be endogenous. In the Swedish setting, however, this was not the case. During the sample period, real estate was taxed separately at 1 percent of the taxable value. In practice, the changes in the wealth-tax brackets, documented in Table 1, were indexed to changes in the taxable value of real estate. This scheme was designed to avoid sudden, large increases in tax liabilities. The large increases in the wealth-tax threshold in the early 2000s are the result of substantial increases in real-estate tax liabilities at the end of the 1990s, following a renewal of the government's procedure for computing the taxable value of housing. This suggests that the shifts in the threshold were not driven by a powerful lobby of

[6] An explanation of how to compute the tax liabilities was appended to the form. Examples of a pre-populated form and the enclosed instructions, respectively, are provided in Figures A.1 and A.2 in the online Appendix.

[7] One way was to report a debt contract with a relative/friend such that the liability was held by someone whose wealth placed them above the threshold while the asset was held by someone located below.

wealthy households. The wealth tax was finally repealed in January 2007, following the victory of a center-right-wing coalition in the September 2006 election.

On top of real-estate and wealth taxation, capital income is taxed according to a two-bracket system, with a marginal tax rate of 22 percent in the lower bracket and a marginal tax rate of 30 percent for capital income above SEK −100,000. Negative capital income is credited against other taxes. The capital income tax base is the sum of interest income, rental income, dividends minus interest payments (mortgages, credit cards, and bank loans), and realized capital gains' net of losses.

II. Data

The paper uses data from the following administrative registers, provided by Statistics Sweden: the Income and Tax Register (Inkomst och Taxeringsregistret); the Swedish Car Registry (Fordonsregistret); the Integrated Database for Labour Market Research (LISA); and military enlistment data from the National Service Administration (Pliktverket). Each register contains Social Security number equivalents, which enable linking together the datasets at the individual level. The dataset is completed with information on prices and returns of financial securities and transactions prices of real estate and cars, which I collect from various sources. Unless otherwise stated, all data are retrieved for the sample period 2000–2006.

Below, I present the details of the data, followed by a discussion of necessary sample restrictions and descriptive statistics of the benchmark dataset.

A. *The Dataset*

The Income and Tax Registry (IoT) is the primary source of data on wealth holdings used in the analysis, and it holds detailed records of all Swedish taxpayers. Third-party reported assets and liabilites are observable asset-by-asset and retrieved along with the tax price, $w_j p_{jt}$, and q_{ijt} so that W_{it}^{TR} in equation (2) can be computed. To express these assets in terms of market values, I use data on end-of-year prices and annual returns from various sources for financial assets and inflate the tax values of real estate using a method proposed by Statistics Sweden.[8]

The IoT also contains data on self-reported assets and liabilities, albeit not at the individual asset level. These records are retrieved to obtain a measure of W_{it}^{SR} in equation (2) and are added to W_{it}^{TR}, described above, to obtain taxable net wealth, W_{it}. From the IoT, I also collect flows to Individual Retirement Accounts (IRAs) and taxable income, which are used as outcome variables in response to the wealth tax in Section IV.

[8] Since some third-party-reported assets are tax exempt, the market price of financial assets cannot simply be inferred from the tax price, $w_j p_{jt}$. Instead, I use information on prices and returns from Bloomberg, Morningstar, Moneymate, FactSet, Datastream, and the Swedish Tax Authority and link them to the IoT assets using their International Securities Identification Number (ISIN). Real-estate holdings are converted to market values using an assessment method employed by Statistics Sweden. The algorithm is based on inflating tax values by the average ratio of transaction prices to tax values of properties sold during the year—a coefficient that varies over time, across geographic regions, and by real-estate type.

In an attempt to obtain an objective account of the true value of self-reported assets, I resort to the Swedish Car Registry, a resource that, somewhat surprisingly, was unavailable to the tax authority when the wealth tax was in place. Since cars were the most important component of self-reported wealth, this registry can provide an insight into whether individuals were truthful in their self-reports. The car registry entails data on all cars owned by Swedish residents by vintage, brand, and model and are linked to the IoT at the individual-year level. Cars were taxed at market value, and to generate accurate measures of their worth, I use prices of new cars from 1989 onwards from the webpage of the Swedish Tax Authority (2013) and devalue old cars according to a model provided at www.bilpriser.se.[9]

Demographic data such as age, education, occupation, wage earnings, and family status for all Swedish residents above 15 years of age is collected from the LISA database, which includes both spouses' Social Security number equivalents, thereby enabling me to link couples filing the wealth tax jointly. I am able to match 99.9 percent of the taxpayers to the demographic database, yielding a matched dataset consisting of 50,894,803 observations over the sample period.

Finally, to obtain a measure of cognitive ability, I link the data to military enlistment records, comprising men born between 1951 and 1979. The records contain a widely accepted measure of cognitive ability that I use to address heterogeneous responses to the wealth tax.[10]

B. *Assumptions and Sample Restrictions*

To avoid econometric pitfalls, some additional assumptions and sample restrictions are warranted.

For households with a pre-populated third-party-reported net wealth above the kink that pay zero taxes and thus, apparently, self-report liabilities so that they end up below the kink, data on taxable net wealth is missing in a number of cases. In an interval of 1.5 million SEK above the threshold, the fraction with missing taxable wealth is around 4 percent. I address this issue in two ways. Under the first approach, I exclude these households in the bunching estimation. Under the second approach, I assume that the distribution of reported taxable net wealth for those in the lower bracket is representative for those where taxable net wealth is missing and impute values where missing. I henceforth refer to the second approach as the *imputation method*.[11]

[9] The tax authority provides no assessment of cars manufactured before 1989, but this is not likely to be a problem because of the rapid depreciation of most cars. Bilpriser monitors car transactions and constructs devaluation models based on vintage, car type, and transaction values and a one-year old gasoline driven SUV is, for example, reported to be worth only 68 percent of the new price. In addition, vintage cars older than 30 years were tax exempt.

[10] A comprehensive overview of the test procedure is found in Lindqvist and Vestman (2011). Carlstedt (2000) argues that the test provides an accurate measure of general intelligence. Importantly, it was not possible to avoid military service or to gain other advantages by performing poorly on the test. Moreover, military enlistment was mandatory in Sweden during this period, rendering concerns about sample selection mute. As the test was subject to minor revisions over the years, I create a normalized measure of cognitive skills that I use in the analysis.

[11] Specifically, I randomly draw non-missing taxable wealth figures from the sample of singles and couples with pre-populated third-party reported net wealth above the kink and assign these to singles and couples with third-party reported wealth above the kink but who pay zero taxes. This method may bias the results if the sample with missing taxable net wealth values is endogenously selected. Running a regression on this subgroup, however, I find that an

Since the wealth tax was filed at the household level and since the LISA database does not comprise individuals under 16 years of age, I am unable to assess taxable net wealth for households with children. Fortunately, the demographic dataset contains information about household status, including information about the number of children below 18 years of age. I thus confine the sample to single households and couples without children. This restriction results in 34,244,990 observations in the matched dataset, representing 67 percent of the total number of observations. In this sample, 6 percent of all observations are associated with positive wealth taxes.[12] To alleviate the concern that the results obtained are sensitive to this restriction, Section IIIB performs a robustness exercise in which bunching at the threshold is estimated including households with children.

Special rules applied to the treatment of asset holdings through closely held businesses, as assets used in business activities were tax exempt (see Section I). Because I do not know whether households who own agricultural, rental, or industry property are using them in business activities or not, I remove them from the sample. This entails dropping 6 percent of all observations.[13] Additional results show that the results are robust to including households holding assets through closely held businesses, but the precision of the estimates increases when excluding them. In what follows, I therefore focus on the results obtained when these sample restrictions are imposed.

C. *Descriptive Statistics*

Table 2 presents summary statistics for the population and different subsamples. The results in columns 1, 2, and 3 show that sample means are largely similar when imposing the sample restrictions described in Section IIB. Columns 3 and 4 display means and standard deviations for the main sample used in the paper. The market value of assets net of liabilities and taxable income suggests that the average wealth-income ratio in the sample is 2.75. This is lower than the ratios found for developed countries in Piketty and Zucman (2014), partly due to pension wealth being excluded from the wealth-tax base in our setting.

III. Estimating the Tax Elasticity of Taxable Wealth

This section estimates the elasticity of taxable wealth with respect to the net-of-tax rate.[14] I start by explaining how bunching can identify the tax elasticity. I then estimate the extent of bunching, using parametric as well as nonparametric methods, and compute the implied taxable wealth elasticity. I estimate bunching for different subsamples based on family status, cognitive ability, education, and financial

indicator for being in the sample is uncorrelated with cognitive skills and wage earnings. This suggests that selection bias, at least along these observable dimensions, is unlikely.

[12] In a window of SEK 1 million centered around the tax thresholds, this restricted sample represents 85 percent of the total number of observations.

[13] Around the tax thresholds, the fraction of households owning potential business property is 18 percent.

[14] Note that an estimated response of measured wealth to changes in tax rates does not necessarily imply that taxable wealth W_{it}, defined in equation (1), has changed. Rather, it could be the self-reporting of assets and liabilities that has responded.

TABLE 2—SUMMARY STATISTICS FOR THE SWEDISH POPULATION AND DIFFERENT SUBSAMPLES, 2000–2006

	Population	Singles and couples without children		
		All	Excluding self-employed	
	Mean (1)	Mean (2)	Mean (3)	SD (4)
Demographics				
Age	47.733	53.268	52.905	20.172
Children (percent)	0.316	0.000	0.000	0.000
Male (percent)	0.491	0.500	0.495	0.500
Married (percent)	0.414	0.384	0.363	0.481
Higher education (percent)	0.215	0.202	0.203	0.402
Income and taxes				
Taxable income	174,602	169,832	169,001	151,721
Wealth tax paid	744	891	743	18,762
Wealth tax (percent)	0.049	0.060	0.054	0.226
Skills				
Cognitive skills	−0.003	−0.052	−0.052	1.022
Car				
Number of cars	0.527	0.514	0.512	0.944
Car value	20,114	19,252	19,347	48,454
Number of new cars	0.161	0.146	0.147	0.453
Car value (new cars)	9,615	8,791	8,848	38,612
Wealth				
Assets (third party)	415,838	440,230	358,161	11,528,085
Debt (third party)	206,166	159,713	142,648	459,004
Financial assets (market)	204,683	249,498	219,782	14,366,532
Real estate (market)	480,740	466,000	366,745	685,100
Portfolio composition				
Real estate (percent)	0.512	0.459	0.474	0.429
Bank account (percent)	0.198	0.231	0.223	0.336
Funds (percent)	0.208	0.218	0.216	0.331
Stocks (percent)	0.060	0.065	0.061	0.180
Bonds (percent)	0.022	0.027	0.026	0.107
Wealth accumulation				
Realized capital gains	10,963	11,753	10,233	390,331
Realized capital losses	1,048	1,033	955	35,332
Capital income	7,101	10,764	7,867	512,606
Retirement savings	1,996	1,942	1,823	6,580
Savings (end-of-year)	4,653	5,436	5,073	40,607
Savings (beginning-of-year)	1,729	2,107	2,004	39,961
Observations	50,894,803	34,244,990	30,999,874	

Notes: This table presents summary statistics in the full sample and in different subsamples at the individual level. *Children* is an indicator variable for having at least one child below 18 years of age in the household. *Married* refers to share individuals married. *Higher education* is a dummy for having a degree beyond secondary school. All monetary values are denoted in SEK. *Taxable income* includes all wage earnings, nonlabor income, as well as pension income. *Car value* is calculated using the valuation method described in Section II while *Car value* (*new cars*) restricts attention to cars bought in the current year. See Section I for a description of wealth variables. *Realized capital gains* and *Realized capital losses* refer to realizations during the year. *Capital income* is capital income (including realizations of gains' net of losses and home mortgage payments, etc). *Retirement savings* is savings in tax-deferred accounts. *Savings* (*end-of-year*) and *Savings* (*beginning-of-year*) refer to active changes in the portfolio.

literacy to uncover potential heterogeneity. The estimates are then subjected to extensive robustness analysis.

A. *Bunching Estimation*

Consider individuals with strictly quasi-concave preferences choosing taxable wealth optimally. Analogous to the labor-supply model in Saez (2010), they are heterogenous with respect to preferences, savings and avoidance/evasion technologies, which are distributed according to some continuous and differentiable cumulative distribution function. In a steady state under a constant linear tax, τ, on wealth (the stock of savings) and no uncertainty, individuals' taxable net wealth, W, will be distributed according to a smooth density function, $h(W)$.

Now, introduce a kink in the budget set at threshold wealth level W^*, associated with a higher marginal tax rate $\tau + d\tau$ above the kink. Agents who chose taxable net wealth levels in some interval $[W^*, W^* + dz]$ under the linear tax scheme will bunch at the kink point. The number of households who bunch can be approximated by $B = h(W^*)\, dW^*$, for a small interval dW^*. Individuals who chose higher wealth levels in the absence of the higher tax reduce their taxable wealth to the point where their indifference curves are tangent to the budget line under the higher tax (with slope $1 - \tau - d\tau$).

For small tax changes, the response is due to the compensated elasticity because the associated income effects in the neighborhood of the kink are negligible. The elasticity refers to the percentage change in wealth arising from a 1 percent increase in the net-of-tax rate, $1 - \tau$. Combining the definition of the elasticity with the expression for bunching at the kink gives

$$\frac{B}{h(W^*)\, W^*} = \varepsilon_{W,\tau} \frac{d\tau}{1-\tau}. \tag{3}$$

The equation identifies the elasticity as a function of bunching and the tax schedule. If responses are heterogeneous over the wealth distribution, the identified elasticity is valid close to the threshold.

Moreover, the responses to a reform that introduces a kink in the budget set might entail important dynamic mechanisms which are not captured in this simple framework. In the short run, wealth is fixed and agents can only respond through legal or illegal reporting responses. In the long run, however, adjustments take place both along the reporting margin and through saving responses.

Section B in the Appendix (available online) lays out a parametric two-period model where individuals decide on evasion in the short run and on both evasion and saving in the long run. In that framework, short-run bunching is the difference in evasion on either side of the threshold while the long-run bunching is given by that difference augmented by the difference in saving rates below the threshold and above. Section IV investigates whether the estimated elasticities are due to saving or reporting responses.

Parametric Estimation.—To compute $\varepsilon_{W,\tau}$ in equation (3), I next estimate the excess mass in the distribution of taxable net wealth at the kink point. To this end,

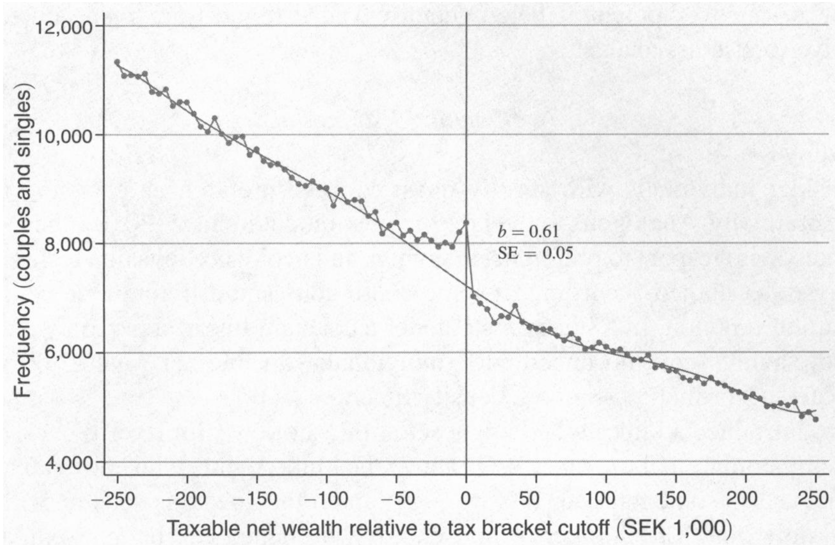

FIGURE 1. ESTIMATED BUNCHING OF TAXABLE NET WEALTH AT THE THRESHOLD

Notes: The figure shows the distribution of taxable net wealth around the shift in the tax brackets, demarcated by the vertical line at 0, for the years 2000–2006. The tax rate is 0 below the threshold and 1.5 percent above. The dotted series consist of a histogram relative to the normalized kink point. Each bin corresponds to the number of households within SEK 5,000. The estimated counterfactual density, displayed by the solid line, was obtained by fitting a seven-degree polynomial to the density, excluding points within SEK 40,000 below the kink. *b* denotes the estimated excess mass, and SE is the estimated standard error.

the counterfactual density, i.e., the mass at the kink point under a zero tax rate, must be estimated. I proceed in two ways. First, I use the parametric method employed in Chetty et al. (2011), which makes my results directly comparable to the existing literature. Second, I estimate bunching nonparametrically, combining the data on third-party reported wealth and taxable net wealth.

Figure 1 plots the distribution of taxable net wealth, around the threshold, for the main sample over the years 2000–2006. The normalized measure of wealth is constructed as the difference between wealth and the exemption level. Households are then grouped into SEK 5,000 bins. Bin counts are plotted around the kink point, represented by the vertical line at point zero. The figure reveals bunching in the distribution of taxable net wealth around the kink point: there is an excess mass of households at the threshold in the otherwise smooth distribution. When computing the counterfactual density and estimating bunching, I take into account that the spike in the empirical distribution is diffused to the left of the threshold.

To quantify the excess mass at the kink, I first estimate the counterfactual density as the distribution of taxable net wealth in the absence of taxation. Specifically, I fit the following polynomial to the empirical distribution:

$$(4) \qquad N_j = \beta_0 + \beta_1 W + \beta_2 W^2 + \cdots + \beta_n W^n + \sum_{i=-R}^{Q} \phi_i \mathbf{1}[W_j = i] + \epsilon_j,$$

where N_j denotes the number of households in bin j, W_j is taxable net wealth relative to the kink in SEK 5,000 intervals, and n is the order of the polynomial. The sum of the indicator variables on the right-hand side reflects the exclusion of observations close to the kink point, as these reflect bunching and must be removed. Values R and Q, respectively, define the lower and upper bounds of the bunching interval. The counterfactual density is obtained as the predicted values from equation (4), $\hat{N}_j$, excluding the contribution of the dummies around the kink, as captured by the ϕ_is.

The bunching estimate is then given by the number of households in excess of the counterfactual density close to the kink point, i.e., $\hat{B} = \sum_{j=-R}^{Q} N_j - \hat{N}_j$. This method of estimating bunching entails a potential bias, since the counterfactual fails to satisfy the integration constraint. I therefore follow Chetty et al. (2011) and estimate a density that shifts the counterfactual distribution to the right of the kink until the integration constraint is met.[15] To obtain the elasticity in equation (3), an estimate of $\frac{B}{h(W^*)}$ is needed, which is computed as:

$$\hat{b} = \frac{\hat{B}}{\frac{\sum_{j=-R}^{Q} \hat{N}_j}{Q+R-1}}. \tag{5}$$

In addition to the distribution of taxable net wealth, Figure 1 plots the counterfactual distribution, estimated as a seven-degree polynomial. The chosen window of bunching stretches from SEK 40,000 below the threshold to the kink, represented by the vertical line at zero. Marginal changes in the window of bunching and the order of the polynomial leave the estimated excess mass largely unaffected. I estimate bunching at $\hat{b} = 0.61$, meaning that there is 61 percent more mass, relative to the counterfactual distribution, within SEK 5,000 of the kink.

The standard error for $\hat{b}$ is estimated using a parametric bootstrap procedure, as in Chetty et al. (2011). It addresses mis-specification of the polynomial rather than sampling errors, as the estimate is constructed using the population distribution. The estimated standard error of $\hat{b}$ in Figure 1 is 0.05 with an implied t-statistic of 12.2, so the null hypothesis of no bunching at the kink point is strongly rejected.[16]

This corresponds to a net-of-tax elasticity of taxable wealth, $\varepsilon_{W,\tau}$, of 0.127 with a standard error of (0.010), computed with the delta method. Under the imputation method, implied elasticities are larger, with a point estimate of 0.271 for the seven-degree polynomial (as displayed in Table 3).

Turning to heterogenous effects, presented in Table 3, I first break up the sample into singles and couples filing the tax jointly and obtain similar results across those groups. Next, I divide the sample into those of high and low cognitive ability, according to the IQ test from the military enlistment records, on rows 4 and 5. The results suggest that the elasticity estimates for high-ability households are roughly 16 percent larger than those of low-ability households. The same holds when comparing households where at least one member has more than one year of tertiary

[15] The counterfactual density is in this case given by: $N_j\left(1 + \mathbf{1}[j > Q]\frac{\hat{B}}{\sum_{j=Q+1}^{\infty} N_j}\right) = \beta_0 + \beta_1 W + \beta_2 W^2 + \cdots + \beta_n W^n + \sum_{i=-R}^{Q} \phi_i \mathbf{1}[W_j = i] + \epsilon_j$.

[16] Online Appendix Figure C.1 tests the robustness of these findings when estimating the polynomial separately on either side of the threshold. The bunching estimate is decreasing in the degree of the polynomial simply because, with a higher degree, the estimation draws more heavily on the density close to, but below, the threshold. However, for reasonable variations, bunching is always significant.

Table 3—Bunching Estimates, $\hat{b}$, 2000–2006

	Parametric				Nonparametric	
	(1)	(2)	(3)	(4)	(5)	(6)
All	0.611	0.597	1.306	1.383	0.447	0.940
	(0.049)	(0.065)	(0.069)	(0.092)	(0.052)	(0.052)
Couples	0.596	0.567	1.336	1.399	0.332	0.840
	(0.080)	(0.080)	(0.081)	(0.112)	(0.041)	(0.052)
Singles	0.623	0.618	1.283	1.373	0.460	0.945
	(0.071)	(0.068)	(0.078)	(0.104)	(0.057)	(0.059)
High IQ	0.492	0.401	0.989	0.982	0.400	1.211
	(0.131)	(0.114)	(0.136)	(0.149)	(0.070)	(0.208)
Low IQ	0.424	0.576	0.711	0.937	0.452	0.732
High education	0.522	0.528	0.986	1.066	0.400	1.028
	(0.117)	(0.093)	(0.134)	(0.125)	(0.070)	(0.242)
Low education	0.402	0.399	0.751	0.838	0.452	0.691
	(0.134)	(0.115)	(0.172)	(0.144)	(0.044)	(0.202)
Financial sector	0.556	0.508	0.846	0.838	0.857	1.300
	(0.147)	(0.139)	(0.181)	(0.154)	(0.193)	(0.227)
Imputation	No	No	Yes	Yes	No	Yes
Degree	7	3	7	3	—	—

Notes: The estimates in columns 1–4 use the parametric approach, see equation (4). Standard errors are estimated by a parametric bootstrap procedure. Columns 5–6 compute bunching estimates in accordance with equation (7), with $\delta = 40{,}000$. Here, standard errors are estimated using a nonparametric bootstrap procedure, as described in the main text. The standard error of each estimate is the standard deviation of the distribution of the $\hat{b}$:s. Imputation refers to the method of handling missing values described in Section II. High-IQ and low-IQ households are defined as the male having, respectively, positive and negative z-scores in cognitive ability. High education refers to whether any of the household members have tertiary education longer than one year. *Financial sector* refers to those households where one member works in the financial sector.

education to those with less education on rows 6 and 7. The results for individuals working in the financial sector are about the same as for the general population. Unreported estimates show no heterogeneity in bunching based on age.

Nonparametric Estimation.—The parametric bunching estimates above rely on the estimated counterfactual accurately reflecting the distribution of taxable net wealth that would obtain if taxes were zero. Since a higher marginal tax rate affects both households to the right of the kink and those in its vicinity, however, it is not obvious that the fitted polynomial closely matches the true counterfactual. A poorly estimated distribution would bias the counterfactual density close to the kink and hence affect the bunching estimates. My second approach instead estimates bunching nonparametrically, by exploiting the paired observations of third-party-reported net wealth and taxable net wealth for each household. Under this approach, $\hat{b}_k$ is defined as the difference in the number of households located within a δ-interval below the kink k in taxable and third-party reported wealth, normalized by the counterfactual density, given by the number of households in the bin immediately to the left of the kink:

$$\text{(6)} \qquad \hat{b}_k = \frac{\sum_i \mathbf{1}\left[W^k - \delta < W_i < W^k\right] - \mathbf{1}\left[W^k - \delta < W_i^{TR} < W^k\right]}{\sum_i \mathbf{1}\left[W^k - \gamma < W_i^{TR} < W^k\right]},$$

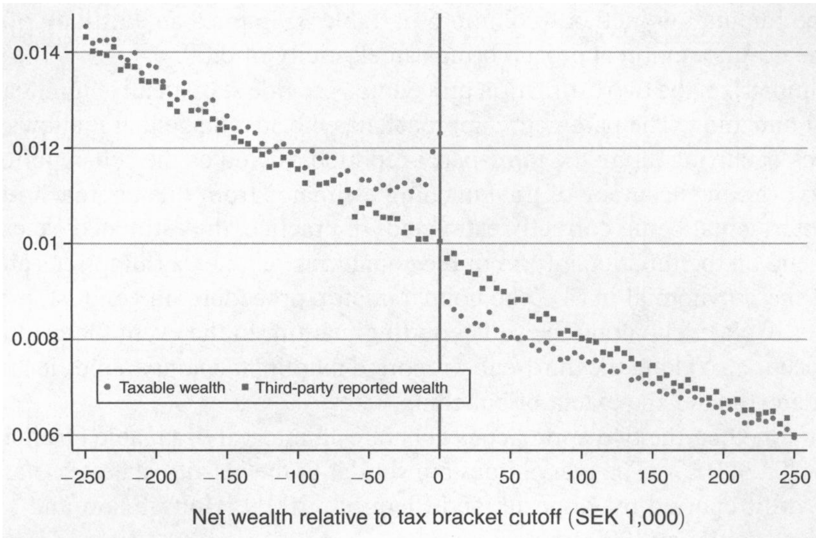

FIGURE 2. TAXABLE AND THIRD-PARTY REPORTED WEALTH

Notes: The figure shows the distribution of taxable net wealth around the shift in the tax brackets, demarcated by the vertical line at 0, for the years 2000–2006 along with third-party reported wealth during the same period. Missing values of taxable wealth have been imputed as described in the text.

where δ is the parameter governing the width of the bunching window while γ denotes the width of the counterfactual density. To simplify comparison, I assume the same window as in the parametric estimation and let $\delta =$ SEK 40,000 and $\gamma =$ SEK 5,000.[17]

To purge the estimated excess mass at the actual tax threshold from systematic differences in taxable and third-party reported wealth, the final bunching estimator $\hat{b}$ is given by

$$\hat{b} = \hat{b}_{W^*} - \hat{b}_{W^p}, \tag{7}$$

where W^p is a placebo kink. The identification assumption is that estimated bunching at the placebo kink is representative of what would occur at the actual kink, in case it were not a tax threshold.

Figure 2 shows the distributions of taxable and third-party reported wealth, when applying the imputation method to deal with missing values of taxable wealth. Columns 5 and 6 of Table 3 presents the bunching estimates obtained under this approach. The standard errors are computed using a bootstrap method, in which new distributions of third-party-reported net wealth and taxable net wealth are drawn with replacement from the true distribution. The standard error of the bunching coefficient is represented by the standard deviation of the distribution of estimated coefficients. The estimated excess mass is always significant. The estimated bunching

[17] Using the average of third-party-reported net wealth in the interval $[W^* - \delta, W^*]$ as the counterfactual density does not affect the results qualitatively, and quantitatively, the differences are small.

under the imputation method (column 6 of Table 3) implies an elasticity of 0.194, while the no-imputation approach brings an elasticity of 0.09.

To summarize, the two estimation procedures provide statistically significant estimates of bunching. The parametric approach has the advantage that it allows for tax responses occurring along the third-party-reported as well as the self-reported margin. However, the accuracy of the bunching estimates from this approach relies on the counterfactual being correctly estimated. In practice, the estimated excess mass will depend on the functional form of the counterfactual, i.e., assumptions about the order of the polynomial in (4). The nonparametric procedure, in contrast, presumes responses exclusively along the self-reporting margin. In the event the responses to the tax occur also along the third-party reported margin, the nonparametric approach thus underestimates the extent of bunching.

Taken together, the two approaches generate an interval of taxable elasticity estimates of [0.09, 0.27]. The magnitudes are similar to the net-of-estate-tax elasticities of net worth reported by Kopczuk and Slemrod (2001), Holtz-Eakin and Marples (2001), and Joulfaian (2006).

B. *Robustness*

To corroborate that the documented bunching is due to the discountinuity in the tax schedule, I perform a placebo exercise to assess the assumption of a smooth distribution of taxable net wealth in absence of the tax. Online Appendix Figure C.2 investigates whether bunching tracks changes in exemptions over time, or if alternative explanations can account for the evolution of bunching. In this figure, I compare the distribution of taxable net wealth for singles in 2001 to that of 2006, a period marked by an increase in the threshold by SEK 500,000. The excess mass in 2001 is located at the tax threshold and the figure presents three candidate locations for bunching in 2006. The first placebo kink denotes the threshold value that would prevail had it followed inflation, the second indicates the corresponding value had it followed the risk-free interest rate, and the final kink illustrates the value tantamount to tracking the Stockholm Stock Exchange Index. The figure confirms that the excess mass does track the 2006 wealth-tax threshold.[18] A similar pattern is displayed in online Appendix Figure C.3 where the time series of bunching estimates—obtained from the nonparametric approach—are plotted. The plot reveals sharp increases in the bunching estimates during the various thresholds for couples that were in place, consistent with causal effects of the kinks.

Furthermore, Figure 1 reveals that bunching is asymmetrical. Theoretically, one should expect a clean spike at the kink point, but the data suggest that there is excess mass also to the left of the distribution. Such asymmetric bunching could be obtained in the presence of a fixed cost of self-reporting, if households confound average and marginal tax rates, or if only households to the right of the kink are more prone to self-report. I assess the plausibility of each of these explanations in online Appendix D.

[18] There appears to be some excess mass at the first placebo kink in 2006, but relative to the counterfactual density at that point, the estimated bunching there is negligible compared to where the kink is actually located.

Finally, a remaining concern is that the results obtained are specific to the sample of households without children. To alleviate this concern, I compute the total wealth of each tax paying unit, using the original data that cover all individuals aged 16 or older. This calculation includes households with children below the age of 16, but not the wealth of those minors. Online Appendix Figure C.4 reveals that the bunching estimates obtained under these assumptions are quantitatively similar to those obtained in the baseline.

IV. Nature of Responses

This section aims to understand the nature of the estimated responses to the wealth tax. Do they represent real saving responses or are they due to reporting margin effects? Slemrod (1990, 1995) analyzes effects of the US Tax Reform Act of 1986 and summarizes responses hierarchically. He concludes that most behavioral responses should occur through avoidance and timing and that only in the absence of such opportunities, real responses are expected to occur. As discussed in Section I, there were ample opportunities to escape the wealth tax in Sweden, both legally and illegally. The taxing of assets at different rates set the stage for avoidance by means of portfolio rebalancing, and the self-reported component of the tax base made the system vulnerable to evasion by means of underreporting or by providing untruthful accounts of nonexistent liabilities.

Figure 3 shows bunching year by year for singles and couples. Since wealth is a stock and not a flow variable, it may take up to a generation to reach a steady-state distribution where everyone's wealth trajectory has been chosen in response to a threshold change. However, the figure suggests that bunching is as strong in years when the threshold was shifted, indicating instantaneous effects and lending support to reporting responses driving the estimated elasticities.[19]

It is natural to organize the analysis of responses to the wealth tax in a manner inspired by Slemrod's hierarchy. I use several, complementary empirical strategies to determine the importance of real and reporting responses. The first approach addresses tax evasion by cross-checking administrative data from the previously unavailable Swedish Car Registry against households' self reports to unveil whether they were prone to report untruthfully. Second, I exploit the numerous threshold shifts over time in a reduced-form approach that estimates effects of the tax on real savings and avoidance decisions. Third, I estimate bunching for a subgroup with limited ability to respond in reporting terms, namely households who do not own a car.

Cross-Checking Self-Reported Assets against Car Holdings.—In an attempt to uncover whether individuals self-report truthfully, I cross-check car values against self-reported assets. As described in Section II, I do not observe self-reported assets item-by-item, which means that I cannot compare the value of cars from

[19] The figure also suggests that the local bunching estimates are similar for different parts of the wealth distribution where the threshold was located, abating concerns that the estimates are applicable only to a narrow part of the wealth distribution.

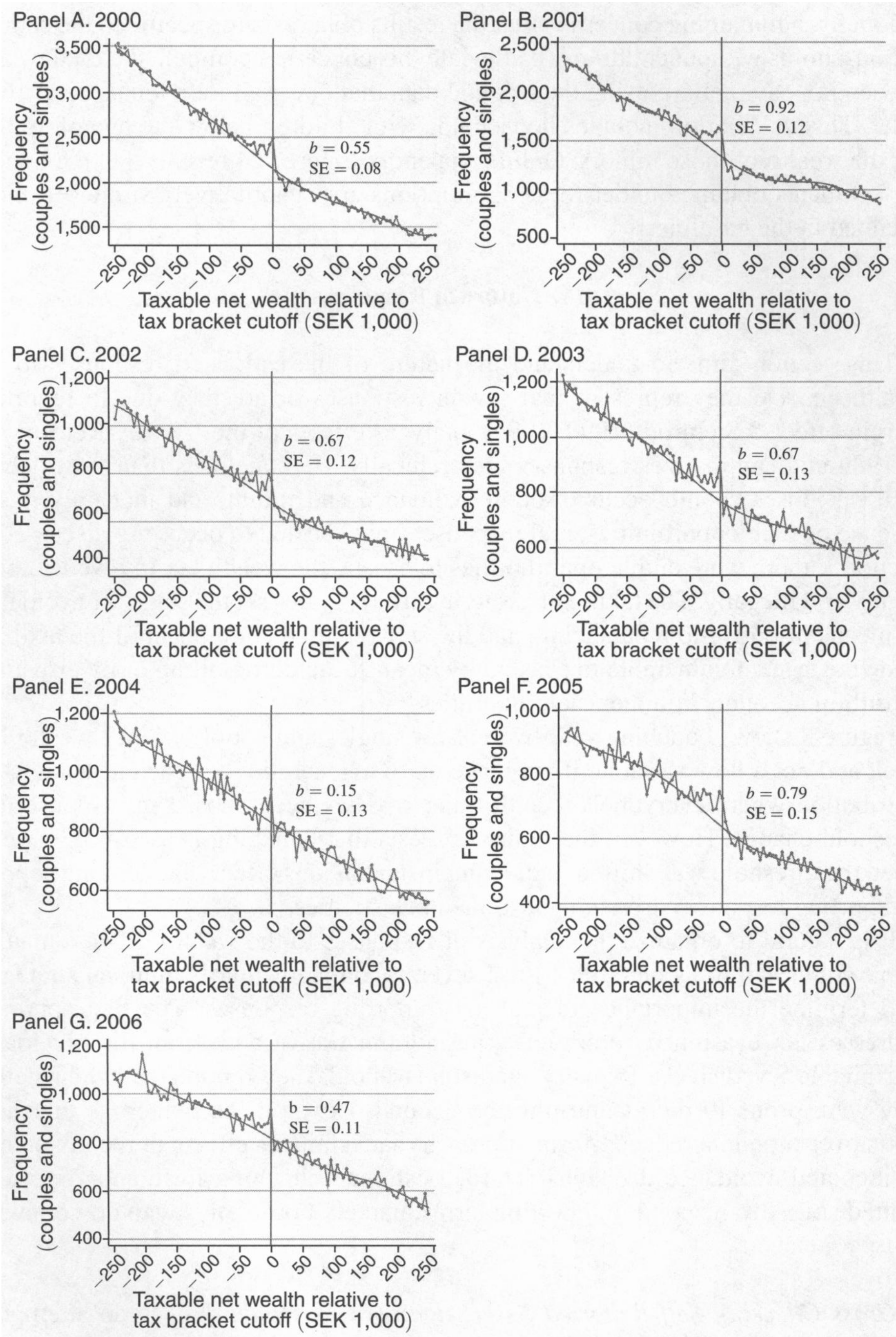

FIGURE 3. TAXABLE NET WEALTH AROUND THE THRESHOLD, YEAR BY YEAR

Notes: These figures plot the empirical distribution of taxable net wealth around the (normalized) kink point year by year for both singles and couples. The vertical line denotes the location of the threshold. Each bin corresponds to the number of households within SEK 5,000. The estimated counterfactual density, displayed by the solid line, was obtained by fitting a seven-degree polynomial to the density, excluding points within SEK 40,000 below the kink. *b* denotes the estimated excess mass, and SE is the estimated standard error.

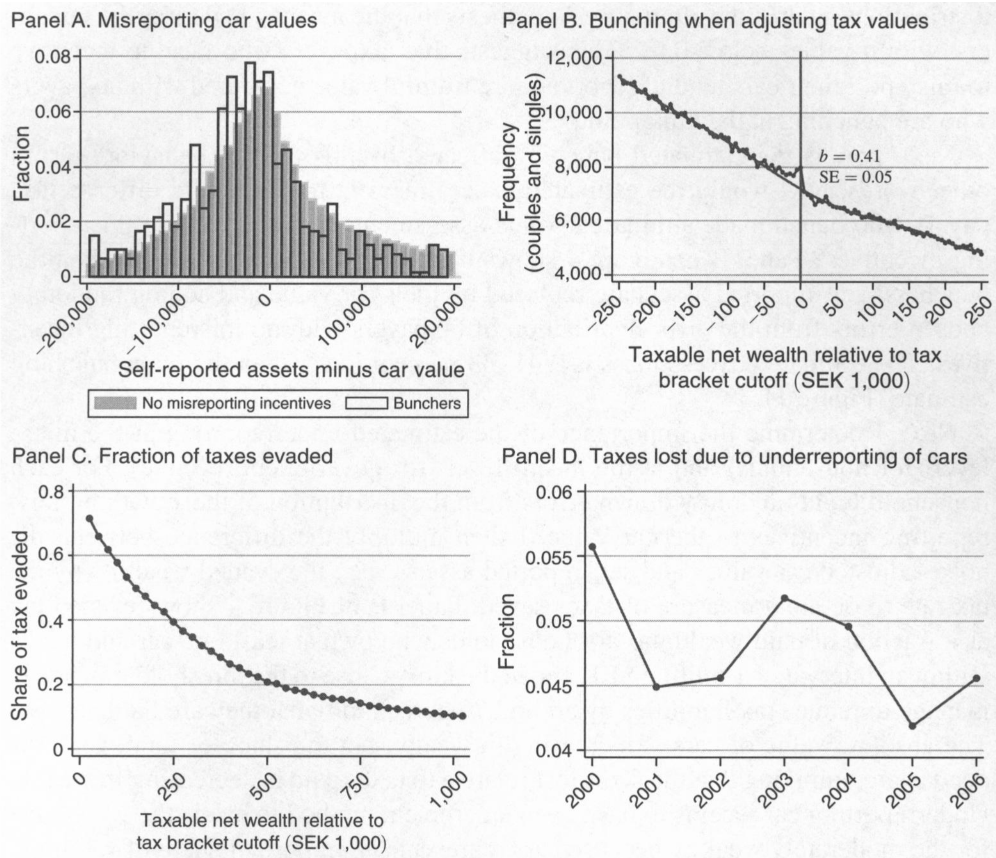

FIGURE 4. CROSS-CHECKING CAR REGISTER

Notes: Panel A shows the distribution of the difference between self-reported assets and car values for car-owning taxpayers who are located below the threshold in terms of both third-party reported and taxable wealth with positive self-reported assets; and car-owning taxpayers who are located above the threshold in third-party reported wealth but below in terms of taxable wealth with positive self-reported assets. Panel B replicates Figure 1 but adjusts the wealth of bunchers by car values, as explained in Section IV. Panel C shows average taxes evaded for car owners as a fraction of total wealth taxes paid for households above the tax threshold. Panel D shows the total taxes lost due to underreporting of cars for each year divided by the total government revenue from the wealth tax.

the car registry to self-reported car values. However, I do observe the total value of self-reported assets (and liabilities, separately), and as cars constituted the lion's share of non-third-party reported wealth, they are a close proxy for the amount that households were supposed to self-report.

To assess the extent to which individuals responded by underreporting car values, I first compute the difference between self-reported assets and the car value. Panel A of Figure 4 shows the distribution of these reporting errors for two groups: those with no misreporting incentives, i.e., taxpayers locating below the threshold in both third-party reported and taxable wealth, and those who bunch, defined here as locating above the threshold in terms of third-party reported wealth but having taxable wealth within SEK 100,000 below. A Kolmogorov-Smirnov test rejects the equality of the two distributions with a p-value less than 0.01. Moreover, both a simple t-test as well as a permutation test reject the null of the equality of the means of the two distributions against the alternative hypothesis that the mean is lower for the bunchers, with p-values below 0.01. This suggests that taxpayers who face no incentive to misreport their cars tend to report a more truthful value compared with taxpayers who are bunching at the kink point.

Next, how is the estimated bunching affected by this type of behavior? Or, in other words, how would the estimated excess mass at the threshold differ if taxpayers who bunch made similar car value assessments as those with no misreporting incentives? Panel B of Figure 4 shows bunching at the tax threshold when the bunchers' self-reported assets are replaced by their car value and adding randomly chosen errors from the error distribution of taxpayers with no misreporting incentives. The estimated excess mass is 0.41, 33 percent lower than the main bunching estimate (Figure 1).

Next, I determine the importance of the estimated underreporting at the micro level (for households) and at the macro level (for government revenue). For each household I add randomly drawn errors from the distribution of those with no misreporting incentives to the car value. I then multiply the difference between the noise-adjusted car values and self-reported assets—i.e., the evaded wealth—by the tax rate to obtain a measure of taxes saved. Panel B of Figure 4 shows evaded tax as a fraction of paid wealth tax for households who own at least one car and locate within an interval of 1 million SEK above the kink. Close to the threshold, taxpayers manage to reduce tax liabilities by around 70 percent of what they are liable to pay. The absolute value of cars is increasing in wealth, but the share of total wealth is decreasing, implying that the taxes lost relative to taxes paid is decreasing in wealth. Underreporting cars seems to have been an efficient method to evade the wealth tax for the moderately wealthy because cars represents an important share of wealth in that segment of the distribution (cars were taxable at 100 percent of their market values implying that car values as a fraction of total taxable wealth was around 10 percent with some variation over time) and potentially because they do not have access to the same avoidance/evasion technologies as the very wealthy (e.g., the ability to place wealth in tax havens).[20] Finally, I compute the total taxes lost to the government in panel C of Figure 4. The fraction of taxes lost ranges between 5.5 percent in 2000 to 4.5 percent in 2006. The fraction is decreasing over time because the upward threshold shifts nudged the composition of taxpayers toward wealthier households, for whom cars constitute a smaller share of wealth.

Reduced-Form Approach.—I complement the reporting analysis above with a reduced-form approach. I implement a difference-in-difference (DD) design that exploits shifts in the exemption threshold over time to investigate effects of the wealth tax on a set of variables that would shed light on the nature of responses: savings, realizations of capital gains and losses, the ratio of tax-to-market value wealth, taxable income, and car purchases.

[20] In robustness checks, panel A of online Appendix Figure E.1 shows that the fraction of households who own cars and who truthfully report them ranges between 15 and 25 percent over the SEK 1 million interval. Panel B restricts the analysis to new cars, suggesting that even households with a better understanding of the value of their cars do not report them truthfully.

I first construct an individual-level measure of financial savings as

$$s_{it} = \sum_{j=1}^{n} \left(p_{jt} q_{ijt} - R_{jt} p_{jt-1} q_{ijt-1}\right), \tag{8}$$

where R_{jt} is the total annual return on security j over year t, using shares, bonds, and funds.[21] Individuals' wealth fluctuations are either market-driven (changes in p_{jt}) or due to active rebalancing decisions by the individual (changes in q_{ijt}). By observing the exact composition of the portfolio security-by-security, equation (8) isolates the active component.

Figure 5 illustrates the DD estimator. For each year 2002–2006, panel A plots mean financial savings, assuming end-of-year rebalancing, for a treatment group and a control group.[22] Financial savings are expressed in market values and comprise active changes in stocks, funds, and bonds held by the individual. Restricting the analysis to couples, the data points indicated by squares depict households locating within 100,000 SEK above the threshold that was in place 2002–2004, while data points indicated by circles depict those locating within 100,000 SEK below the cutoff. The former group constitutes a treatment group, as their incentives to save changed between 2004 and 2005 when the threshold was raised, while the latter group is the control group.[23]

There is a trade-off in the choice of bandwidth: a narrow interval delivers treatment and control groups more similar to each other in terms of pre-trends, while a wider one provides a stronger treatment because wealth growth is volatile. To shed light on this issue, online Appendix Figure F.2 shows that both pre-trends and effects are robust to instead including households within 250,000 SEK on either side of the kink. Despite adding treatment intensity by widening the interval, estimated effects do not increase in magnitude. The reason is probably that the outcomes under study can easily be manipulated on short notice, such that even if there is noise in wealth growth, households could have optimized their decisions on December 31, after all noise has been realized and before the tax is determined.[24]

Panel A of Figure 5 shows no evidence of a treatment effect. Making the simplifying assumption that treatment effects are constant over time, I look for statistical support for this conjecture by estimating the following equation:

$$Y_{it} = \beta_0 + \beta_1 post_{it} + \beta_2 above_{it} + \beta_3 post_{it} \times above_{it} + \gamma \mathbf{X}_{it} + \epsilon_{it}, \tag{9}$$

[21] I exclude quoted options as they are hard to price and held by very few. I also exclude bank account holdings when computing savings and interest measures because of a change in how they were reported. Before 2006, bank account holdings were only reported if the interest payments exceeded SEK 100. For 2006 and 2007, all bank account holdings of SEK 10,000 or more were reported, leading to a surge in bank account holdings in the data.

[22] Equation (8) assumes that portfolios are rebalanced at the end of each year. I explore the robustness of this assumption by considering beginning-of-year rebalancing in online Appendix Figure F.1.

[23] There were a number of threshold shifts during the period under study, but I focus on 2005 for two reasons. Compared to other shifts, the pre- and post-reform subsamples implied by this shift are sufficiently long to enable a thorough assessment of the assumption of parallell trends. In addition, this threshold was located high up in the distribution, where one might expect responses to be stronger because households hold more liquid assets compared to the lower part of the distribution (where real estate is relatively more important).

[24] The fraction of households locating within 250,000 SEK below the thresholds but end up above the threshold after one year is 30 percent.

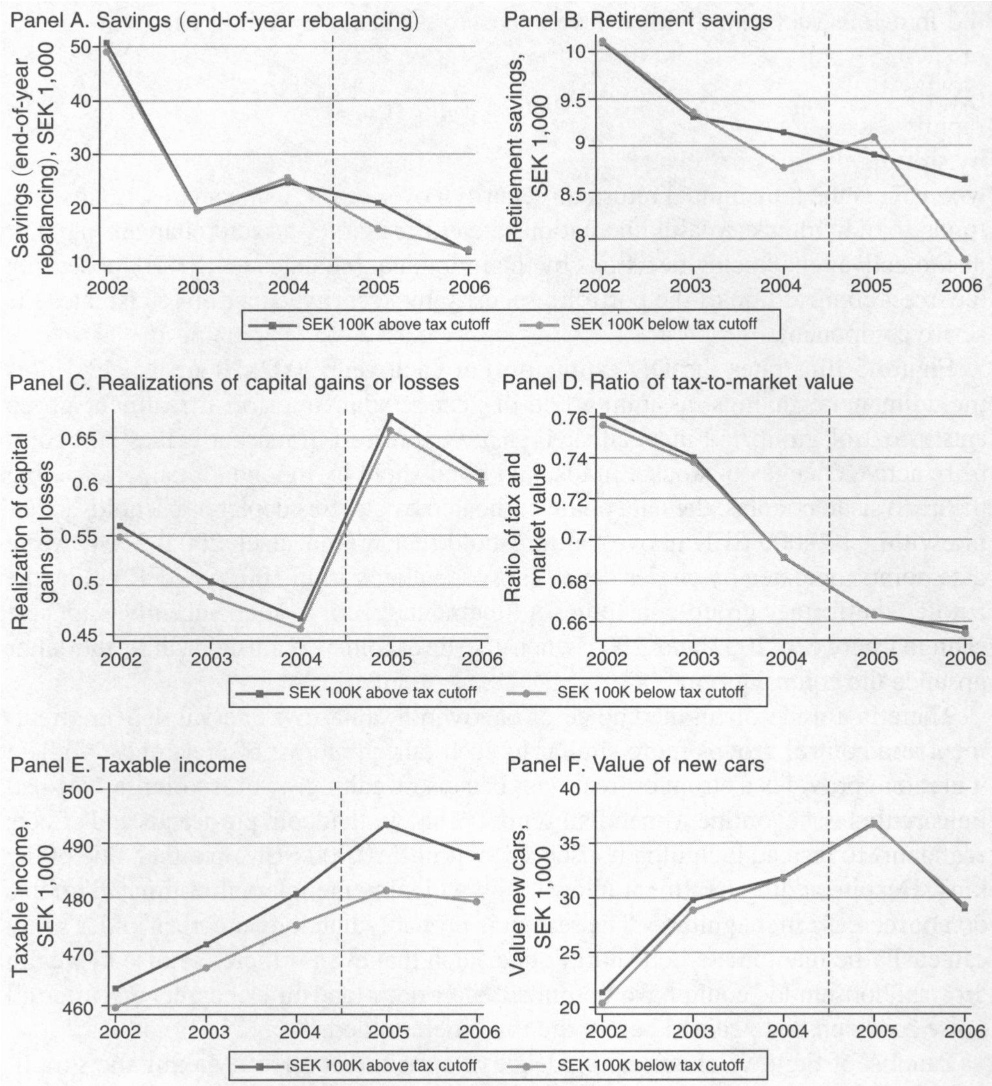

Figure 5. Effects of the Wealth Tax on Various Outcomes

Notes: This figure shows outcome variables for couples divided into two groups year by year: those with wealth within SEK 100,000 above the threshold that was in place during 2002–2004 (treatment group) and those with wealth within SEK 100,000 below (control group). Panel A shows active changes in financial wealth (stocks, funds, and bonds), while panel B and C display retirement savings and the fraction of households who realize capital gains or losses over time. Panel D shows the average ratio of tax value of assets to market values, and panels E and F consider taxable income and the value of new cars, respectively.

where Y_{it} denotes the various outcomes of household i during year t, $post_{it} = \mathbf{1}(t \geq 2005)$ is an indicator for the years after the reform, $above_{it} = \mathbf{1}(W_{it} > W^*_{2002})$ is an indicator for having taxable wealth above the threshold that was in place 2002–2004, and $\mathbf{X}_{it}$ is a vector of potential control variables.

Ex ante, it is not clear if the reform would increase or decrease financial savings. The wealth tax lowers the return on financial savings since accumulated assets are taxable. One should thus expect increased savings for the treatment group when the threshold is shifted upwards. On the other hand, a dynamic life-cycle model of savings (presented in online Appendix Section G) shows that households spend less time above the kink because of the tax. Moreover, wealth is accumulated more rapidly above the kink compared to underneath it. An agent striving to avoid the tax by shifting wealth from taxable financial assets to pension wealth would conversely lower financial savings in response to the tax.

Column 1 of Table 4 suggests that when assuming that savings are rebalanced at the end of the year, the threshold shift increases savings by SEK 2,091 or by 8 percent of mean savings in 2004 for the treatment group, but the effect is not statistically significantly different from zero.[25] When assuming beginning-of-year rebalancing, the effect is even more imprecisely estimated. The results are unaffected by including controls. Results are also insensitive to both widening the interval of the treatment and control groups and excluding those who locate near the kink because of bunching.

Since accumulated retirement savings were tax exempt, pension savings were likely to be an attractive investment opportunity under the wealth tax. Because of the tax exemption, pension wealth is unobservable, but since contributions are income tax deductible, I do observe annual transfers. Panel B of Figure 5 shows mean savings in retirement accounts over time for the treatment and control groups. The absence of responses is corroborated by the econometric results in row 3 of Table 4.

Not all decisions on wealth accumulation involve financial savings. Households may respond to the wealth tax by selling off illiquid assets, such as real estate, to a greater extent than they would in the absence of a tax. Such responses would not be captured through financial savings, but would be detectable when looking at the realization of capital gains and losses. Panel C of Figure 5 shows the fraction of couples who realize gains or losses over time for treatment and control groups defined as above. Despite an increasing trend before 2005, there is no sign of a treatment effect. This is confirmed statistically in Table 4.

I next look for avoidance by comparing the wealth-tax value of assets to their market values over time. The basic idea is that households with a ratio of tax-to-market value close to 1 mainly hold assets that are taxed at 100 percent of their market value, thus indicating no tax avoidance, while a low ratio is consistent with avoidance by means of investments in assets taxed at a lower rate. Panel D of Figure 5 shows that the tax-to-market value is declining over time but there are no differences between the treatment and control groups. This is also corroborated by the regression results.

If households work to finance future consumption through wealth accumulation, taxing wealth is equivalent to taxing future consumption, which weakens the incentives to earn labor income. Panel E of Figure 5 suggests a small, negative effect of the wealth tax on taxable income. The corresponding positive point estimate is confirmed in Table 4, but is not statistically significant.

To further explore whether responses were real or reporting in nature, I consider the possibility that households may have exploited the part of the tax base that was self-reported. It is easier to enforce a tax base reported by third parties than

[25] This is equivalent to an increase in the saving rate (relative to income) of 0.7 percentage points, from 8.6 percent to 9.3 percent.

TABLE 4—RESPONSES TO THRESHOLD SHIFTS

	Above cutoff × post	
Interval:	Within 100 kSEK (1)	Within 250 kSEK (2)
Savings (end)	2,090.602 (1,727.772)	1,060.535 (977.406)
Savings (beginning)	1,040.628 (2,461.803)	3,962.902 (1,820.482)
Retirement savings	276.135 (253.228)	188.182 (145.196)
Realizations	−0.001 (0.008)	−0.000 (0.004)
Ratio of tax-to-market value	0.000 (0.002)	−0.002 (0.001)
Taxable income	5,276.845 (4,102.870)	−1,820.081 (2,763.609)
Car value	−2,440.356 (1,141.491)	−3,803.881 (930.034)
New car value	−943.619 (905.802)	−2,623.846 (804.247)
Min observations	66,652	168,576

Notes: This table shows estimates of equation (9) where the sample includes couples over the years 2002–2006. All specifications control for education (six dummies), age, and county fixed effects. Standard errors are clustered at kSEK five intervals. Observations differ slightly across specifications, which is why I report the minimum number of observations.

a self-reported one, providing an opportunity to cheat (Kleven et al. 2011). With access to the car registry, the most important component of non-third-party-reported assets, I investigate whether the wealth tax deterred households from buying more or expensive cars, or if households increased their car holdings because that source of wealth was easy to evade.

Panel F of Figure 5 shows the mean value of newly purchased cars for the treatment and control groups, defined as above. The two groups' car holdings are strikingly similar. The DD estimate is negative but insignificant.

The results from these program-evaluation approaches thus suggest no statistically significant real responses to the wealth tax.

Auxiliary Evidence.—The excess mass at the wealth-tax threshold (Figure 1) is consistent with both real and reporting responses. In yet another attempt to detect real responses, I study households with limited ability to self-report. Online Appendix Figure C.5 shows third-party-reported and taxable wealth for households who do not own a car. Although cars are not the only component of non-third-party reported wealth, it is, as discussed above, the most important one. The only way that these households can respond to the tax is essentially by changing their saving behavior in terms of third-party reported wealth adjustments. Real responses to the wealth tax within this group would thus most likely materialize as excess mass in the distribution of third-party reported wealth. However, the figure shows a similar pattern as for the population as a whole, which is consistent with no real responses within this subgroup. Moreover, the documented difference in bunching suggests that these households find other ways to reduce tax liability.

To summarize, responses appear to occur on the self-reported margin and are likely to represent tax evasion. I find no evidence of households changing their saving or their portfolio composition as a response to the wealth tax around the tax threshold. However, more than half of the bunching responses disappear when I correct household's self-reported assets by their car holdings. The lack of responses on the savings margin and the existence of reporting responses is consistent with the hierarchy of Slemrod (1990, 1995): since avoidance and evasion opportunities were abundant, the tax did not compel households to change their saving behavior.

V. Conclusion

Rising inequality has led some academics and policymakers to advocate redistribution by means of progressive wealth taxation. This paper adds empirical evidence to the debate.

I estimate net-of-tax-rate elasticities of taxable wealth in the range [0.09, 0.27] and find that these small, but positive, elasticities appear to reflect tax avoidance and evasion. The findings are consistent with the hierarchy of behavioral tax responses proposed by Slemrod (1990, 1995), conjecturing that households will respond in real terms only as a measure of last resort, should avoidance or evasion opportunities prove limited.

The small behavioral effects and lack of real responses to the wealth tax that I find imply that wealth taxation may be an efficient redistributional tool. A comparison to income taxation within the framework proposed by Hendren (2014)—presented in online Appendix Section H—suggests that wealth taxation can be a more efficient way to redistribute resources compared to income taxation if the behavioral responses to wealth taxes are smaller than those of income taxes. This is true in the paper at hand because wealth taxpayers tend to locate in the upper end of the income distribution, where taxable income elasticities are largest. Such a suggestive normative exercise, comparing the welfare effects of wealth taxes to those of progressive income taxes, suggests that the social surplus is 24 percent higher when redistribution is accomplished by wealth taxation. I am not able to detect the strategic placement of assets abroad due to the wealth tax and future work should seek to investigate such shifting.

On a final note, the results suggest that the behavioral effects of the wealth tax were small but primarily reflecting evasion. This indicates that the Swedish wealth tax was plagued by loopholes and insufficient enforcement. More research is therefore needed on the effects of wealth taxation in settings with limited avoidance and evasion opportunities. In accordance with Slemrod's hierarchy of responses, real effects of the wealth tax cannot be ruled out in an environment of stricter enforcement, less reliance on self-reports, and a more homogenous treatment of taxable assets.

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