Borrowing, Avoidance, and Tax Base Design: Evidence from Norwegian Shareholder Loans

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When governments shut down legal methods of tax avoidance, taxpayers may substitute toward taxable income—or toward other avoidance margins. We study an overnight reform in October 2015 that reclassified loans from Norwegian closely held corporations to their personal owners as taxable dividends, effectively shutting down a widespread channel through which wealthy owner-managers accessed corporate cash without triggering shareholder-level taxation. Using linked administrative data on household finances, firm balance sheets, and creditor–borrower relationships, we first document that such loans were prevalent, carried interest rates well below those on other personal debt, and were concentrated among the wealthiest households. Exploiting the reform in a difference-in-differences design, we show that closing this channel induces substantial substitution toward taxable payouts: treated owners increase equity-related taxable income by about $32,000 per year and tax payments by roughly $11,000. However, treated owners also partially substitute toward third-party borrowing, increasing external debt and expanding the number of distinct creditor relationships. The reform thus partially broadens the tax base but alternative sources of untaxed liquidity remain available.

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1 Introduction

A foundational insight of modern public finance is that behavioral responses to taxation depend on the avoidance channels available to taxpayers. Despite the theoretical importance of this idea, direct causal evidence on what happens when a specific avoidance channel is closed remains limited. A large literature decomposes behavioral responses to tax rate changes into avoidance and real components, finding that income shifting and retiming often dominate real labor supply adjustments.[1] A separate strand studies whether enforcement increases cause substitution from evasion to legal avoidance, generally finding limited substitution (Alstadsæter et al., 2022; Fack and Landais, 2016). But a distinct question—what happens when a specific legal avoidance technology is eliminated—has received far less empirical attention. This is the question most directly relevant to Slemrod and Kopczuk (2002): if the elasticity of taxable income depends on the available avoidance channels, then removing a channel should reduce the elasticity and broaden the tax base. Yet it could also push taxpayers toward other avoidance technologies, partially or fully offsetting the revenue gain.[2] Whether avoidance channel closure broadens the tax base or merely reshuffles avoidance is ultimately an empirical question—and one for which evidence on personal (as opposed to corporate) avoidance is especially scarce.

This paper provides such evidence. We study the elimination of a specific channel through which wealthy owner-managers of closely held corporations in Norway used borrowing to avoid shareholder-level taxation. Prior to October 2015, firm owners could borrow from their own businesses without triggering personal taxation—a simple and cheap way to extract corporate cash while deferring taxable realizations. With effect from October 7, 2015, the Norwegian government reclassified shareholder loans as deemed dividends, effectively

[1] See, among others, Harju and Matikka (2016) for Finland, Alstadsæter and Jacob (2016) for Sweden, Kreiner et al. (2016) for Denmark, and Goolsbee (2000) for the United States.

[2] In the corporate tax literature, several papers document such substitution: Carrillo et al. (2017) find near-complete margin substitution among Ecuadorian firms; Johannesen and Zucman (2014) show geographic relocation of offshore deposits in response to information-exchange treaties; Bilicka et al. (2022) document both financial and real substitution after the UK limited multinational debt shifting.

shutting down the tax advantages of this scheme overnight. We exploit this reform in difference-in-differences and event-study designs, using comprehensive administrative data that links household income, wealth, and liabilities to firm ownership and firm-side balance sheets.

Our setting also speaks to a question of growing policy interest: whether restricting debt-based liquidity can raise tax revenue from the top of the wealth distribution. In the United States, the proposed "ROBINHOOD Act" (December 2025) would impose tax penalties on certain asset-backed loans, motivated by concerns that wealthy households use borrowing to finance consumption while deferring taxable realizations—the "borrow" component of so-called "buy, borrow, die" strategies. Fox and Liscow (2025) document the joint role of unrealized gains and borrowing among wealthy U.S. households using survey data, but note that survey data cannot capture the extreme top tail or identify behavioral responses to policy changes. We provide the first causal evidence on what happens when a specific borrowing-based avoidance channel is shut down: do taxpayers substitute to taxable realizations (broadening the tax base) or to other sources of or forms of avoidance?

Our analysis delivers two sets of results: **(1) Descriptive evidence on debt, liquidity, and owner–firm borrowing.** We first document the prevalence of shareholder loans, and contrast these loans to other types of personal borrowing across the wealth distribution. Total personal debt rises steeply with wealth: the top 1% of the household wealth distribution in Norway carries average debt on the order of $1 million, several times larger than households further down the distribution.[3] Shareholder loans are also larger at the top: the 1% of the wealth distribution (unconditional on firm ownership) owes $200,000 in shareholder loans immediately prior to our reform. Borrowing costs on total debt decline with wealth: richer households face systematically lower effective interest rates on their personal debt. Even compared to the average interest rates faced by top households, the effective interest rate

[3]Debt at the very top is extremely heterogeneous: within the top 1%, a non-trivial share reports essentially zero personal debt, while the upper decile holds liabilities an order of magnitude larger than the median.

on firm-to-owner borrowing is low—around 2%—making it a cheap and scalable liquidity source. This discrepancy is in spite of the fact that shareholder loans (even prior to the reform) were meant to be made only at market interest rates.

(2) Treatment effects: closing the shareholder-loan channel induces substitution toward taxable realizations. We exploit the October 2015 reform as a quasi-experimental closure of the shareholder-loan channel. The central behavioral response is a pronounced substitution toward taxable dividends: treated owners increase dividends by approximately $35,000 per year and total household tax payments by about $11,000 per year. Event-study estimates show minimal evidence of differential pre-trends and reveal a sharp, persistent post-reform increase in both outcomes.

In contrast to the sharpness of our results on dividends and tax payments, we do not find evidence of a contraction in household consumption. This null result is consistent with wealthy owners maintaining spending by altering how they obtain liquidity rather than whether they spend. On other margins, we find that treated owners increase third party (i.e., non-shareholder) debt by roughly $30,000 following the reform and expand the number of distinct creditor relationships, consistent with partial substitution toward external borrowing. The interest rate that treated owners pay for this new debt does not appear to differ from interest rates on external debt taken out prior to the reform.

Taken together, the results show that closing this avoidance channel partially broadens the tax base: taxable realizations and tax payments rise substantially, but substitution toward external borrowing is also substantial. The channel closure thus reduces the elasticity of taxable income—confirming the Slemrod and Kopczuk (2002) mechanism—but does not eliminate all avoidance, because alternative liquidity technologies remain available.

1.1 Related literature

Our paper contributes to several literatures.

The elasticity of taxable income and tax base design. Slemrod and Kopczuk (2002) show theoretically that the ETI depends on the breadth of the tax base and the set of available avoidance channels, implying that the government can influence the elasticity through base-broadening reforms. Kopczuk (2005) provides empirical support: using two U.S. reforms in the 1980s, he shows that the elasticity of reported income depends on the available deductions, and that TRA86's base broadening reduced the measured ETI. Our paper complements this work by studying a discrete channel closure rather than a broad base-broadening reform, which allows us to trace the full anatomy of behavioral responses—including substitution to alternative avoidance margins—with a cleaner identification strategy. More broadly, our decomposition of responses into real adjustment (consumption), taxable-income substitution (dividends), and avoidance-margin substitution (external borrowing) contributes to the literature on the "anatomy" of behavioral responses to taxation.

Avoidance-channel substitution. A growing literature, primarily in corporate and international taxation, studies whether increases in tax enforcement pushes behavior to other channels. Carrillo et al. (2017) find near-complete substitution among Ecuadorian firms: when notified of revenue discrepancies, firms offset 96 cents per dollar by inflating reported costs. Johannesen and Zucman (2014) document geographic relocation of offshore deposits after bilateral information-exchange treaties. Bilicka et al. (2022) show that UK limits on multinational debt shifting reduced targeted debt but triggered intracorporate reallocation of debt and real activity. In the personal tax domain, Alstadsæter et al. (2022) find that wealthy Norwegian taxpayers who stopped evading via offshore accounts did not increase legal avoidance. Our paper extends this literature to a setting where a specific legal avoidance technology (rather than an evasion channel) is eliminated for individual taxpayers, and finds an intermediate result: substitution is substantial but incomplete, so that meaningful revenue gains survive.

Closely held corporations and income shifting. A large literature documents how owner-managers of closely held corporations exploit the form and timing of payouts to reduce tax liabilities. Alstadsæter et al. (2014) show that Norwegian closely-held companies function as tax shelters to retain earnings; Alstadsæter and Jacob (2016) and Harju and Matikka (2016) document wage-dividend income shifting in Sweden and Finland. For listed U.S. firms, Chetty and Saez (2005) find large payout responses to the 2003 dividend tax cut, while Yagan (2015) finds little effect on real investment or worker earnings, and Alstadsæter et al. (2017) show that dividend taxes can affect investment allocation across firms with different liquidity positions. We contribute by studying a previously undocumented avoidance margin—owner–firm lending as a substitute for taxable dividends—and by providing quasi-experimental evidence on the behavioral response to its elimination.

Household debt and liquidity among the wealthy. Our descriptive analysis connects to work documenting the financial positions of wealthy households. Bach et al. (2020) show substantial heterogeneity in household leverage across the Swedish wealth distribution; Fagereng et al. (2020) document heterogeneous and persistent returns to wealth in Norway; and Kaplan et al. (2014) emphasize that high net worth can coexist with low liquid wealth, making liquidity—not just wealth—central for spending behavior and for identifying demand for credit-based liquidity technologies.

2 Institutional Background and Data

2.1 Norwegian tax system and the 2015 reform

Norway taxes personal shareholders on distributed corporate resources. Dividends and dividend-like transfers are included in the personal tax base when they are received, subject to a rate-of-return allowance (the shielding deduction, *skjermingsfradrag*). This realization-

based structure makes both the timing and form of payouts from the firm central to personal shareholder taxation.

In Norway, as in many countries, borrowing is not typically subject to tax.[4] Prior to October 2015, loans from a firm to that firm's shareholder were generally treated as ordinary debt for tax purposes, meaning that shareholder-firm borrowing was not a taxable event. This created a tax wedge for owners of closely held firms: if they extracted liquidity from their firm using dividends or by selling shares, they would face dividend or capital gains taxation. But borrowing from the firm (provided that the arrangement was structured as a genuine loan) would not trigger any tax at the time of the cash withdrawal.[5] This created a liquidity technology that allowed owners to circumvent (or at least defer) shareholder-level taxation: they could access corporate cash for private purposes without triggering taxation, and potentially repay later out of future income or asset sales. Additionally, interest payments on shareholder loans (as with ordinary loans) was deductible for the shareholder and taxable for the lending company.

We study an October 2015 reform that changed the tax treatment of shareholder loans. Any extension of credit from a firm to that firm's personal shareholders after October 6, 2015 resulted in dividend taxation for the shareholder.[6] Pre-existing loans taken up before the reform were not automatically reclassified. However, increases in an old loan's balance after the reform date are treated like new credit and therefore trigger dividend taxation (Skatteetaten, 2025). The 2015 reform also changed the treatment for repayments of shareholder loans. Repayments of the shareholder loans taxed as dividends are now treated as new con-

[4] As a general tax principle, the act of borrowing or repaying a loan is a non-event for income tax purposes; borrowed funds are not considered income because it is assumed the funds will be repaid.

[5] Even prior to October 2015, the Norwegian tax authorities could reclassify shareholder loans as dividends if the following conditions were not met: there needed to be a real obligation for the shareholder to repay, the interest rate should be set at market terms and the shareholder should provide satisfactory security (Finansdepartementet, 2015). But this reclassification would have only been done on an ad-hoc basis and we find evidence that the interest rates on shareholder loans were systematically low.

[6] Exceptions were made for minority shareholders in financial institutions (i.e., for banks the extend loans to individuals in their regular course of business). Loans to the close relatives of shareholders are also included.

tributions of capital by the shareholder, which increase the shareholder's basis in his shares and ensure the repayment will not be taxed again if the shareholder sells his position. This treatment is symmetric to how the tax system would treat a dividend followed by a later injection of new capital.

The shareholder loan reform took effect on the date of its announcement in the 2016 budget proposal on October 7, 2015 (Finansdepartementet, 2015). This timing was designed to limit the scope for anticipatory behavior, as there was no official indication that the reform was in the works before this date. However, highly interested observers may have had hints that the Norwegian government was concerned about the use of shareholder loans as a substitute for dividends in late 2014 (Norges offentlige utredninger, 2014).[7] In December 2014, a government commission on broader reforms to capital taxation (the Scheel Commission, NOU 2014:13) included a proposal to tax all shareholder loans as dividends (Norges offentlige utredninger, 2014).

The implementation date (7 October 2015) created a sharp discontinuity in the effective tax price of obtaining liquidity from one's own closely held firm through borrowing. The reform provides a quasi-experimental setting to study (i) the role of shareholder loans as a pre-reform liquidity channel and (ii) substitution toward dividends, other borrowing instruments, or alternative tax-planning margins once this channel became substantially more expensive. In the following section, we describe the administrative data we use to study this reform.

2.2 Data sources

The analysis combines several administrative data sets in order to link individual shareholders to firm balance sheets:

- **Household income, wealth and debt:** Our primary data on individuals and house-

[7] Government reports from 2014 emphasize that the stock of loans from companies to personal shareholders had increased sharply over time and that distinguishing genuine loans from dividend-like withdrawals can be difficult in practice, especially among closely held firms.

holds comes from their yearly tax filings. The personal income tax return collects detailed information (mostly third-party reported) on wage income, capital gains income and interest paid. Household-level wealth tax filings allow us to construct a household's balance sheet: important variables include total personal debt, total net wealth and wealth in particular asset classes including stocks and liquid assets. Following Eika et al. (2020), we also use the household balance sheets to construct an implied measure of yearly household consumption equal to the difference between a household's income streams and their year-to-year change in net wealth.

- **Corporate tax returns:** Our primary firm-level data comes from the corporate tax return filed on a yearly basis by closely held corporations (form RF-1028). Key fields in this data include the aggregate amount of outstanding loans from the firm to its owners and loans from owners to the firm, as well as special fields introduced in 2016 capturing the stock of owner loans and related collateral as of October 6, 2015.

- **Firm-Shareholder registry:** We use the shareholder registry to match closely held firms to their shareholders, determine individual ownership shares, and link between individuals and firms.

- **Detailed external debt data:** From 2011, we have detailed annual information on debtor–creditor relationships reported by banks and financial institutions to the Norwegian Tax Administration. Variables include outstanding debt amounts and interest payments. This data covers debt relationships where the lender is a reporting financial institution, including typical products like mortgages and consumer credit. It does not capture informal private loans or debt outside the financial sector (i.e., it notably excludes almost all shareholder loans). We use this data to examine substitution patterns towards more formal lending relationships after the shareholder loan reform.

3 Descriptive Evidence on Household Debt, Liquidity, and Shareholder Loans

This section documents descriptive facts about indebtedness and liquidity among wealthy households. We first characterize how the level and distribution of personal debt vary across the wealth distribution and how borrowing costs differ by wealth. We then zoom in on loans between closely held corporations and their owners ("shareholder loans"), which provide a distinct (and low-cost) liquidity channel for business owners.

Measurement and scope. Throughout this section, "personal debt" refers to total liabilities reported on the personal tax return (including mortgages and other personal borrowing). These measures do not include borrowing undertaken inside businesses that households own unless the business is a sole proprietorship.[8]

Debt levels across the wealth distribution. Figure 1 Panel (a) shows that wealthy households carry substantially more debt in levels than the rest of the population. The top 1% stands out: average debt is on the order of $1 million over the sample period, compared to roughly $0.5 million in the top 2–5% and about $200,000–300,000 for the groups below. Outside the very top, average debt levels are much closer together, consistent with household debt (e.g. mortgage debt) being widespread throughout the population.

Average Borrowing costs decline with wealth. Figure 2 shows that richer households face lower effective interest rates on their personal debt. The top 1% consistently pays the lowest average rate, while the bottom 90% pays the highest. The level differences are economically meaningful and persist across years, even as average rates vary over the business cycle.

Owner–firm loans carry low interest rates. Figure 3 shows that effective interest

[8]Norway does not have pass-through business entities other than sole proprietorships.

rates on firm-to-owner loans are low (around 2–3% in most years) and are generally below the effective interest rates on household personal debt in Figure 2. This is consistent with shareholder loans being a low-cost internal borrowing channel for owners. Furthermore, Figure 3 also shows how the debt-weighted interest rate is higher than the unweighted rate.

Shareholder loan volume and prevalence. We next document borrowing between closely held corporations and their owners using the RF-1028 corporate tax returns. Figure 4 Panel (a) shows that total loans from firms to owners are economically large, on the order of tens of billions of NOK per year. Figure 4 Panel (b) shows that such loans are also widespread: in the early 2010s roughly one in ten closely held firms report a positive loan to owners. Conditional on having a positive loan, Figure 4 Panel (c) shows that loan sizes are highly skewed: the median loan is relatively small, while the 90th percentile reaches a quarter of a million USD.

4 Empirical Strategy

We exploit the 2015 reform as a quasi–experimental shock to the cost of using shareholder debt as a source of personal liquidity. We compare outcomes for closely-held firm owners who engaged in shareholder borrowing before the reform to other individuals who own closely-held firms that did not engage in pre-reform lending to shareholders.

4.1 Treatment Definition and Samples

We construct treatment indicators by combining administrative ownership records with firm balance–sheet information just before the reform. First, we use shareholder registers (the `aksjonar` files) and firm accounts (`areg`) to compute person–firm ownership shares at the end of 2014. For each person–firm pair, we calculate an ownership share share_{if} for individual i in firm f in 2014. Second, we use the corporate tax returns for privately-held businesses

`rf1028` to measure loans from the firm to its owners. Post-reform, these filings include a question about the firm's amount of shareholder loans outstanding on 6 October 2015.

We limit ourselves to the population of *majority owners of private firms*, which we define as all individuals who own at least 50% of a private firm in 2014. For each majority owner i, we then define total pre–reform shareholder debt as

$$\text{DebtPre}_i \;=\; \sum_{f \in \mathcal{F}_i} \text{debt_pre}_f \times \text{share}_{if},$$

where $\mathcal{F}_i$ is the set of majority–owned firms and debt_pre_f is the firm's shareholder loan balance on 6 October 2015.

Our baseline treatment indicator (at the individual level) is

$$\text{Treat}_i \;=\; \mathbf{1}\{\text{DebtPre}_i > \bar{D}\},$$

where $\bar{D}$ is a cutoff. For clearest economic interpretability, we set $\bar{D} = 0$, which means we contrast majority owners of firms with any shareholder loans to majority owners of firms with no shareholder loans. This specification allows us to run difference-in-difference regressions with binary treatment that (under a standard parallel trends assumption) identify the average treatment on the treated for the population with $\bar{D} > 0$ (Callaway et al., 2025). However, our results are also robust to setting $\bar{D}$ to NOK 100,000 ($\approx$ \$11,600) in order to limit the sample to economically meaningful shareholder loans.

Outcomes from the Third-Party Debt Data Figure 7 and Table 2 use outcomes constructed from the borrower–creditor register , which is observed at the borrower–creditor–year level. We aggregate these data to a person–year panel by summing across all creditors c observed for individual i in year t. Specifically, we construct total outstanding debt $D_{it} = \sum_c \text{Debt}_{ict}$, total interest payments $I_{it} = \sum_c \text{Interest}_{ict}$, and the number of distinct creditors

N_{it} (the count of unique creditors observed for i in year t). Our main debt outcomes are: (i) total debt D_{it} (and $\log(1 + D_{it})$), (ii) the number of creditors N_{it}, (iii) an implied interest rate on the debt stock $r_{it} = I_{it}/D_{it}$, and (iv) an implied interest rate on newly established creditor relationships, $r_{it}^{\text{new}} = I_{it}^{\text{new}}/D_{it}^{\text{new}}$, where I_{it}^{new} and D_{it}^{new} sum interest and debt over creditors that are observed for the first time for individual i in year t. To reduce sensitivity to extreme ratios, we set the small number of observations with implied interest rates that are negative or exceed 25% to missing.

4.2 Difference-in-Differences Specification

Our main empirical specification is a two–way fixed–effects difference–in–differences (DiD) design that compares treated and control units before and after the 2015 reform.

For individual- or household-level outcome y_{it}, we estimate

$$y_{it} \;=\; \alpha_i \;+\; \lambda_t \;+\; \beta\left(\text{Treat}_i \times \text{Post}_t\right) \;+\; X_i'\gamma \;+\; \varepsilon_{it}, \tag{1}$$

where α_i are individual (or household) fixed effects, λ_t are year fixed effects, Post_t is an indicator for the post–reform period, and X_i is a vector of time–invariant baseline controls measured in 2014 (in our baseline specifications, log wealth and leverage). The coefficient of interest, β, captures the average change in the outcome for treated individuals relative to control individuals after the reform.

Estimation and inference. All our regressions are estimated with including unit and year fixed effects . Standard errors are clustered at the relevant unit level to allow for arbitrary serial correlation within units. Because baseline controls are time-invariant, they are absorbed by unit fixed effects in fixed-effects specifications; in practice, "FE + controls" specifications correspond to estimating the fixed-effects model on the subsample with non-missing baseline covariates.

In practice we treat 2014 as the last fully pre–reform year and consider 2015 onwards as the post–reform period. The simple two–period DiD estimates are complemented by more flexible dynamic specifications described below.

4.3 Event-Study Specification

To study the timing and dynamics of behavioral responses, and to assess the plausibility of the parallel–trends assumption, we estimate an event–study version of the DiD specification. For each outcome we regress

$$y_{it} \;=\; \alpha_i \;+\; \lambda_t \;+\; \sum_{k \neq -1} \beta_k \left(\text{Treat}_i \times 1\{t - t^\star = k\} \right) \;+\; X_i' \gamma \;+\; \varepsilon_{it}, \tag{2}$$

where $t^\star$ is the first post–reform year, k indexes event time in years relative to the reform, and $k = -1$ (the last pre–reform year, 2014) is omitted as the reference period. The coefficients β_k therefore measure the difference in outcomes between treated and control individuals in year t, relative to the corresponding difference in 2014.

The pre–reform coefficients ($k < 0$) provide a visual diagnostic of the parallel–trends assumption: if treated and control units evolved similarly before the reform, these coefficients should be close to zero and statistically insignificant. The post–reform coefficients ($k \geq 0$) show the time profile of the treatment effect and allow us to distinguish one–off level shifts from gradual adjustments or delayed responses.

4.4 Identification and Threats to Validity

Identification in our DiD framework relies on a standard parallel–trends assumption: in the absence of the 2015 reform, outcomes for treated and control units would have evolved in parallel. Because treatment status is defined using pre–reform shareholder loans measured at a single point in time (6 October 2015) and based on ownership as of 2014, treatment is

predetermined with respect to post–reform shocks in our outcomes. The reform itself was implemented broadly and at a sharp date, reducing concerns about selective timing.

Several features of the design strengthen the plausibility of the identifying assumptions:

1. We compare treated majority owners to other majority owners with similar firms but no pre–reform shareholder loans, rather than to the full population. This ensures that both groups have similar access to corporate saving vehicles and differ primarily in their pre–reform use of the loan channel.

2. We include individual (or firm) fixed effects and year fixed effects, which absorb all time–invariant differences across units and common macroeconomic shocks.

3. Event–study plots generally show no evidence of differential pre–trends for the main outcomes, while they reveal sharp and persistent changes following the reform for outcomes closely linked to shareholder borrowing.

5 Treatment Effects of the 2015 Reform

Figure 5 plots the evolution of mean personal debt for the treated and control groups of majority owners. Treated owners (those with substantial pre-reform exposure to loans from their own firms) have higher average debt levels even excluding the portion of their debt attributable to shareholder loans. Despite this difference in base levels, the treatment and control groups have parallel trends in their levels of non-shareholder debt. The groups have diverging levels of total debt because of the treatment groups' increasing use of shareholder loans between 2010 and 2015 (Figure 5).

5.1 Household responses: substitution from shareholder borrowing to taxable capital gains income

The central household response is a sharp increase in taxable dividends following the reform. In our preferred fixed-effects specifications, treated owners increase dividends (hh_aksje) by roughly \$32,000-35,000 per year relative to the control group (Table 1). The event study in Figure 6 shows no evidence of differential pre-trends and a discrete, persistent post-reform increase, consistent with the reform closing the shareholder-loan channel and inducing owners to extract liquidity through taxable payouts/realizations instead.

This substitution has clear fiscal consequences. Household tax payments rise by about \$11,000 per treated owner-year in the post period (Table 1), with a similarly sharp and persistent pattern in the corresponding event study (Figure 6). Taken together, these estimates imply that closing the shareholder-loan channel translates into economically meaningful increases in taxable capital gains income and government revenue among highly exposed owners.

We find little evidence of a contraction in household consumption. The event-study for total consumption (Figure 6) is close to zero in the post-reform period, suggesting that the reform primarily changes the *mode* of financing liquidity (loan versus taxable payout) rather than reducing spending.

5.2 External Debt and credit-market adjustment

A natural question is whether treated owners respond to the closing of the shareholder-loan channel by (i) reducing overall leverage, (ii) substituting into other borrowing relationships, and/or (iii) facing worse borrowing terms. To study this, we use creditor–borrower matched debt data for all formal loans to construct an individual-by-year panel (2011–2018) with four main outcomes: total outstanding debt from third parties, the number of distinct external

creditors, the average interest rate on the outstanding external debt stock, and the interest rate on *new* creditor relationships (defined as relationships that appear for the first time in the external debt data for a given person in a given year).[9]

External debt increases for treated owners. Table 2 shows that treated owners increase external debt relative to controls in the post period. In our preferred fixed-effects specification (individual and year fixed effects, restricting to individuals with baseline covariates available), treated owners hold about 355,000 NOK more external debt in the post-reform period. The increase in total debt is accompanied by an increase in the number of creditors. Table 2 shows that treated owners add about 0.16 additional creditors on average in the post period (relative to the control group). Figure 7 Panel (a) confirms the timing: the treated–control difference is small in the last pre-reform years, and rises steadily after 2015 (about +0.11 creditors in 2015, increasing to roughly +0.25 by 2017, relative to 2014). Together, the debt and creditor-count results suggest that the reform led treated owners to substitute away from the shareholder-loan channel by expanding borrowing relationships rather than deleveraging. We next ask whether substitution into new borrowing relationships comes with higher interest rates (compared to other third-party loans). Table 2 Row 3 shows no evidence of higher interest rates on newly formed creditor relationships for treated owners: the estimate is economically small and statistically indistinguishable from zero. The next row of Table 2 reports the same exercise for the average interest rate on the outstanding debt stock. While interest rates fall markedly over time in the post period for both groups, the estimated treated–control differential is small relative to the common time trend. These estimates speak to whether treated owners pay higher market interest rates than comparable control owners on external borrowing after the reform. They do not rule out that shifting from low-rate shareholder loans to market-based borrowing increases owners' absolute

[9] All interest-rate outcomes are constructed as interest payments divided by debt amounts and are trimmed to remove implausible values. The "new-creditor" interest rate is debt-weighted within the set of creditor relationships that first appear in that year, and should be interpreted as a proxy for borrowing costs on newly formed relationships. Since the reform takes effect late in 2015 and the data are annual, the 2015 "post" year likely mixes pre- and post-reform months, making the first post-year effect conservative.

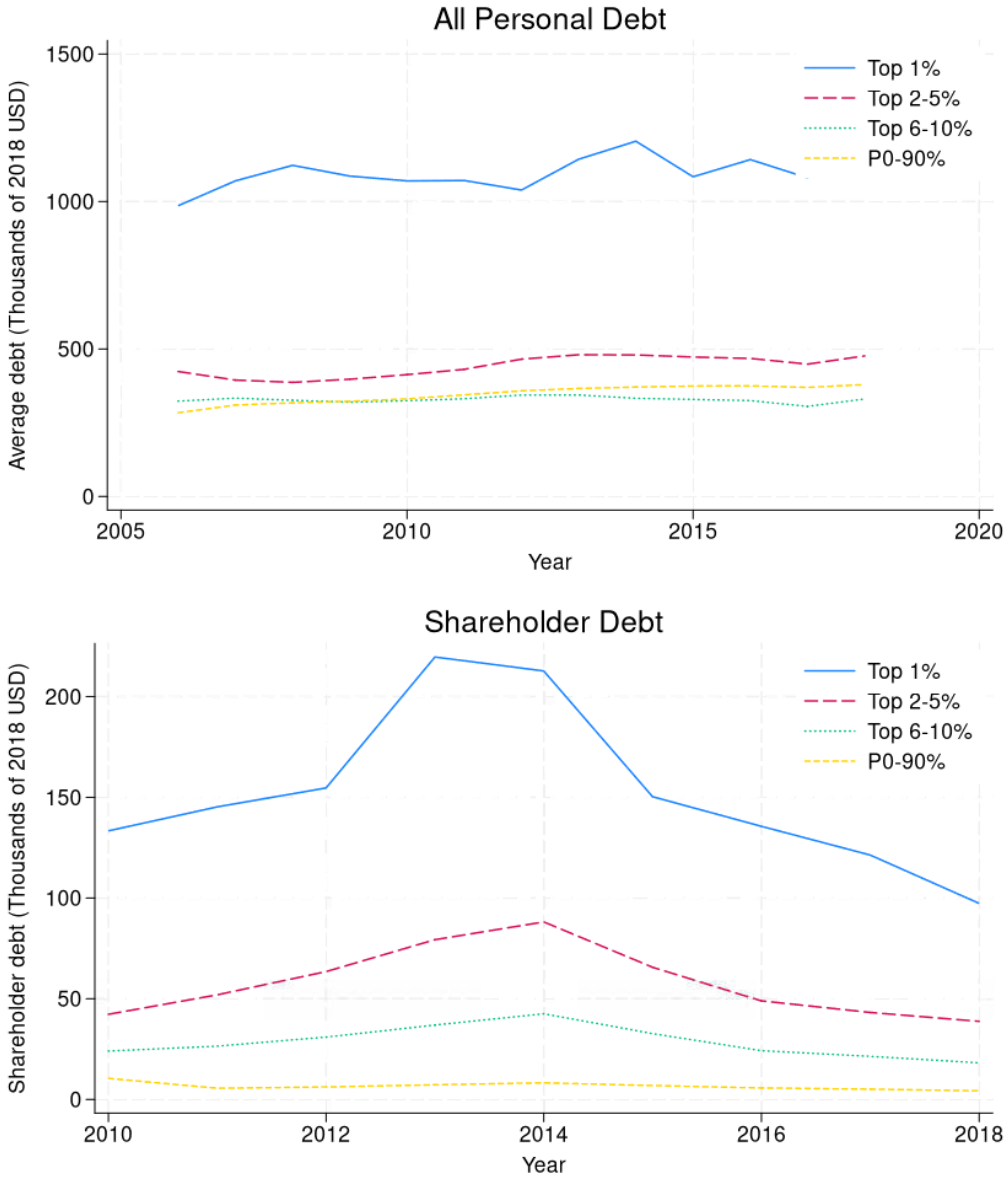
borrowing costs relative to the pre-reform internal-loan margin.

Table 1: Difference-in-differences Estimates for Household Outcomes

	(1) FE DiD	(2) FE DiD + controls
Coefficient: Treated × Post		
Outcome: Household dividends (2018 USD)	31,583.711***	34,959.850***
	(3,975.115)	(5,252.264)
Outcome: Household taxes (2018 USD)	10,467.914***	11,369.221***
	(1,554.599)	(2,055.788)
Outcome: Household consumption (2018 USD)	7,298.556	-886.177
	(5943.869)	(6151.662)
Baseline controls (2014)	No	Yes
Household fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	2,262,706	1,269,427

Notes: This table reports difference-in-differences estimates for three outcomes: household's yearly receipt of dividends , total yearly tax payments, and implied yearly consumption. Households are classified using baseline (2014) household membership: a household is treated if it contains at least one treated majority owner; it is control if it contains at least one control majority owner and no treated majority owner. Treated (control) majority owners are defined as owners with pre-reform firm-to-owner loans above NOK 100,000 (equal to zero) in majority-owned firms. Households containing both treated and control owners in 2014 are excluded. "Post" is an indicator for years 2015–2018. Baseline controls (when included) are measured in 2014. Standard errors are clustered at the household level and reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Average Personal Debt over Time by Wealth Group



Notes: The figure plots mean total personal debt over time for four wealth groups (top 1%, top 2–5%, top 6–10%, and bottom 90%). Panel (a) plots all personal debt as reported on household wealth tax filings. Panel (b) reports shareholder debt, as assigned to households using our procedure described in Section 4.1. Wealth groups are defined using baseline net wealth (e.g. 2014) and held fixed over time. Amounts are reported in thousands of 2018 USD.

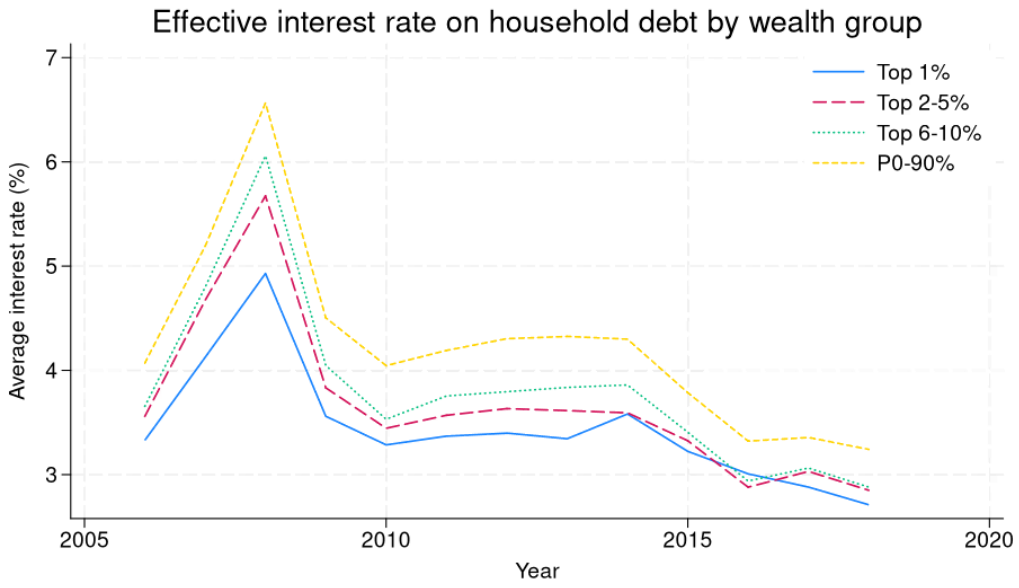


Figure 2: Interest Rate on All Personal Debt by Wealth Group

Notes: The effective interest rate is measured as total interest paid on personal debt divided by total personal debt, using the household wealth tax records. A small number of extreme outliers are trimmed. Wealth groups are defined as in Figure 1.

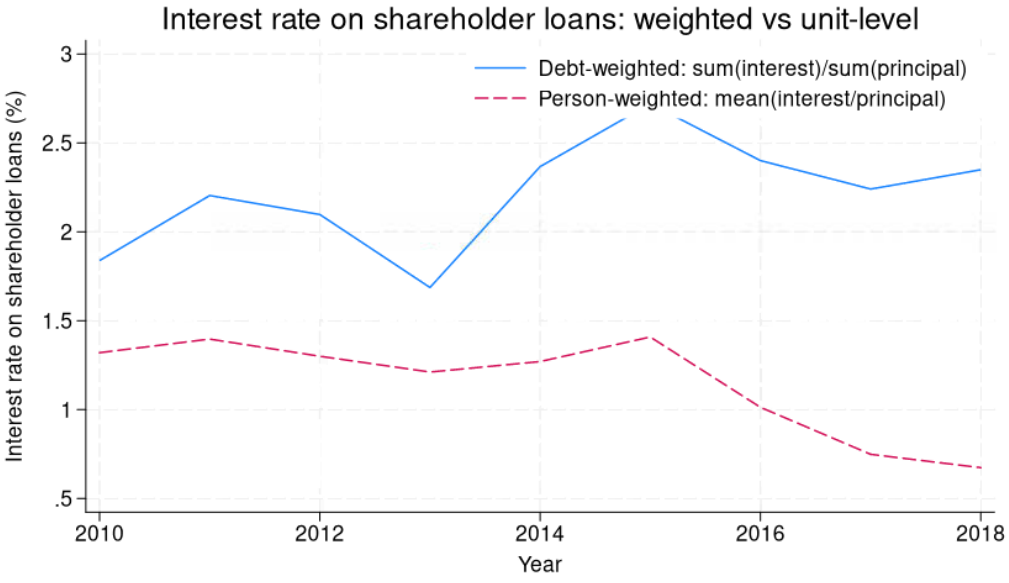
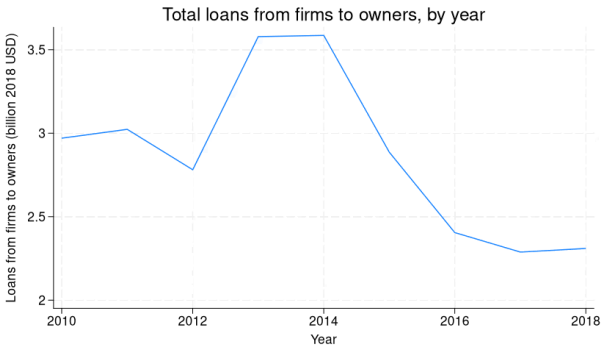


Figure 3: Interest Rates on Loans Between Firms and Owners

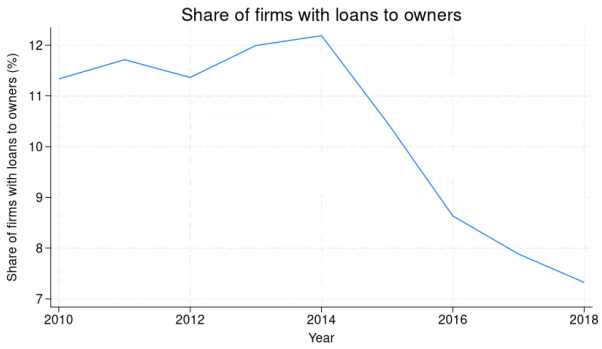
Notes: Effective interest rates on shareholder loans are constructed in this figure using two alternative calculations. For the debt-weighted line, the interest rate is calculated as average interest paid divided by average loan size. For the person-weighted line, a ratio of interest paid to loan size is calculated for each person, and we then take the average of those ratios. Values are shown in percent.

Figure 4: Firm-to-Owner Loans: Descriptive Statistics Over Time

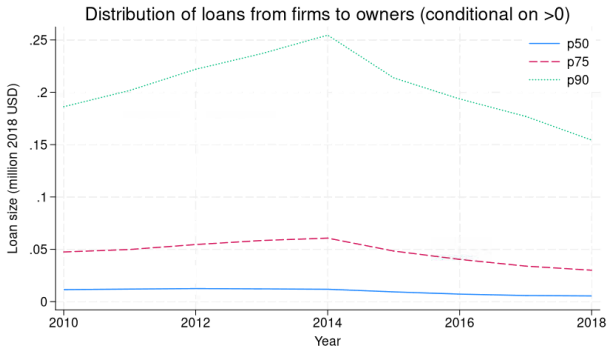
(a) Total Volume of Firm-to-Owner Loans



(b) Share of Firms with Positive Firm-to-Owner Loans

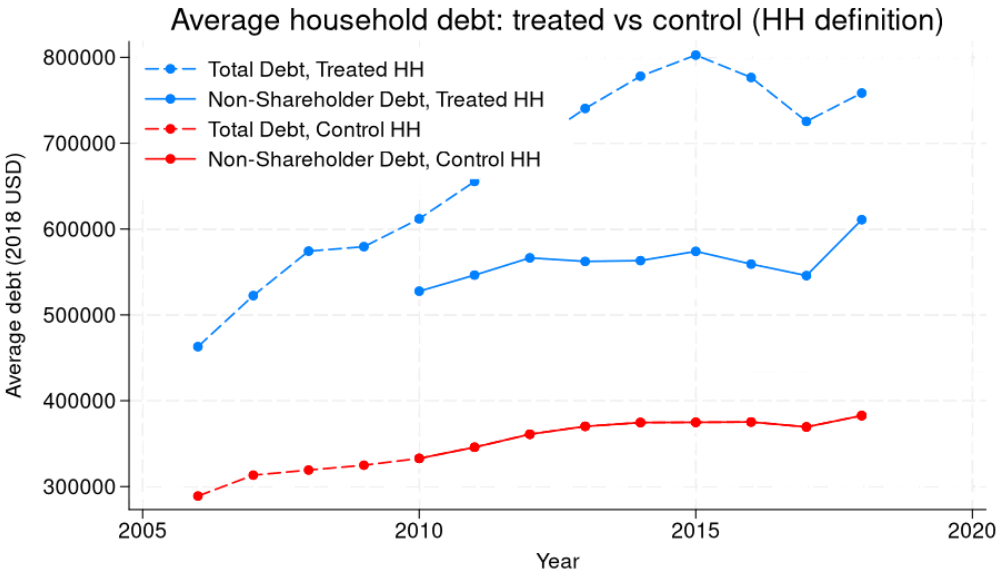


(c) Distribution of Firm-to-Owner Loan Sizes (Conditional on > 0)



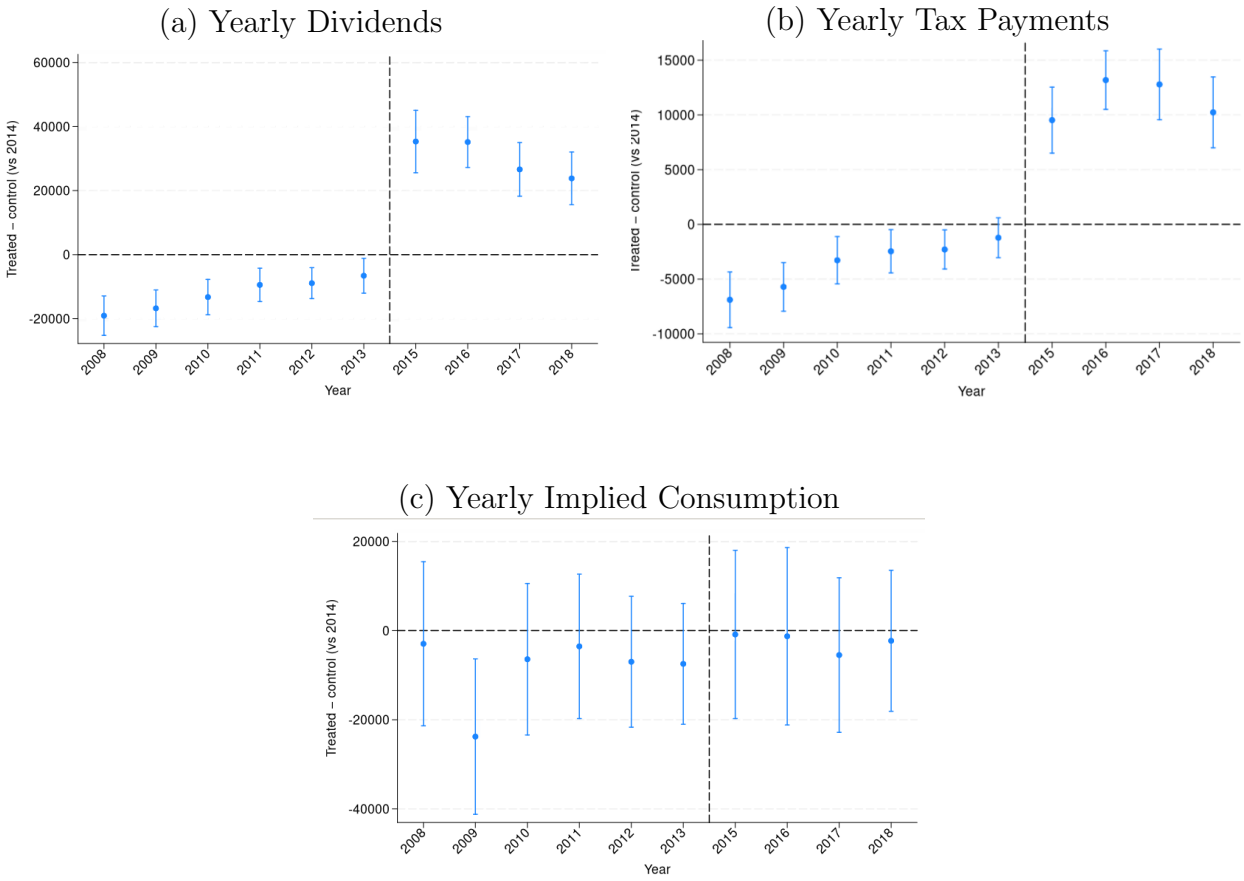
Notes: This figure plots descriptive statistics from the data on firm-to-owner loans from the corporate tax records (form RF-1028). Panel (a) plots total loans from firms to owners (in billion USD) from RF-1028. Panel (b) plots the share of active closely held firms filing RF-1028 that report a strictly positive loan from the firm to its owners in a given year. Panel (c) plots selected quantiles (as shown in the legend, e.g. p50/p75/p90) of the firm-to-owner loan amount among firms with a positive reported loan in RF-1028.

Figure 5: Average Personal Debt over Time: Treated vs. Control



Notes: This figure compares the evolution of mean personal debt for individuals classified as treated versus control based on their pre-reform exposure to owner loans from their own firms. Treatment is defined using 2014-2015 information on ownership shares and firm-to-owner loans (e.g. owners with a majority share and positive pre-reform debt from their own firm), while controls are similarly wealthy owners with no such loans. Debt is total personal debt in each year. This descriptive comparison is meant to complement, not replace, the formal difference-in-differences analysis later in the paper.

Figure 6: Event Study Outcomes for Households Exposed to Shareholder Loans



Notes: The figure shows dynamic treatment effects from event-study regressions with household and year fixed effects, comparing treated owners (with substantial pre-reform loans from their own firms) to control owners (no such loans). All coefficients are expressed relative to the last pre-reform year; standard errors are clustered at the household level. In Panel (a), the outcome is hh_aksje, a measure of household dividends. In Panel (b), the outcome is hh_skatt, a measure of household tax payments from the consumption data. In Panel (c), the outcome is yearly household consumption, constructed as the residual of yearly income and changes in net wealth from the tax registers following Eika et al. (2020). Vertical lines indicate the first post-reform year. Confidence intervals are based on standard errors clustered at the household level.

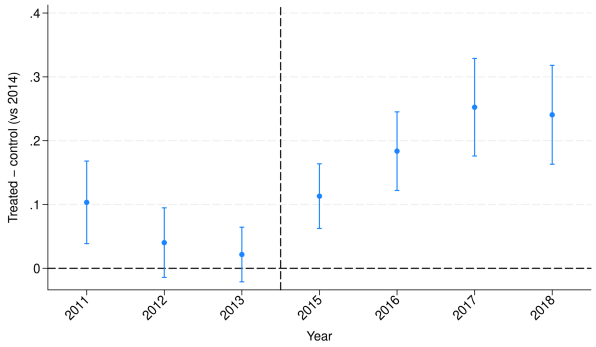
Table 2: Difference-in-differences Estimates for Third-Party Debt Outcomes

	(1) FE DiD	(2) FE DiD + controls
Coefficient: Treated × Post		
Outcome: Total external debt (NOK)	285,671***	355,057***
	(47,764)	(69,987)
Outcome: Number of creditors	0.141***	0.156***
	(0.023)	(0.029)
Outcome: Interest rate (total debt)	-0.001**	-0.001
	(0.000)	(0.001)
Outcome: Interest rate (new creditors)	-0.000	0.001
	(0.001)	(0.002)
Baseline controls (2014)	No	Yes
Individual fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	371,875	130,302

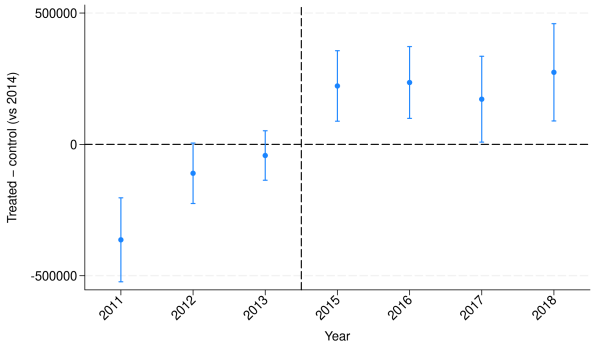
Notes: This table reports difference-in-differences estimates for four outcomes constructed from the detailed third-party debt data. In Row 1, the outcome is total debt across all third-party creditor relationships. Note that due to data export delays at the time of this draft, results in this row are reported in nominal Norwegian Kroner (in contrast to figures in the rest of this paper, which are reported in 2018 USD). 10 NOK $\approx$ 1 USD. In Row 2, outcome is the number of distinct creditors per individual-year. In Row 3, outcome is total interest divided by total debt. In Row 4, outcome is the debt-weighted interest rate among creditor relationships that appear for the first time in a given year. Interest rates are trimmed to $[0, 0.25]$. Standard errors clustered at the individual level.

Figure 7: Event Study Outcomes in the Third-Party Debt Data

(a) Number of Third-Party Creditors



(b) Total Third-Party Debt (NOK)



Note: The figure plots event-study estimates for two outcomes constructed from the detailed third-party debt data. In Panel (a), the outcome is the number of distinct creditors per person-year. In Panel (b) the outcome is the total value of all third-party loans. Note that due to data export delays at the time of this draft, Panel (b) is reported in nominal Norwegian Kroner (in contrast to figures in the rest of this paper, which are reported in 2018 USD). 10 NOK $\approx$ 1 USD. Coefficients are normalized to zero in the base year (2014). The vertical line indicates the first post-reform year (2015). Confidence intervals are based on standard errors clustered at the individual level.

6 Conclusion

Norway's October 2015 reform that taxes loans from closely held corporations to personal shareholders provides a clean test of whether wealthy owners rely on debt to obtain liquidity without realizing capital gains. When this internal credit channel becomes tax-disfavored, exposed owners substitute toward taxable payouts and realizations, increasing equity-related taxable income by about $35,000 per year and tax payments by about $11,000. We find little evidence of reduced consumption, and preliminary results-in-progress suggest firm-side patterns are consistent with payouts being financed in part by drawing down liquid assets. Overall, the results imply that taxing shareholder borrowing can broaden realization-based tax bases at the top, with adjustment occurring primarily through higher realizations rather than lower spending.

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