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An imperfect wealth tax and employment in closely held firms

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Abstract

Fuelled by increasing inequality and rising fiscal deficits, the interest in wealth taxation has grown over recent years, both in the public debate and in academia. A key concern is that the wealth tax may reduce the amount of capital available to closely held firms and drag down their employment. Yet knowledge about the behavioural effects of a wealth tax is limited. A wealth tax is almost by construction imperfect, as the value of some assets is unobserved. In particular, intangible assets held by non-traded firms are in practice tax-exempt, giving firm owners an incentive to allocate wealth into their businesses, for example, in the form of (untaxed) human capital investments. We utilize rich Norwegian register data and a series of tax reforms implemented between 2007 and 2017 to study how a net wealth tax imposed on owners of small and medium-sized businesses affects their firms' employment. Identification of causal effects is based on a saturated control function approach, fully isolating the influence of tax reforms. Our results indicate a positive causal relationship between the level of a household's wealth tax and subsequent employment growth in the taxpayers' closely held firms.

1 | INTRODUCTION

After the abolition of the wealth tax in a number of European countries during recent decades, rising inequality and deteriorating public finances have ignited a renewed interest in the wealth tax's merits and potential harmful effects (Piketty 2014; OECD 2018; Guvenen *et al.* 2019; Saez and Zucman 2019; Kopczuk 2019; Advani *et al.* 2020; Bastani and Waldenström 2020; Scheuer and Slemrod 2020, 2021). From both fiscal and egalitarian perspectives, there may be

good reasons for maintaining or reintroducing some form of a wealth tax. However, as for all redistributive taxes, a wealth tax creates behavioural distortions. A particular concern is that it discourages savings and investment, and drags down economic growth. Furthermore, a wealth tax is almost by nature imperfect, in the sense that it is impossible to assess the true value of all types of assets. This may undermine the redistributive purpose of the wealth tax and distort the allocation of resources toward lower-valued (or hard-to-evaluate) assets. Existing empirical evidence indicates a considerable negative impact of the wealth tax on reported taxable wealth, but also that this effect reflects primarily tax avoidance rather than real changes in wealth accumulation (Seim 2017; Zoutman 2014; Durán-Cabré *et al.* 2019; Brülhart *et al.* 2022; Jakobsen *et al.* 2020). Recent evidence from Norway even points towards a positive effect of the wealth tax on overall savings, suggesting that a positive income effect dominates a negative substitution effect (Ring 2020a).

A concern that has received less attention in the academic literature, but has been central in the policy debate, is the possible influence of the wealth tax on entrepreneurship and growth of small businesses; see, for example, OECD (2018, ch. 3). Asymmetric information causes a linkage between the capital available to a firm and its owner. Although the wealth tax is levied on individuals, it will be based partly on firm-level assets, and since it has to be paid regardless of current profits, it may force liquidity-constrained owners to extract capital from their firms in order to pay their personal wealth tax. However, asymmetric information also means that it is difficult for the tax authorities to assess the true value of non-traded assets. In practice, private businesses are typically subjected to an explicit tax rebate and/or valuation at book value, which is often well below market value due to the lack of inclusion of most intangible assets; see, for example, Corrado *et al.* (2022). Hence closely held businesses can serve as vehicles for tax reduction, such that the wealth tax has a positive effect on capital allocated to closely held firms.

Norway is one of very few countries that still has an annual net wealth tax levied on individuals. It is highly controversial, however, and has been subjected to frequent modifications and heated debates, the latter also within academia; see, for example, Johnsen and Lensberg (2014), Sandvik (2016), NOU (2018) and Bjerksund and Schjelderup (2019). The purpose of the present paper is to use administrative data that combine information about firms and owners to examine empirically the influence of the wealth tax on investment and job creation/destruction in small and medium-sized family-controlled businesses. To identify causal effects, we exploit a sequence of tax reforms between 2007 and 2017 that modified the wealth tax through three different margins; that is, the exemption threshold, the valuation rules and the tax rate. Our identification strategy is based on a saturated control function approach, where we regress the outcomes of interest on predicted future wealth tax liability derived from an initial (predetermined) wealth level and the upcoming tax rules, while controlling for the (counterfactual) tax liability that *would have applied* under the tax regimes belonging to other years. Hence we allow the outcome to be correlated with the wealth tax levels calculated according to all possible tax regimes in all years, but identify the causal part as the extra effect associated with the tax schedule currently applying.

Our results do *not* indicate that the wealth tax kills jobs in companies controlled by the taxpayers. On the contrary, we robustly identify a positive causal relationship between the size of the wealth tax and employment growth in small and medium-sized closely held businesses. The rise in employment applies both to labour supplied by members of the taxpaying family and to the use of non-family labour. Hence the positive employment effects may arise from a combination of an income effect, triggering higher labour supply among the taxpayers, and a portfolio reallocation effect implying that a larger share of the savings is invested in the (*de facto*) tax-favoured business. We provide supporting evidence for the latter mechanism in the form of a positive effect of the wealth tax on the fraction of savings held in non-listed shares, and a negative effect on the capital flow from the firm to the owner in the form of dividends and changes in paid-up equity. We find

no clear evidence supporting either a positive or a negative effect on wealth accumulation. It is also notable that although our results indicate that the wealth tax increases the capital available to the firm, we find no effect on the firm's investment in tangible assets. Hence our results suggest that the wealth tax has a negative influence on the ratio of physical capital to labour, at least in the short run. As physical capital enters directly into the firms' balance sheets, and is thus not subjected to the same tax preference as intangible assets, this is exactly what we would expect if the positive effect on employment arises from a tax-motivated portfolio reallocation response.

Our paper relates to an existing empirical literature examining credit market frictions, and the influence of liquidity constraints on the establishment and growth of small businesses. Although there appears to be a positive relationship between personal wealth and business entry (e.g. Evans and Jovanovic 1989; Blanchflower and Oswald 1998; Berglann *et al.* 2011), it has proven difficult to sort out undisputed causal effect estimates. A popular identification strategy is to compare entrepreneurs and business owners who to varying degrees are exposed to house price shocks. An early contribution to this literature is by Hurst and Lusardi (2004), who find that the positive relationship between entrepreneurship and wealth in the USA is largely spurious, and thus conclude that borrowing constraints are unimportant in deterring small business formation. The typical finding in the more recent literature, however, is that credit constraints are indeed quantitatively important for the establishment and growth of small firms (Nykvist 2008; Fairlie and Krashinsky 2012; Adelino *et al.* 2015; Corradin and Popov 2015; Schmalz *et al.* 2017). The significance of credit constraints is also confirmed by empirical analyses exploiting variation in the extent to which firms' credit lines were affected by the financial crisis (Chodorow-Reich 2014; Duygan-Bump *et al.* 2015). A study of particular relevance to us is Ring (2020b), which exploits idiosyncratic shocks to Norwegian investors' wealth during the financial crisis to show that private wealth has a considerable influence on investment and employment in family-controlled firms.

There is little direct empirical evidence on the influence of the wealth tax on entrepreneurship and on entrepreneurs' investment behaviour. A notable exception is by Berzins *et al.* (2020), who examine the effect of the Norwegian wealth tax based on regulatory changes in the tax value of shareholders' personal homes that occurred between 2006 and 2010. In contrast to us, they find that the tax increases were followed by lower firm investments as well as lower growth in sales and profitability. However, while Berzins *et al.* (2020) zoom in on the liquidity effect by exploiting an almost inescapable one-time tax shock, our approach allows for effects also operating thorough a potential reallocation of wealth across assets. The differences in results highlight that a wealth tax may affect owners' contributions to investment and employment thorough different mechanisms, and thus that the effects of, say, a rise in the wealth tax may depend critically on the way it is raised. If it is raised such that the incentives for wealth reallocation become stronger (e.g. a pure increase in the marginal tax rate), then a negative liquidity effect may be more than offset by a positive portfolio reallocation effect.

As the empirical analyses provided by us, as well as by Berzins *et al.* (2020), are based on partial variation in particular wealth tax parameters *given the existence of other features of the wealth tax*, neither of them provides answers to the question of how the wealth tax affects aggregate investment, entrepreneurship and overall employment. Such questions would in any case involve specification of alternative taxes and general equilibrium effects, given some fiscal budget constraint. Hence the evaluation of the overall case for a wealth tax entails the comparison of complete tax systems, which is well beyond the scope of this paper. The only attempt in this direction that we are aware of is by Hansson (2008), who exploits the variation in the existence of a wealth tax across countries to examine its influence on rates of self-employment. Based on a difference-in-differences estimation using the abolition of the wealth tax in four countries as natural experiments, she finds that abolishing the wealth tax increases self-employment by 0.2–0.5 percentage points. However, it is not clear if (or how) these tax cuts were financed through other

taxes, and given the challenges associated with cross-country comparisons (differences along many dimensions across both time and space, few observations, potentially endogenous policy choices), the empirical evidence regarding the overall effects of wealth taxes (compared to other taxes) is far from conclusive.

2 | INSTITUTIONAL SETTING

The Norwegian wealth tax levies an annual tax on the individual's net taxable wealth. The tax applies to the worldwide net wealth exceeding a basic allowance. In 2021, the tax rate was 0.85% of taxable wealth exceeding Norwegian Krone (NOK) 1.5 million (approximately €150,000). The valuation of wealth for tax purposes varies across asset classes, and for some classes (such as housing and shares), the tax value is substantially below the market value. Differences between market value and tax value arise both because the real market value of non-traded (and thus non-priced) assets is estimated conservatively by the tax authorities, and because some asset types are subjected to explicit tax rebates. As mortgage is deductible at market value, many individuals are left with low or negative taxable wealth, even though they have substantial positive wealth measured at market value.[1]

A household's wealth tax liability depends on the level and composition of wealth and on a set of tax system parameters. The latter consists of the tax rate(s), the basic allowance threshold(s) and the asset-type-specific valuation discounts. As the strategy of the present paper is to exploit the variation in system parameters to identify causal effects of the wealth tax on investment behaviour, we show in Table 1 how these parameters have changed over the past 15 years. It is clear that there have been considerable changes in all the parameters of the tax system. In that sense, Table 1 describes a series of tax reforms.

During the period covered by Table 1, there have been six tax-differentiated asset classes in the Norwegian wealth tax system: (i) assets with no tax rebate (mainly bank deposits and cash), (ii) primary home, (iii) leisure home, (iv) secondary home, (v) business property, and (vi) listed and unlisted shares. In principle, assets are valued at end-of-year market value before the application of any discount. However, unlisted shares are valued at start-of-year values based on a firm's underlying assets as they appear on the balance sheet. The latter includes financial assets and tangible assets (machinery, buildings and property), but not intangible assets such as ideas, brands, customer relations and expertise. Furthermore, acquired goodwill and patents held by the inventor are explicitly exempted from the tax base (even if they appear on the balance sheet). Based on examination of unlisted firms that are traded outside the stock exchange ("over-the-counter" trades), Gobel and Hestdal (2015) estimate that the average valuation discount for such firms is 68% (before application of the rebate shown in Table 1). Looking at newly listed firms, they estimate that the discount is as large as 91%. Although the representativeness of these numbers can be questioned, it seems clear that unlisted companies on average are valued well below their market value. This is one reason why investment in unlisted firms is a well-known strategy to reduce taxable wealth. If the initial tax value of a firm is negative (debt exceeds the tax value of assets), then while the owner's overall wealth has a positive tax value, any transfer of wealth from the owner to the firm will reduce the wealth tax liability. If the tax value of the firm is positive, then a wealth-tax-exposed person/household can still reduce the tax by investing in the firm's intangible assets, that is, assets that do not show up on the balance sheet.

Intangible assets may be created by a firm's employees, and also be complementary to the use of labour in the production process. For example, a company may have "invested" in a stock of loyal customers through marketing and high-quality services, and the existence of such "customer capital" makes it more profitable to raise employment. Human capital in the form of experienced employees with valuable firm-specific skills is typically an important part of a firm's

TABLE 1 Wealth Tax rates, Thresholds and Valuation rules, by Tax year

	Tax rates and thresholds				Valuation of assets for tax purposes				
Year	Tax rate 1 (%)	Threshold 1 (basic allowance)	Tax rate 2 (%)	Threshold 2	Primary home[b]	Leisure home[b]	Secondary home[b]	Business property	Listed and unlisted shares
2005[c,d]	0.90	151,000	1.10	540,000	PY: 0	PY: 0	PY: 0	PY: 0	MV: 65
2006[d]	0.90	200,000	1.10	540,000	PY: 25	PY: 25	PY: 25	PY: 25	MV: 80
2007[d]	0.90	220,000	1.10	540,000	PY: 10	PY: 10	PY: 10	PY: 10	MV: 85
2008[d]	0.90	350,000	1.10	540,000	PY: 10	PY: 10	PY: 10	PY: 10	MV: 100
2009	1.10	470,000	Removed		PY: 10	PY: 10	PY: 10	PY:60/MV:40[e]	MV: 100
2010	1.10	700,000			MV: 25	PY: 10	MV: 40	MV: 40	MV: 100
2011	1.10	700,000			MV: 25	PY: 0	MV: 40	MV: 40	MV: 100
2012	1.10	750,000			MV: 25	PY: 10	MV: 40	MV: 40	MV: 100
2013	1.10	870,000			MV: 25	PY: 0	MV: 50	MV: 50	MV: 100
2014	1.00	1,000,000			MV: 25	PY: 10	MV: 60	MV: 60	MV: 100
2015	0.85	1,200,000			MV: 25	PY: 0	MV: 70	MV: 70	MV: 100
2016	0.85	1,400,000			MV: 25	PY: 0	MV: 80	MV: 80	MV: 100
2017	0.85	1,480,000			MV: 25	PY: 0	MV: 90[f]	MV: 80[f]	MV: 90[f]
2018	0.85	1,480,000			MV: 25	PY: 0	MV: 90[f]	MV: 80[f]	MV: 80[f]
2019	0.85	1,500,000			MV: 25	PY: 0	MV: 90[f]	MV: 75[f]	MV: 75[f]
2020	0.85	1,500,000			MV: 25	PY: 0	MV: 90[f]	MV: 65[f]	MV: 65[f]

Notes: PY denotes % adjustment of previous year's tax value; MV denotes % of assessed market value.[a]

[a] Since 2010, assessed market values of housing are based on sale values of comparable properties. Assessed market values of business properties are based on rental values (of comparable properties if not rented out). The tax values of leisure homes are based on historical costs (up to 2009, this was also the case for other properties). A 'safety valve' applies to all real estate, i.e. the tax value should not exceed a given share of documented market value. For unlisted shares, assessed market values are based on the book value of firm's total assets (excluding goodwill and patents held by the investor) minus debt.

[b] The division between residential property (primary and secondary home) and leisure home is based not on actual use, but on the features of the property and how the building is permitted to be used. A primary home is where the taxpayer lives (it is not possible to have more than one primary home). All other residential properties are considered secondary homes.

[c] In 2005, married couples shared one basic allowance and a joint threshold in bracket 2 of NOK 580,000. Since 2006, the thresholds for married couples, who are taxed jointly, are double what is shown in the table.

[d] In 2005–8, a tax ceiling applied: wealth tax was reduced if the total tax liability exceeded 80% of ordinary income. Wealth tax could not be lower than 0.6% (0.8% in 2008) of net wealth exceeding NOK 1~million.

[e] In 2009, rented business property was valued at 40% of assessed market value, while the tax value of non-rented business property was stepped up by 60%.

[f] The valuation discounts apply to these specific assets, and associated debt, owned directly by the individual taxpayer. Operating assets (excluding business property) are valued the same shares.

real value, although it is not counted as taxable wealth. According to estimates for the UK, investment in intangible assets exceeds tangible investments, and the largest intangible component is in firm-specific training (Martin 2019).

Note that the imperfections in the Norwegian wealth tax system distort real economic behaviour; that is, they make it more profitable to invest in assets that are valued below their true market value. The discounts applying for non-listed shares may also give some scope for pure repackaging (tax avoidance), although they have been motivated explicitly by the aim of affecting real behaviour.

Even though the wealth tax gives financial incentives to allocate savings into non-listed firms, some owners may be prevented from doing so due to liquidity constraints. Indeed, the Norwegian wealth tax debate has been dominated by a reverse argument, namely that firm owners are more or less forced to pull resources out of their businesses in order pay the tax. This argument has particular force for owners who have a disproportionally large share of their wealth locked into a valuable firm, for example, as a result of inheritance of a family business.[2] If at the same time the firm faces some credit constraints due to asymmetric information, then it is probable that the wealth tax drags down investments.

The Norwegian wealth tax is levied in a setting with dual income tax: a progressive tax on labour income (top rate was 51.3% prior to 2006, and 47.8% for most of the post-2006 period) and a flat tax on capital income (22% for 2021/22, but 28% for most of the period covered in this paper), the latter including dividends exceeding an imputed normal return (until 2005, dividends were tax-exempt).

Based on the tax rules that applied in 2011, Halvorsen and Thoresen (2021) examine the distributional effects of the Norwegian wealth tax and show that a considerable share of the wealth tax is levied on individuals with low current (annual) income. However, when evaluated against lifetime rather than annual income, the wealth tax is born largely by high-income taxpayers, such that the tax indeed fulfils its redistributive purposes.

To prepare the ground for a more formal analysis, we set up the wealth tax function explicitly, emphasizing the distinct roles of (endogenous) household wealth characteristics and (exogenous) tax system parameters. A household's (or an individual's) wealth tax in a particular year t is determined as follows[3]:

$$T_{it} = \max\left(0, \left(\sum_j w_{ijt} R_{jt} - d_{it} - A_t\right)\tau_t\right) = T\left(\mathbf{w}_{it}, \boldsymbol{\tau}_t\right), \tag{1}$$

where

$$\mathbf{w}_{it} = \{w_{i1t}, \ldots, w_{i6t}, d_{it}\}, \quad \boldsymbol{\tau}_t = \{R_{1t}, \ldots, R_{6t}, A_t, \tau_t\}.$$

Here, T_{it} is the tax imposed on household i in year t, w_{ijt} is the assessed market value of the household's wealth held in asset type j ($j = 1, \ldots, 6$), d_{it} is the household's debt, R_{jt} is the fraction of wealth held in asset type j that is subjected to the wealth tax, A_t is the threshold for the basic tax-exempted allowance, and τ_t is the tax rate.

3 | IDENTIFICATION STRATEGY

The research questions addressed in this paper involve the causal effects of a variable (the wealth tax) that is subjected to multiple sources of variation—some endogenous (the level and composition of wealth $\mathbf{w}_{it}$) and some exogenous (the tax system parameters $\boldsymbol{\tau}_t$). The empirical challenge is to isolate the influence of the tax system parameters through the exploitation of tax reforms. Although we can assume safely that the tax system itself is exogenous with respect to the

behaviour of each (potential) taxpayer, the way it affects economic behaviour clearly depends on the level and composition of wealth. Hence without proper controls, identification of the wealth tax's causal effects relies on parallel trend assumptions. In the spirit of Borusyak and Hull (2021), our solution to this problem is to compute the wealth tax that *would have applied* under all the tax regimes that have existed in our data period (see Table 1), and include these counterfactual tax liabilities as controls in regression models. This implies that the control function is saturated, in the sense that without the tax reforms, it would have soaked up all the variation in the wealth tax and thus induced perfect multicollinearity. We show below that controlling for all counterfactual tax rates purges omitted variables bias and ensures valid identification of causal parameters under plausible assumptions.

In order to set up a proper causal model, we also need to take into account the fact that the actually paid wealth tax is itself a choice variable, in the sense that the household can adjust the level and composition of wealth in response to the tax system. We deal with this problem by examining the effects of the *potential* (rather than the actual) wealth tax, that is, the tax computed from superimposing a particular tax regime on a given *predetermined* wealth. The tax parameters applying for a particular year are always announced the year before, and since the value of non-listed shares is assessed on the basis of start-of-year book value, there may in some cases be incentives to reallocate wealth in response to an announced tax reform already in the year before the reform comes into force. To ensure that the household's wealth is predetermined with respect to the explanatory wealth tax variable, we use the potential wealth tax liability that will apply in two years, given a current wealth level, as the central explanatory variable.

The causal models are framed in terms of a base year, a tax year (two years later) and an outcome year, where the latter may (or may not) be the same as the tax year. The base year b is the year in which the owner's actual wealth and ownership share are measured, and the year in which we define the criteria for being included in the dataset. Let $y_{i,b+s}$ be some outcome for firm/household i measured s years after the base year. Let $\mathbf{w}_{i,b}$ be a vector of assets measured in base year b, and let $f_b\left(T\left(\mathbf{w}_{i,b},\boldsymbol{\tau}_t\right)\right)$ be some functional form representation of the hypothetical wealth tax calculated according to tax rules applying in year t. Finally, let BY indicate base-year fixed effects. For a given choice of s, the models that we estimate will then have the following structure:

$$y_{i,b+s} = \delta_{b+s} f_b\left(T\left(\mathbf{w}_{i,b},\boldsymbol{\tau}_{b+2}\right)\right) + \sum_{t=2007}^{2017} \pi_t f_b\left(T\left(\mathbf{w}_{i,b},\boldsymbol{\tau}_t\right)\right) + BY + controls + \varepsilon_{i,b+s}, \tag{2}$$

for $b = 2005, \ldots, 2015$. The parameter of interest is δ_{b+s}, which captures the effect of the potential tax liability calculated for the second year after the base year. In the causal analysis, we focus on $s = 2, 3, 4$, while we let s vary from -4 to 4 in the validation part of the analysis (exploiting that $\delta_{b+s} = 0$ for all $s < 0$). The model is estimated separately for each choice of s, implying that the inclusion of base-year fixed effects is equivalent to inclusion of outcome-year fixed effects.

Unbiased estimation of the causal parameter δ_{b+s} requires that

$$E\left[f_b\left(T\left(\mathbf{w}_{i,b}\boldsymbol{\tau}_{b+2}\right)\right)\ \varepsilon_{i,b+s} \mid f_b\left(T\left(\mathbf{w}_{i,b}\boldsymbol{\tau}_{2007}\right)\right), \ldots, f_b\left(T\left(\mathbf{w}_{i,b}\boldsymbol{\tau}_{2017}\right)\right), BY, controls\right] = 0. \tag{3}$$

This assumption will be satisfied by construction provided that any unaccounted for relationships between the tax variables and the influence of (or spurious correlation with) wealth characteristics $\mathbf{w}_{i,b}$ do not change over time in a way that is correlated with the changes arising from the tax reforms. If equation (3) holds, then we have ensured that any misspecification of the direct wealth effects and its correlates will be absorbed by the hypothetical tax functions in their capacity as controls.[4] Equation (2) will then yield unbiased estimates of the causal effects of the potential wealth tax. The intuition is that while the causal effect of any year-s-calculated wealth tax can apply only when s corresponds to the actual tax year in question (or in the years

afterwards if the effect builds up gradually or operates with a lag), the spurious associations will be there regardless of outcome year. By allowing the outcome to be influenced by hypothetical wealth taxes calculated according to all possible tax regimes in all years, we ensure that the causal part is identified as the "extra" effect associated with the wealth tax currently applying.

Identification strategies akin to ours have been used previously in studies of the impacts of unemployment benefits on unemployment duration in Norway and Sweden (Røed *et al.* 2008), the impact of student aid on college enrolment in Denmark (Nielsen *et al.* 2010), and the impact of disability insurance benefits on labour supply in Norway (Fevang *et al.* 2017) and Austria (Mullen and Staubli 2016). Our identification strategy is also similar in spirit to the approach used in the taxable income literature—for example, by Gruber and Saez (2002) and Kleven and Schultz (2014)—to estimate the elasticity of taxable income on the basis of tax reforms. But while there have been various solutions in the taxable income literature to deal with the spurious correlation problem by controlling for base-year income in flexible ways, we introduce a novel solution by controlling for all possible hypothetical taxes under all tax regimes.[5]

Our identification strategy has similarities to a standard difference-in-differences approach, as the effect is encapsulated by the *interaction* of time and treatment, with control for the respective separate influences of time and treatment. However, as the treatment is continuous and reforms occur every year, there are neither well-defined treatment and control groups nor any unaffected pre-period for which to report pre-trends. To assess the validity of the identifying assumption, we thus rely on two alternative strategies. First, we use equation (2) to perform an "event study" where we estimate "effects" for years both prior to and after the base year, facilitating a graphical validation of the identifying assumption. Second, we include additional sets of controls in a step-by-step fashion, accounting for the possibility of differential employment trends along multiple dimensions (household income, location, industry, initial firm size). In addition, we perform a number of robustness exercises based on alternative cuts of the data and different specifications of the functional form relationships (as captured by $f_b(\cdot)$).

4 | DEFINITION OF OUTCOME AND CHOICE OF FUNCTIONAL FORM

The dependent variable of primary interest in this paper is the relative change in employment from a base year b to an outcome year $b+s$, that is,

$$y_{i,b+s} = \frac{E_{i,b+s} - E_{i,b}}{E_{i,b}}, \tag{4}$$

where $E_{i,t}$ is total employment in the firm of household i in year t, weighted with the household's owner share in the base year. Ideally, $E_{i,t}$ should be a precise measure of total labour input during year t. However, administrative register data for the period covered by our analysis do not contain precise and fully reliable information about hours or days worked. On the other hand, they contain very precise and reliable information about annual wage costs. We are thus going to use total annual wage costs as our primary outcome measure. To the extent that the wage level reflects marginal productivity, we can think of total wage costs as a productivity-adjusted employment metric. However, as we cannot rule out that the owner's wealth tax also influences the wage level among employees (particularly employees belonging to the owner's own family), we also perform the analysis based on an employment definition that counts contracted work hours as (imperfectly) reported to the administrative employer–employee register. Moreover, to distinguish extensive and intensive response margins, we apply a pure head count, that is, an employment measure giving the total number of employees during a year (regardless of hours).

The choice of functional form for the influence of the wealth tax represents a challenge in our case, as the distribution of taxable wealth is heavily skewed. Our primary strategy will be to normalize the wealth tax variables (and other controls) either with total (owner-weighted) wage costs or with the household's net taxable wealth. With total wage costs as the normalization variable, we specify $f_b(\cdot)$ as the total potential wealth tax as a fraction of total owner-weighted wage costs in the base year, that is,

$$f_b\left(T\left(\mathbf{w}_{i,b},\boldsymbol{\tau}_t\right)\right)=\frac{T\left(\mathbf{w}_{i,b},\boldsymbol{\tau}_t\right)}{E_{i,b}}. \tag{5A}$$

An important advantage with the specifications in equations (4) and (5A) is that the causal parameter δ_{b+s} in equation (2) has a simple and intuitive interpretation as the change in money spent on wages in the closely held firm caused by each extra NOK of potential wealth tax. This appears convenient, given the prominent role of the argument that liquidity constraints force many owners to pay the wealth tax NOK-for-NOK by pulling resources out of closely held firms. Dividing both the regressor and the regressand by the same variable is known to entail a "division bias" if the latter is measured with error, as it induces a spurious correlation between them (Borjas 1980). Measurement error in the total wage bill is likely to be small, however, as it is reported directly to the tax authorities. Moreover, as we describe in more detail below, the regression model that we use is designed to deal with the division bias problem.[6]

With net taxable wealth as the normalization variable, we circumvent the division bias problem. We can then use a log(net-of-tax rate) specification, which is more standard in the tax literature; that is,

$$f_b\left(T\left(\mathbf{w}_{i,b},\boldsymbol{\tau}_t\right)\right)=\ln\left(1-\left(T\left(\mathbf{w}_{i,b},\boldsymbol{\tau}_t\right)/NW_{i,b}\right)\right), \tag{5B}$$

where $NW_{i,b}$ is the household's net taxable wealth in the base year.[7] In this case, δ_{b+s} is interpreted as an elasticity, that is, the percentage change in a firm's owner-weighted employment level caused by a 1% change in the owner's net-of-tax rate. A problem with this specification is that the size of the owner's wealth and the size of the closely held firm vary enormously across owners. In some cases, we look at small firms owned by extremely wealthy owners (who have only a small share of their wealth in the firm), and in others, we consider large firms owned by less wealthy owners (who may have all their wealth in the firm). There is no reason to believe that a given percentage change in the net-of-tax rate for these owners has the same percentage effect on their firms' employment. A response proportional to the actual NOK change in the owners' wealth tax appears more reasonable, and ensures that the explanatory variable and the outcome are measured on the same scale. We thus use equation (5A) as our primary specification, but report main results also based on equation (5B).

5 | DATA SAMPLING AND DESCRIPTIVE STATISTICS

Our analysis is based on encrypted administrative registers of high quality. We combine four blocks of linkable data. The first block contains detailed information about taxable wealth (total wealth and its components) for all adult residents (and households) in Norway, and covers the period from 2005 to 2015. This facilitates accurate computation of the hypothetical wealth tax according to all the tax regimes described in Table 1. The second block contains annual accounts for all limited liability firms in Norway and data on self-employment earnings for sole proprietorships, and these data also cover the years 2016 and 2017. The third block contains a list of ultimate owners of limited liability companies in Norway, including total owner shares (owned either directly or indirectly through other companies). And the fourth block contains accounts

of all employees in Norway, including their annual salaries and the identities of their employers. The latter data are available also for years prior to 2005 and up to 2019.

As the primary purpose of the analysis in this paper is to examine the impacts of the wealth tax on employment in small and medium-sized closely held (family-controlled) businesses, we combine these four data blocks to establish an analysis dataset consisting of firms and owners that fall into this category. In the main part of our analysis, we define a small or medium-sized closely held business as a firm that has between 1 and 100 (owner-weighted full-time-full-year-equivalent) employees and is (directly or indirectly) controlled by a single person or household (owner share at least 50%). The lower inclusion threshold of at least one employee is implemented to ensure that the firms under study have some real economic activity, and it is operationalized by requiring an annual wage cost exceeding NOK 500,000 measured in 2015 value (approximately €50,000, corresponding roughly to the cost of one full-time-full-year employee), excluding self-employment income. In Online Appendix C, we provide results for a wide range of alternative cut-offs, also facilitating separate analysis of small and large firms.

Each observation in our data is a match of a firm and an owner in a particular year. It is instructive to think of the owner as the unit of observation, as the wealth tax is imposed at the household level. All firm variables will be weighted by the family's owner share, such that, for example, a firm with 10 employees, which is owned 50% by a single family, will for this family count as 5 employees. In Online Appendix D, we provide results for models where we merge firms that are owned jointly by two families into single observation units, as well as for models where we examine only firms that are fully owned by single families.

To construct a baseline dataset for empirical analysis, we sample all small and medium-sized closely held firms in Norway, for each year from 2005 to 2015.[8] This gives us approximately 460,262 firm–household by base-year observations. Potential wealth tax liability is then measured two years after the respective base years, that is, in 2007–17, whereas primary outcomes are measured 2–4 years after the base year (2007–19). As explained in the previous section, the central explanatory variable in our analysis is the owner's potential wealth tax relative to (owner-weighted) base-year wage cost in the closely held firm. Figure 1 shows the distribution of the owner's maximum wealth tax (with the maximum taken over all the tax regimes in operation during our estimation period) relative to their closely held firm's wage costs. Almost 50% of the owners do not pay any wealth tax at all, regardless of tax regime, and approximately 95% never pay more than 9% of the owner-weighted wage costs in their closely held firm. The 99th percentile

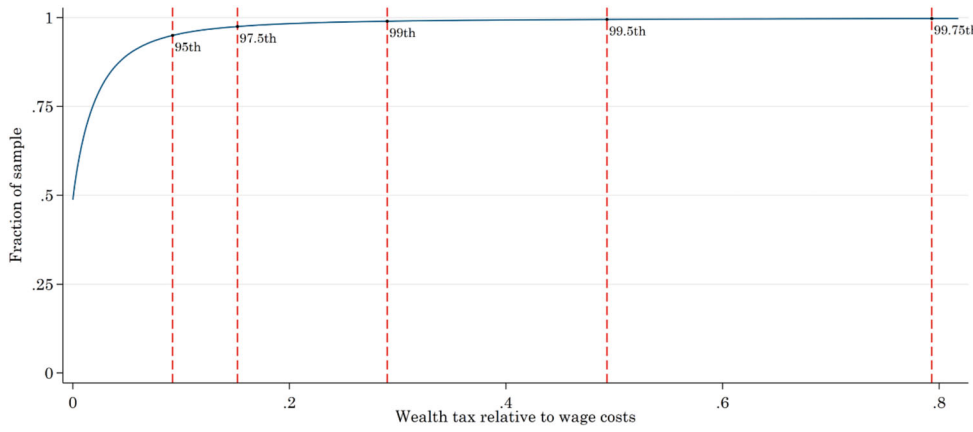

FIGURE 1 The distribution of owners' maximum wealth tax liability relative to the owner-weighted total wage cost in a closely held firm *Notes*: The figure shows the distribution of the highest possible wealth tax (out of all the regimes applying during 2007–17) that can be calculated given the observed taxable base-year wealth (2005–15). The number of observations is 460,292.

is just below 30% of the wage cost. Yet there are some observations (approximately 0.19%) with potential tax liability above 100% of wage costs, and even some above 1000% (approximately 0.1%), suggesting that the owner's wealth in these cases has little to do with the firm in our sample. If these observations are included in the analysis, then they will potentially drown any systematic relationship in the central parts of the data. Hence, to avoid excess influence from outliers and to ensure that the firms included in our analysis have a non-negligible economic activity relative to the owner's wealth, we trim the sample somewhat at the top of the (maximum) wealth tax relative to wage costs distribution. In the main part of the analysis, we trim the sample at the 99th percentile, at which point the owner's highest possible wealth tax constitutes 29.1% of the firm's (owner-weighted) wage costs.[9]

We then end up with 455,681 base-year observations consisting of 106,534 unique firm–household combinations, each observed for an average number of 4.3 base years. Table 2 shows some descriptive statistics. Approximately 64% of the owner observations are married couples, 29% are single men, and 7% are single women. On average, these households hold

TABLE 2 Descriptive statistics analysis data

	Mean/ fraction	Median	Standard deviation
Panel A: Type of owner/household ($N = 455{,}681$)			
Married couples	0.64		
Single male	0.29		
Single female	0.07		
Panel B: Household characteristics ($N = 455{,}681$)			
Gross wealth before valuation rebates (1000 NOK)	9677	6156	16,680
Gross wealth, tax value (1000 NOK)	5641	2875	13,536
Net wealth before valuation rebates (1000 NOK)	6913	3831	15,184
Net wealth, tax value (1000 NOK)	2877	682	12,827
Potential wealth tax (1000 NOK)	30.4	0	125
Liquid assets (bank deposits, listed shares, fund shares) (1000 NOK)	917	302	3303
Potential wealth tax rate (% net taxable wealth)	0.17	0	0.26
Potential wealth tax relative to (owner-weighted) wage costs (%)	1.30	0	3.03
Panel C: Firm characteristics (weighted by owner share) ($N = 455{,}681$)			
Total wage bill (1000 NOK)	2263	1270	3332
… accounted for by own family	440	404	361
Total employment (full-time equivalents)	5.16	3.10	6.91
… accounted for by own family	0.77	0.79	0.55
Panel D: Firm characteristics, limited liability companies only (weighted by owner share) ($N = 405{,}003$)			
Tangible assets (machinery, buildings, property) (1000 NOK)	1224	219	7839
Liquid assets (bank deposits, listed shares, fund shares) (1000 NOK)	1415	580	3573
Dividend payments to owner (1000 NOK)	246	0	1071
Salary to own family (1000 NOK)	494	455	347

Notes: Each observation is a household–owner combination in a particular base year. There are 106,534 unique household–owner combinations, on average observed in 4.3 years. The term 'potential wealth tax' is used to indicate the wealth tax liability based on the level and composition of wealth two years before the respective tax years. Data reported in panel D are available only for limited liability companies (not for sole proprietorships), implying that approximately 11% of the observations are lost when variables in this panel are used as outcomes.

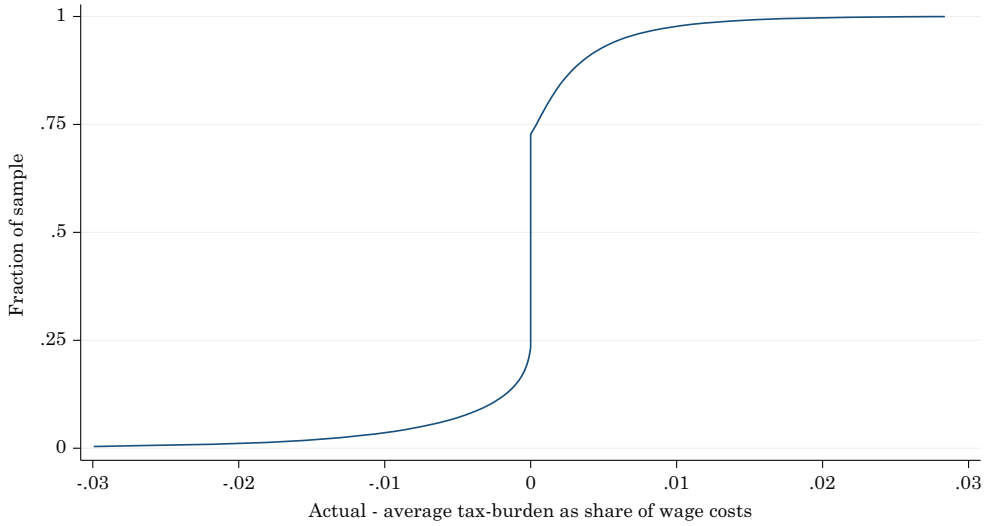

FIGURE 2 The reform-generated variation in wealth tax liability relative to the closely held firm's owner-weighted wage costs *Notes*: The reform-generated variation is defined as the difference between the actual and the average wealth tax liability, where the average is taken over all the tax regimes that have existed between 2007 and 2017. The number of observations is 455,681.

approximately NOK 2.9 million (roughly €290,000) in net taxable wealth, and NOK 6.9 million in total net wealth (before valuation rebates), and pay NOK 30,000 in wealth tax.[10] The average tax rate is 0.17% of net taxable wealth, and constitutes approximately 1.3% of the firm's total (owner-weighted) wage costs. Due to the tax reforms described in Table 1, the fraction of owners paying any wealth tax at all has declined considerably over time, from approximately 55% in 2007 to 38% in 2017. The family-run businesses analyzed in this paper are typically small, with 5 (fulltime-equivalent) employees on average and median employment as low as 3. Together, they account for approximately 13% of all employees in Norway. It is also notable that a non-negligible share of the employees in these firms belong to the owner family (defined as the owner, the owner's spouse, and the owner's children below age of majority). On average, 19% of the firms' wage costs are paid out to employees belonging to the owner-families.

To provide some intuition on the variation in tax liability created by the tax reforms, Figure 2 shows the distribution of differences between the actual and regime-averaged tax liabilities, relative to the each firm's wage costs in the base year, where the regime-averaged tax liability is calculated based on all tax regimes that existed between 2007 and 2017. For roughly half of the household–firm observations, there is no reform-generated variation at all, simply because the wealth tax is zero in all regimes. For the remaining observations, the reform generated a variation ranging from −3% to 3% of the firms' total wage costs.

Although the main part of our analysis is based on the dataset described in Table 2, we use somewhat modified datasets in parts of the analysis. First, in the analysis where we use the log(net-of-tax rate) as the key explanatory variable (equation (5B)), we do not have to trim the data to avoid outlier problems; hence we use all the available 460,262 observations. Second, in the analysis of wealth accumulation, we condition on savings exceeding NOK 100,000 in the base year, and in the analysis of wealth composition, we condition on savings exceeding NOK 100,000 in the outcome year. (With negligible wealth in the outcome year, an analysis of wealth composition is meaningless.) Finally, in analyses of capital flows between firms and owners and firms' investment in tangible assets, we can include only limited liability companies (for which there is a formal distinction between firm and owner).

6 | EMPIRICAL ANALYSIS

In this section, we present our estimation results. We begin with the analysis of employment outcomes, where we first validate our identifying assumptions and then present the main findings of the paper together with a robustness analysis. We then take a closer look at employment responses in terms of extensive and intensive margins, and examine how they are composed of responses from family and non-family workers, respectively. After that, we examine the extent to which employment effects are moderated by owner's liquidity constraints, and in the final subsection, we investigate effects of the wealth tax on savings behaviour, on the capital flows between owner and firm, and on investments in physical capital within the family-controlled firm. Additional robustness analyses are provided in the Online Appendices.

6.1 | Effects on employment

As hiring—and firing—typically takes time (and involves elements of irreversibility, due to employment protection legislation and labour relations norms), we expect employment effects to build up gradually; hence to examine employment effects, we look at outcomes both in the tax year ($b+2$) and in the two following years ($b+3$ and $b+4$). Given that our employment data are updated until 2019, this implies no loss of observations. Figure 3 illustrates the distribution of the employment changes observed for these three years, in all cases relative to the base year. In the year of the potential wealth tax liability ($b+2$), 10% of the firms no longer have employees. Approximately 30% have roughly the same total wage costs as in the base year (±10%). Only around 1% of the firms have increased wage costs by 200% or more. For the subsequent years, the changes become somewhat larger. Four years after the base year, approximately 20% of the firms no longer have any employment, and 2% have grown by more than 200%.

Before we examine the impacts on employment in years $b+2$, $b+3$ and $b+4$ in more detail, we provide a graphical validation of our identification strategy in the form of an event-study. Figure 4 reports estimated employment effects for a range of outcome years, also covering the pre-base-year period. Here, we use $\left(E_{i,b+s}-E_{i,b}\right)/E_{i,b}$ as the outcome variable in equation (2), with s varying from −4 to 4, and the key explanatory variable is the total potential wealth tax in year $b+2$ as a fraction of total owner-weighted wage costs in the base year (equation (5A)).[11]

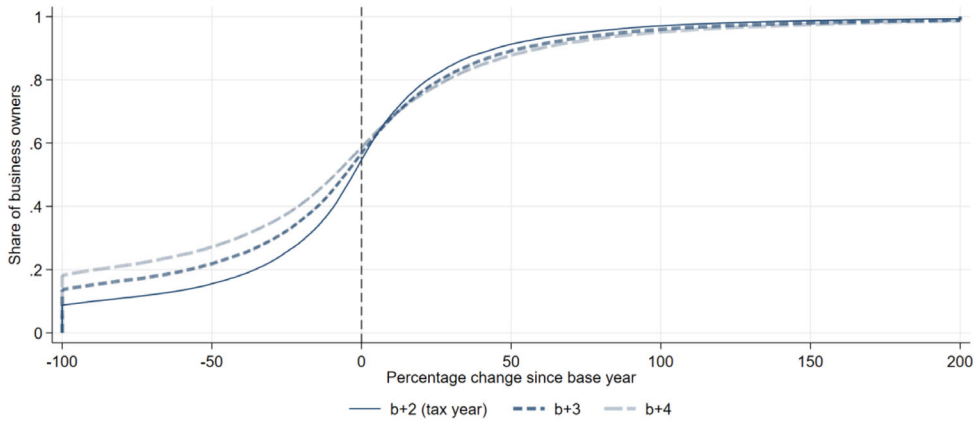

FIGURE 3 Distribution of the percentage change in total wage costs from the base year to the outcome year
Notes: The figure shows the cumulative density function of the relative change in the owner-weighted total wage bill from the base year to the potential tax year (two years after the base year), and for the two subsequent years. Data pooled over all available base years and outcome years.

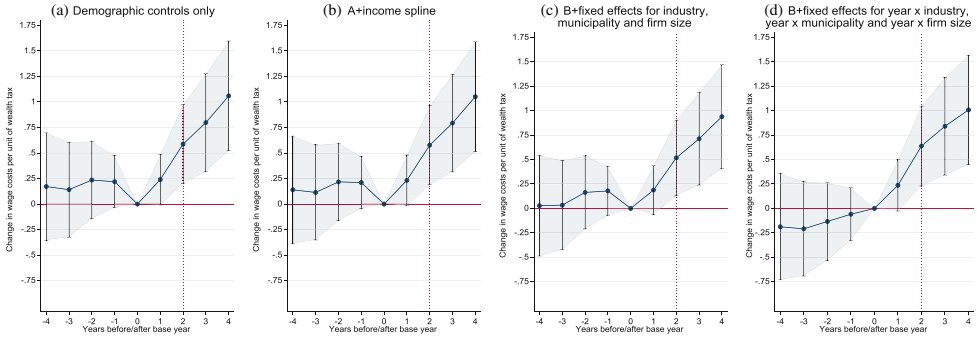

FIGURE 4 The estimated effects of potential wealth tax in year $b+2$ on total wage costs in years from $b-4$ to $b+4$. *Notes*: The graphs show the estimated δ coefficients from equation (2) when the dependent variable is the relative change in the owner-weighted total wage bill from the base year to the outcome year (equation (4)) and the tax is measured relative to the wage bill (equation (5A)). Outcome years are indicated on the horizontal axis relative to the base year, and year $b+2$ is the year of the potential wealth tax (given the wealth in year b). Firms that close down (after the base year) and firms that are not yet established (prior to the base year) are interpreted as having zero employment in the relevant years. Control variables in panel A include 11 base-year fixed effects and separate indicators for all 789 actually occurring combinations of household type (three categories: couple, single man, single women), age (66 categories) and immigrant status (four categories: native, immigrant from Eastern Europe, immigrant from developing country, immigrant from other developed country), with age and immigrant status referring to the male for couples. The cubic income spline added in panel B has 7 knots. The controls added in panel C are 438 indicator variables for municipality, 653 indicator variables for industry (based on 5-digit NACE), and 107 indicator variables for number of employees in the base year (based on total wage bill, with cell sizes equal to NOK 100,000 up to NOK 5,000,000, thereafter 500,000 up to 10,000,000, followed by 1,000,000 up to 50,000,000, and finally 5,000,000 above 50,000,000. For the model in panel D, all the municipality, firm size and industry dummies used in panel C are interacted with base year dummy variables. In total, the model in panel D contains 12,461 fixed effects in addition to the base-year income spline. The total number of observations is 455,681, but the numbers used in each regression are slightly lower as some owners are no longer alive (or resident in Norway) in the respective outcome years. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the owner level.

In order to reduce the influence of outliers, we winsorize the outcome variable at 2 (200% increase in employment).[12] To assess the robustness of our validation exercise, we introduce control variables in a stepwise fashion. We start out using a version of equation (2), where in addition to the base-year fixed effects, we include only controls for the demographic characteristics of the owner in the form of indicator variables for all (789) combinations of household type (single man, single women, couple), age and immigrant status (the latter two characteristics with reference to the male partner within couples). The result is shown in Figure 4(A). We add controls for the owner's base-year income, in the form of a cubic spline (Figure 4[B]), and then non-parametric controls for firm size (107 categories), industry (653 categories) and municipality (438 categories) (Figure 4[C]). Finally, to allow for differential trends in different types of firms, we interact the latter set of controls with base-year dummy variables, ending up with fixed effect for firm-size by year (1063 categories), industry by year (5891 categories), and municipality by year (4718 categories) (Figure 4[D]).

As can be seen from Figure 4, all the models indicate that the wealth tax influences employment growth *positively* in the tax year, as well as in the two subsequent years. There are also some indications of a response already in year $b+1$ although this is not statistically significant at the 5% level. A small effect in $b+1$ is plausible, given that tax rules applying for $b+2$ will be common knowledge in $b+1$. In addition, as shown in Table 1, the tax reforms during our estimation period have had a sort of incremental structure, such that neighbouring tax regimes are more similar than more distant regimes. In particular, the tax regimes applying from 2010 to 2012 were almost identical. Consequently, the $b+2$ calculated wealth tax may pick up some effects of the omitted same-year-calculated wealth taxes in regressions applying for other

TABLE 3 Estimated effects of potential wealth tax on total wage costs

Effect in:	Year fixed effects and demographic controls (A)	A + income spline with 7 knots (B)	B + fixed effects for industry, municipality and firm size (C)	B + fixed effects for industry by year, municipality by year and firm size by year (D)
$b+2$	0.593***	0.583***	0.537***	0.636***
	(0.195)	(0.195)	(0.194)	(0.206)
R-squared	0.017	0.018	0.033	0.064
Number of observations	455,615	455,615	455,037	454,340
$b+3$	0.833***	0.829***	0.761***	0.871***
	(0.241)	(0.241)	(0.239)	(0.252)
R-squared	0.019	0.020	0.042	0.071
Number of observations	453,917	453,917	453,342	452,643
$b+4$	1.047***	1.040***	0.939***	0.999***
	(0.272)	(0.272)	(0.269)	(0.283)
R-squared	0.020	0.021	0.048	0.074
Number of observations	452,246	452,246	451,674	450,973

Notes: Standard errors (in parentheses) are clustered at the person/household level. The dependent variable is the relative change in the owner-weighted total wage bill from the base year to the outcome year. Firms that close down after the base year are interpreted as having zero employment. The reported estimates are the δ coefficients in equation (2). For a detailed description of the control variables included in each model, see Notes to Figure 4. The total number of observations is 455,681, but the numbers used in each regression are slightly lower as some owners are no longer alive (or resident in Norway) in the respective outcome years, and some of the fixed effects are unique for specific observations.
*, **, *** indicate statistical significance at the 10%, 5%, 1% level, respectively.

years, such as $b+1$ and $b+3$. Most importantly, none of the models indicates any effects in the pre-base-year period. Table 3 reports more detailed results for the three outcome years $b+2$, $b+3$ and $b+4$. The estimates are quite stable across the different models, and imply that a one unit increase in the potential wealth tax increases the money spent on wages in the taxpayer's firm by 0.54–0.64 units in the same year, and by 0.76–0.87 units and 0.94–1.05 units, respectively, in the subsequent two years. Although the explanatory power (measured by R-squared) increases by a factor 3.7 from the most parsimonious models in column (A) to the models with all controls included in column (D), the parameter estimates of interest remain similar. The effects identified for $b+3$ and $b+4$ are likely to reflect both the longer-term influence of tax exposure in year $b+2$ and a positive correlation with (the omitted) tax exposure in years $b+3$ and $b+4$.

As an alternative to the linear NOK-for-NOK specification of the model based on equation (5A), we repeat the whole estimation exercise based on the elasticity specification outlined in equation (5B).[13] This specification has the advantages that it goes clear of any division bias and naturally deals with outlier problems (the net-of-tax rate is always between 0.99 and 1); hence we can use the complete (rather than the trimmed) dataset. A potential disadvantage is that it fits poorly to the alleged liquidity-driven NOK-for-NOK responses. Since the net-of-tax rate is 1 minus the tax rate, we obviously expect coefficients with signs opposite to those presented in Figure 4 and Table 3. Figure 5 shows results for the event study validation. There are some indications of suspicious pre-base-year effects in the model with only demographic controls, but these disappear as more controls are included in the model. Table 4 provides the full set of estimation results for years $b+2$, $b+3$ and $b+4$. Again, the estimated parameters are stable across

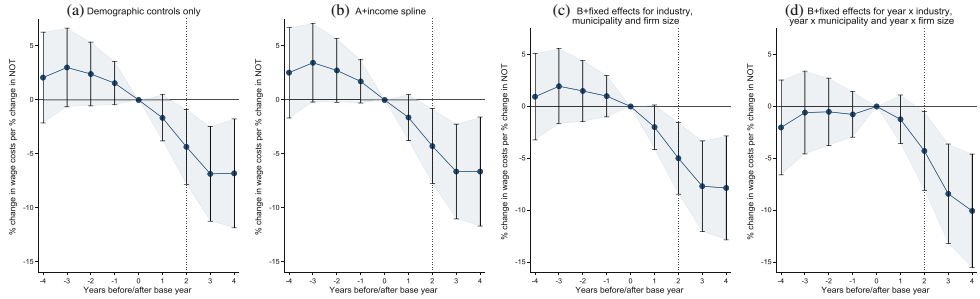

FIGURE 5 The estimated effects of potential net-of-tax rate in year $b+2$ on total wage costs in years from $b-4$ to $b+4$. *Notes*: The graphs show the estimated δ coefficients from equation (2) when the dependent variable is the relative change in the owner-weighted total wage-bill from the base year to the outcome year and the explanatory tax variable is log(net-of-tax rate). The number of observations is 460,262. See Notes to Figure 4 for a detailed description of the different models.

TABLE 4 Estimated effects of potential net-of-wealth tax on total wage costs

Effect in:	Year fixed effects and demographic controls (A)	A + income spline with 7 knots (B)	B + fixed effects for industry, municipality and firm size (C)	B + fixed effects for industry by year, municipality by year and firm size by year (D)
$b+2$	−5.057***	−4.895***	−5.439***	−4.836***
	(1.567)	(1.568)	(1.563)	(1.696)
R-squared	0.018	0.019	0.034	0.064
Number of observations	460,191	460,191	459,610	458,924
$b+3$	−6.397***	−6.221***	−7.016***	−7.752***
	(1.987)	(1.988)	(1.974)	(2.137)
R-squared	0.022	0.022	0.043	0.071
Number of observations	458,463	458,463	457,885	457,197
$b+4$	−5.712**	−5.425**	−6.298***	−8.607***
	(2.282)	(2.285)	(2.261)	(2.444)
R-squared	0.023	0.024	0.049	0.075
Number of observations	456,765	456,765	456,190	455,500

Notes: Standard errors (in parentheses) are clustered at the person/household level. The dependent variable is the relative change in the owner-weighted total wage bill from the base year to the outcome year. Firms that close down after the base year are interpreted as having zero employment. The reported estimates are the δ coefficients in equation (2). For a detailed description of the control variables included in each model, see Notes to Figure 4. The total number of observations is 460,262, but the numbers used in each regression are slightly lower as some owners are no longer alive (or resident in Norway) in the respective outcome years, and some of the fixed effects are unique for specific observations.

*, **, *** indicate statistical significance at the 10%, 5%, 1% level, respectively.

the different models, and all the coefficients indicate statistically significant negative effects of the net-of-tax rate (positive effects of the wealth tax rate).

To compare the implications of the two models, we compute, for all individuals/households in our data, the implied employment (wage cost) effects of moving from the tax regime with the highest to the lowest average wealth tax, that is, from the 2008 regime to the 2017 regime. On average, the difference in the wealth taxes between these tax regimes constituted 0.6% of the owner-weighted wage costs. Based on our main specification (equation (5A)), a wealth tax reduction of this size is predicted to cause a 0.4% drop in employment (total wage costs) in the closely held firms in the tax year ($b+2$). Based on the log(net-of-tax rate) specification (equation (5B)), the predicted employment drop is slightly larger, that is, 0.5%. In 2015, the closely held firms in our data employed approximately 259,000 (full-time-equivalent) workers. Measured in sheer numbers, a wealth tax reduction corresponding to the changes from the 2008 to the 2017 tax regime is predicted to eliminate somewhere between 1000 and 1300 jobs, less than 0.06% of the total number of 2.05 million (full-time-equivalent) jobs in Norway. Hence, from a macroeconomic viewpoint, the estimated employment effects of the wealth tax operating through closely held firms are almost negligible.

In Online Appendix C, we present results for the main specification based on alternative data restrictions on the initial firm size, including a model where we add self-employment income into the definition of the wage bill (dropping the requirement of at least one employee). Despite considerable changes in size as well as composition of the estimation samples, with sample sizes varying from 107,669 (only firms with more than NOK 2.5 million in base-year wage costs) to 686,841 (all firms with more than NOK 0.5 million in wage costs, including self-employment income), the main results are stable across the different data cuts. In Online Appendix D, we present results based on firms that are fully owned by single families (57.1% of the observations) and based on data where we treat firms owned jointly by two families as single observations. Both these analyses indicate somewhat larger employment effects than those shown in the present section.

6.2 | Alternative employment measures and the role of family workers

In this subsection, we take a closer look at the composition of the identified employment effects in terms of labour supplied by family and non-family workers, and in terms of extensive versus intensive margins. To examine the role of own family, we define two additional outcomes te be used in equation (2), namely, $y_{i,b+s} = \left(E^{FAM}_{i,b+s} - E^{FAM}_{i,b}\right)/E_{i,b}$ (the change in wage costs related to family members) and $y_{i,b+s} = \left(E^{NOFAM}_{i,b+s} - E^{NOFAM}_{i,b}\right)/E_{i,b}$ (the change in wage costs related to non-family), and use the model with all explanatory variables included, that is, the model described in column (D) of Table 3. For expository reasons, we present the estimation results graphically; see Figure 6. Figure 6(a) shows results for the employment outcome used in the previous subsection (total wage bill), with the overall employment effect repeated from Table 3. In the tax year ($b+2$), the employment effect is approximately equally split between family and non-family, whereas the non-family component becomes a little bigger in the subsequent years.

The apparent non-negligible role of within-family employment responses may raise questions about the appropriateness of using total wage costs as a measure of productivity-adjusted employment. Could higher wage costs reflect higher wages (possibly implemented to pay for the higher tax) rather than higher labour input?[14] To examine this question, we redefine the employment outcome variable (E) such that it measures the total number of contracted hours worked instead.[15] As can be seen from Figure 6(b), the estimated effect pattern for contracted work hours is similar to that based on total wage costs, suggesting that the identified effects indeed reflect labour input rather than wage adjustments.

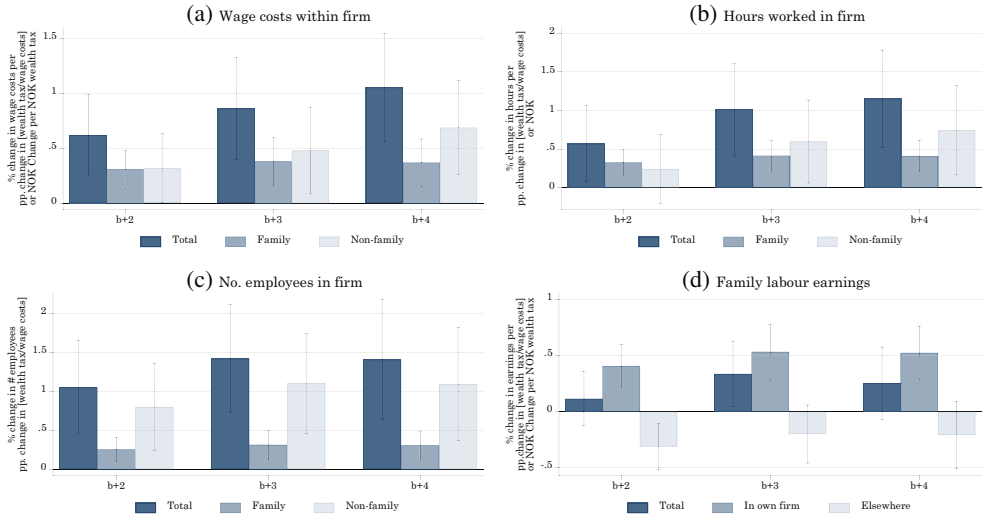

FIGURE 6 The estimated effects of potential wealth tax on employment in the family-controlled firm and elsewhere (with 95% confidence intervals) *Notes*: In panel (a), the estimates denoted "Total" are repeated from column (D) of Table 3. The estimates denoted "Family" and "Non-family" are based on the same model, but with the outcomes defined in terms of wage costs paid out to the owners' own families and to non-family members, respectively (still normalized with total wage costs). In panel (b), the estimates are based on regressions using changes in reported work hours as outcome instead of changes in total wage costs. In panel (c), they are based on regressions using changes in the total number of registered employees (regardless of work hours) instead. To ensure comparability with the results in panel (a), the dependent variables in the regressions reported in panels (b) and (c) are respectively normalized with hours worked and total number of employees in the base year. In panel (d), the reported estimates are based on the same models as in panel (a), but with the dependent variable defined in terms of the family's total labour earnings (also outside the firm). The total number of observations is 455,681, but the numbers used in each regression are slightly lower as some owners are no longer alive (or resident in Norway) in the respective outcome years. The reported confidence intervals are based on standard errors clustered at the owner level.

To examine the margins of the employment responses, we redefine the outcome so that it measures the relative change in the total number of employees (i.e. pure head count regardless of work hours). The result is shown in Figure 6(c). The estimated effect on the overall number of employees is considerably larger than the effect on total labour input (measured by either wage costs or contracted hours), particularly for the non-family part. Hence it appears that the marginal employees tend to work less than full hours through the whole year.

As the identified employment effect of the wealth tax is partly attributable to the owner-family's own labour supply, it is of some interest to investigate whether more labour supplied within the closely held firm means less labour supplied elsewhere. If not, then our findings suggest a total increase in labour supplied by households subjected to higher wealth tax, thus indicating some sort of income effect. To investigate this hypothesis, we define a new outcome capturing the family's total earnings, as well as the respective contributions from work within the closely held firm and work outside. The result indicates that the wealth tax has a (borderline significant) positive influence on the owner-family's total labour supply, suggesting that there may indeed be a positive income effect on labour supply caused by a higher wealth tax; see Figure 6(d).

6.3 | The role of liquidity constraints

The apparent dominance of positive employment effects does not imply that liquidity constraints are irrelevant for all firms. For owners with little liquid wealth, the tax liability may still

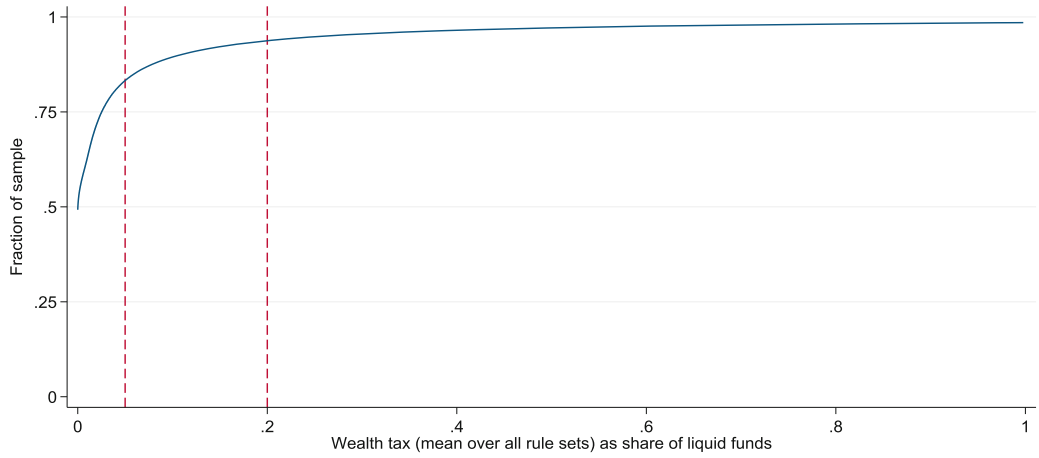

FIGURE 7 The distribution of owner illiquidity in relation to wealth tax exposure *Notes*: Illiquidity is defined as average expected wealth tax liability (calculated for all base years and over all the 11 tax regimes that have existed in our data period) relative to the taxpayer's liquid assets. The vertical lines indicate our grouping into good liquidity (average wealth tax less than 5% of liquid assets), medium liquidity (average tax between 5% and 20% of liquid assets) and poor liquidity (average tax more than 20% of liquid assets).

generate a negative association between the wealth tax level and the firm's employment growth, as the owner may be forced to pull capital out of the firm in order to pay the tax. Most of the taxpayers in our dataset are not subjected to severe liquidity constraints. This is illustrated in Figure 7, where we show the distribution of the average expected wealth tax liability (calculated for all base years and over all the 11 tax regimes that have existed in our data period) relative to the taxpayer's liquid assets (defined as bank deposits, listed shares and fund shares). For 75% of owners, the average tax liability calculated this way constitutes less than 3% of liquid assets. For an additional 8% of owners, it constitutes less than 5%. To see how owner liquidity may influence the employment effects of the wealth tax, we divide the owners into three categories, demarcated in Figure 7 by the vertical dotted lines: (i) owners with sound liquidity relative to the potential tax burden, defined as average tax liability constituting less than 5% of liquid assets; (ii) owners with medium liquidity, defined as average tax liability between 5% and 20% of liquid assets; and (iii) owners with poor liquidity, defined as average tax liability above 20% of liquid assets.

As we measure liquidity relative to the prospective wealth tax, it is important to bear in mind that variations in liquidity may result from variations in the wealth tax as well as from variations in available economic resources. In particular, households with zero wealth tax have good liquidity by definition. Table 5 provides descriptive statistics for the three liquidity categories. It is clear that those with poor liquidity are on average much wealthier than those with good liquidity. This reflects that the typical poor-liquidity household in our data is a household with considerable taxable wealth, but with most of it placed in the family business.

We estimate the effects of the wealth tax separately for each of these three owner groups, again relying on the model with all covariates included. The results are shown in Figure 8, together with the estimated effects for the whole sample repeated from column (D) of Table 3. For the majority of owners with good liquidity, the positive effects of the wealth tax become considerably larger than in the total sample. For owners with medium or poor liquidity, the estimates become smaller and statistically insignificant. Point estimates actually indicate a negative effect for owners with poor liquidity, particularly in the year of the tax liability. Hence although higher wealth tax improves the incentives for investing more savings into the firm, we cannot rule out that liquidity constraints prevent some owners from doing that.

TABLE 5 Descriptive statistics analysis data

	Good liquidity	Medium liquidity	Poor liquidity
Panel A: Type of owner/household ($N = 455{,}681$)			
Married couples	0.65	0.66	0.54
Single male	0.29	0.29	0.40
Single female	0.07	0.05	0.05
Age (mean)	48.7	52.6	51.3
Panel B: Household characteristics ($N = 455{,}681$)			
Gross wealth before valuation rebates (1000 NOK)	7407	18,100	25,724
Gross wealth, tax value (1000 NOK)	3801	12,141	19,197
Net wealth before valuation rebates (1000 NOK)	4572	16,024	22,753
Net wealth, tax value (1000 NOK)	966	10,063	16,227
Potential wealth tax (1000 NOK)	13.9	89.6	150.8
Liquid assets (1000 NOK)	952	1008	311
Potential wealth tax rate (% net taxable wealth)	0.11	0.45	0.53
Potential wealth tax relative to (owner-weighted) wage costs (%)	0.01	0.03	0.04
Panel C: Firm characteristics (weighted by owner share) ($N = 455{,}681$)			
Total wage bill (1000 NOK)	1857	3578	5149
… accounted for by own family	425	520	509
Total employment (full-time equivalents)	4.37	8.03	10.78
… accounted for by own family	0.75	0.87	0.82
Number of observations (panels A–C)	379,329	47,751	28,601
Panel D: Firm characteristics, limited liability companies only (weighted by owner share) ($N = 405{,}003$)			
Tangible assets (1000 NOK)	894	2237	3444
Liquid assets (1000 NOK)	1006	2929	3649
Dividend payments to owner (1000 NOK)	170	529	669
Salary to owner (1000 NOK)	487	536	518
Number of observations (panel D)	330,620	46,273	28,110

Notes: The term 'potential wealth tax' is used to indicate the wealth tax liability based on the level and composition of wealth two years before the respective tax years. 'Good liquidity' is defined as the potential wealth tax (averaged over all tax regimes) constituting less than 5% of liquid assets. 'Medium liquidity' is defined as the potential wealth tax between 5% and 20% of liquid assets. 'Poor liquidity' is defined as the potential wealth tax exceeding 20% of liquid assets. Data reported in panel D are available only for limited liability companies (not for sole proprietorships), implying that approximately 11% of the observations are lost when variables in this panel are used as outcomes.

6.4 | Effects on savings and investment behavior

How can we rationalize a positive effect of the wealth tax on employment in the taxpayers' businesses? We see two possible explanations. The first is that the wealth tax triggers portfolio composition responses designed to reduce the actual tax liability, and such responses entail more resources spent on intangible firm assets such as its human capital. The second is that the wealth tax has a positive effect on overall capital accumulation due to a strong income effect, as suggested by Ring (2020a). In this subsection, we take a closer look at these possible explanations by examining household savings behaviour and financial transactions between firms and households. The analysis is based on tax and wealth data for households and accounting data for firms. The latter are available for limited liability companies only, and also for a shorter time period.

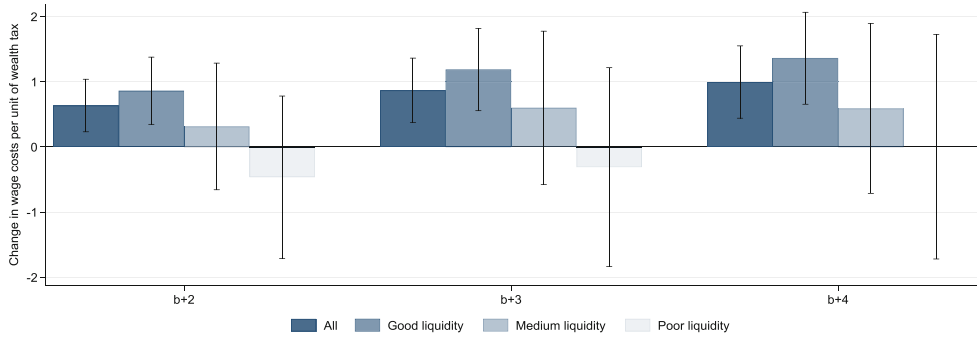

FIGURE 8 The estimated effects of potential wealth tax on total wage costs, by owner's liquidity *Notes*: The graphs show the estimated δ coefficients from equation (2) when the dependent variable is the relative change in the owner-weighted total wage bill from the base year to the outcome year, and the vector of control variables corresponds to those used in column (D) of Table 3; see Notes to Table 3 for a detailed description. The number of observations is reported in Table 5. Point estimates are reported with 95% confidence intervals. Standard errors used to compute the confidence intervals are clustered at the owner level.

Given data limitations as well as the expectation that financial transactions respond more quickly than employment to changes in the tax environment, we focus exclusively on outcomes measured in the year of predicted tax liability ($b+2$) in this subsection. In light of the apparent importance of liquidity constraints for the estimated employment effects, we report separate results by owner liquidity.

We first use a version of equation (2) to examine the impact of the potential tax liability on the actually paid tax. In this case, we normalize the variables by the owner's net wealth rather than by the firm's employment, such that $y_{i,b+2} = T\left(\mathbf{w}_{i,b+2}, \boldsymbol{\tau}_{b+2}\right)/NW_{i,b}$ and $f_b\left(T\left(\mathbf{w}_{i,b}, \boldsymbol{\tau}_{b+2}\right)\right) = T\left(\mathbf{w}_{i,b}, \boldsymbol{\tau}_{b+2}\right)/NW_{i,b}$ in equation (2), where $NW_{i,b}$ denotes the net value of the wealth of owner i in the base year, before valuation rebates. To ensure a meaningful normalization and to reduce outlier problems, we require net wealth to exceed NOK 100,000 in the base year, that is, we drop owners with negative or very small net wealth (13% of the sample).

Again, we are interested in the δ parameter in equation (2), which can now be interpreted as the effect of the potential $b+2$ wealth tax (given the wealth level/composition in b) on the actually realized tax liability in $b+2$. The estimation results are provided in panel A of Table 6. They indicate that a NOK 1 increase in potential wealth tax (given initial wealth) implies a NOK 0.5–0.6 increase in the *actual* wealth tax liability. The estimates are similar across the liquidity groups. The finding of a coefficient considerably below unity may indicate that taxpayers deliberately adjust the wealth composition in order to minimize the tax, perhaps by allocating more assets into the family-controlled firm. However, there are annual fluctuations in household wealth unrelated to the wealth tax also, and such fluctuations imply that $\mathbf{w}_{i,b+2} \neq \mathbf{w}_{i,b}$ and thus push the effect of potential on actual wealth tax below unity. Hence, in order to shed light on how a given tax regime affects the accumulation and composition of wealth, we need to look more directly at these outcomes.

We start this part of the analysis by examining overall wealth accumulation. We define savings as the change in net wealth (before valuation rebates) from the base year to the outcome year; such that $y_{i,b+2} = \left(NW_{i,b+2} - NW_{i,b}\right)/NW_{i,b}$. As most of the tax is typically paid during the tax year (although it is permissible to pay it the year after), this definition implies that our savings measure incorporates the mechanical (negative) effect of the tax payment.[16] It should also be noted here that net wealth is imperfectly measured. While we take the various tax-rebates described in Table 1 into account, we cannot adjust for the fact that non-listed businesses are notoriously undervalued. Hence if higher wealth tax triggers a reallocation of wealth toward the family business, then this may show up in a negative estimated savings effect. Moreover, a higher wealth

TABLE 6 The Estimated effects of potential wealth tax on savings and investment behaviour (year $b + 2$)

	All	Good liquidity	Medium liquidity	Poor liquidity
Panel A: Actual wealth tax liability				
Effect estimate (standard error)	0.504***	0.569***	0.620***	0.537***
	(0.011)	(0.012)	(0.038)	(0.062)
R-squared	0.5249	0.4714	0.5058	0.5210
Number of observations	394,888	318,714	45,753	26,291
Panel B: Wealth accumulation				
Effect estimate (standard error)	0.042	0.143***	−0.194*	−0.121
	(0.042)	(0.046)	(0.106)	(0.153)
R-squared	0.0643	0.0685	0.2494	0.3118
Number of observations	394,892	318,718	45,753	26,291
Panel C: Fraction of wealth in non-listed shares				
Effect estimate (standard error)	0.225***	0.230***	0.063	0.112*
	(0.014)	(0.016)	(0.041)	(0.066)
R-squared	0.2835	0.2164	0.5978	0.6278
Number of observations	396,767	322,029	44,951	25,668
Panel D: Capital flow from firm to owner				
Effect estimate (standard error)	−0.779***	−0.690***	−0.887	−0.931
	(0.218)	(0.265)	(0.552)	(0.711)
R-squared	0.1439	0.1419	0.2794	0.3313
Number of observations	403,611	329,194	44,334	25,937
Panel E: Investment in tangible assets				
Effect estimate (standard error)	0.096	0.195	0.417	−0.674
	(0.189)	(0.245)	(0.482)	(0.617)
R-squared	0.0633	0.0685	0.2073	0.2835
Number of observations	403,611	329,194	44,334	25,937

Notes: Standard errors (in parentheses) are clustered at the person/household level. The dependent variable in panel A is the actual wealth tax liability in year $b + 2$ divided by net wealth in year b. The reported coefficients are the estimated effects of potential tax liability in year $b + 2$, given the wealth in b, also divided by the net wealth in year b. The dependent variable in panel B is the relative change in the owner's net wealth from year b to year $b + 2$. The reported coefficients are the estimated effects of potential tax liability in year $b + 2$, given the wealth in b, divided by the owner's net wealth in year b. The dependent variable in panel C is the fraction of net wealth held in unlisted shares in year $b + 2$. The reported coefficients are the estimated effects of potential tax liability in year $b + 2$, given the wealth in b, divided by the owner's net wealth in year b. The sample in panels A and B is restricted to owners with net wealth exceeding NOK 100,000 in the base year. The sample in panel C is restricted to owners with net wealth exceeding NOK 100,000 in the outcome year. The dependent variable in panel D is the dividends paid out from the firm to the owner in year $b + 2$ minus the change in paid-up equity from b to $b + 2$, divided by the firm's (owner-weighted) wage bill in b. The reported coefficients are the estimated effects of potential tax liability in year $b+2$, given the wealth in b, divided by the firm's (owner-weighted) wage bill in b. The dependent variable is the change in the (owner-weighted) value of tangible assets in the firm from b to $b + 2$, divided by the firm's (owner-weighted) wage bill in b. The reported coefficients are the estimated effects of potential tax liability in year $b + 2$, given the wealth in b, divided by the firm's (owner-weighted) wage bill in b. All models include all control variables described in column (D) of Table 3; see Notes to Table 3 for a detailed description. *, **, *** indicate statistical significance at the 10%, 5%, 1% level, respectively.

tax may increase incentives for transferring wealth to adult offspring (or other family-members who are taxed separately), which could also bias the estimated wealth accumulation effect downwards.

The estimation results are provided in panel B of Table 6. For the sample as a whole, we do not find evidence for either a positive or a negative effect on savings. This result appears to conceal some heterogeneity, however. While we find positive effects for owners with good liquidity, we estimate negative effects for owners with medium liquidity. Given that any wealth reallocation responses will bias in the estimated savings effect downwards, we cannot rule out positive average savings effects. However, the results reported in panel B do indicate that positive savings effects are unlikely to be the primary mechanism behind the identified employment effects.

In order to look more closely at possible wealth reallocation effects, we use the fraction of net wealth placed in non-listed shares as an alternative outcome, such that $y_{i,b+2} = NLS_{i,b+2}/NW_{i,b+2}$, where $NLS_{i,b+2}$ denotes the assessed market value of non-listed shares in the outcome year (book value, excluding goodwill and patents, minus debt). We find that the wealth tax positively affects the share of wealth allocated into tax-favoured non-listed businesses, most evidently for owners with good liquidity; see panel C of Table 6.

For owners of limited liability firms, we also examine the capital flows between owners and firms more directly. To do this, we use as an additional outcome the dividends paid out to the owner in the tax year minus the change in paid-up equity from the base year to the tax year. We think of this as a firm-level variable and thus normalize with base-year firm size, such that $y_{i,b+2} = CF_{i,b+2}/E_{i,b}$, where $CF_{i,b+2}$ denotes the capital flow from the firm to the owner (dividends in outcome year minus paid-up equity since the base year). In accordance with the portfolio composition hypothesis, we find that the wealth tax reduces the take-out of capital from the firm (or increases the paid-up equity); see panel D of Table 6. For each NOK increase in potential wealth tax, the net capital flow from the firm to the owner is estimated to decline by approximately 0.8 units. Similarly to the effects estimated for employment, the positive effects on capital allocated to the firm are significant only for firms with good liquidity.

As a final assessment of possible mechanisms behind the positive employment effects, we examine the effect of the wealth tax on investment in a firm's tangible assets. To the extent that the increased money available to the firm is tax-motivated, we do not expect to find large effects on tangible assets, as such investments (in contrast to investment in intangible assets) do show up in the balance sheet, and hence become subjected to the wealth tax (although with a rebate in some years; see Table 1). We define investment as the change in the reported value of tangible assets from the base year to the outcome year, and normalize with the base-year size of the firm, that is, $y_{i,b+2} = \left(PC_{i,b+2} - PC_{i,b}\right)/E_{i,b}$, where $PC_{i,b+2}$ denotes the book value of tangible assets in the outcome year. The estimation results are provided in panel E of Table 6. Although point estimates are positive (except for owners with poor liquidity), there is no statistically significant evidence suggesting that the wealth tax affects investments in tangible assets.

7 | CONCLUDING REMARKS

As with all redistributive taxes, the wealth tax creates behavioural distortions. The research literature has focused primarily on how a wealth tax distorts decisions regarding consumption and saving through a substitution effect. In addition, there is a literature focusing on credit-constrained businesses and the risk that a wealth tax imposed on owners may drain their firms for economic resources and reduce employment. In the present paper, we have examined the empirical relationship between the level of the wealth tax and subsequent employment growth in the taxpayers' closely held firms. On average, we have found no support for a negative effect of a moderate wealth tax on employment in firms controlled by the taxpayers. To the contrary, we have identified a statistically significant positive causal relationship between wealth tax liability

and employment, operating partly through adjustment of family members' own labour supply. A positive employment effect can be explained by a strong income effect. However, although we have found some indications of an income effect for members of the taxpaying family, this effect does not appear to be of sufficient magnitude to raise overall household savings net of the tax. Our results point to another mechanism as the major causal channel, namely that the wealth tax influences the portfolio composition of assets. The portfolio composition effect arises because it is almost impossible for tax authorities to assess the true market value of non-listed firms that are not traded in a market, implying a tendency for such firms to obtain a tax value well below their true market value. This gives firm owners a tax-based incentive for allocating their wealth and labour into the firm, and this incentive becomes stronger the higher is the (marginal) wealth tax.

Although the portfolio composition effect appears to dominate the overall causal relationship between the wealth tax and the employment growth in closely held firms, our analysis confirms that credit constraints may generate negative employment effects in firms owned by households with poor liquidity relative to the size of the wealth tax. A typical example may be a family that has inherited a firm with high tax value, but otherwise has limited financial resources. Hence there is no single and unambiguous answer to the question of how changes in the wealth tax influence employment in small and medium-sized businesses.

Although we have identified a positive relationship between wealth tax liability and employment in closely held firms, we emphasize that our analysis is narrow in the sense that it does not provide answers to more general questions about the wealth tax's effects on overall employment, entrepreneurship or economic growth. Such questions would also involve comparisons of complete tax systems, which is beyond the scope of this paper.

Our results suggest that a wealth tax distorts investments towards human capital and other intangible assets in family-controlled businesses. The distortion affects both the allocation of savings between closely held firms and other assets, and the labour to capital ratio within firms. Whether or not this is desirable from a social efficiency point of view depends on the existence of other distortions, and in particular, on the extent to which the distribution of taxes between capital and labour is considered optimal in the absence of the wealth tax.

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NOTES

[1] Until 2016, all debt was fully deductible. Since 2017, debt related to discounted assets other than primary homes is deducted at reduced value.

[2] From 2014, there is no inheritance/gift tax in Norway.

[3] Note that the tax function applying before 2009 was a bit more complicated than suggested by the first equality in equation (1), as the system then had a more progressive structure, with a top rate applying for wealth exceeding a second threshold. Such a progressive system was reintroduced in 2022.

[4] Note that the tax variables included as controls are indexed by absolute years 2007–17 and hence are distinct from the tax rate of interest that is indexed relative to the base year.

[5] Also, while the taxable income literature often uses predicted tax rates (based on initial income) as instruments for actual tax rates, we use the predicted tax level (based on initial wealth) itself as the causal variable. In our case, an instrumental variables strategy is ruled out because we do not think of the actually paid wealth tax as the explanatory variable of interest, but rather use the potential wealth tax, calculated for the initial structure of wealth. The actually paid tax is instead considered as an outcome.

[6] In Online Appendix E, we show this in more detail by deliberately inducing a measurement error into the wage cost variable.
[7] In (the many) cases with non-positive net wealth, there is obviously no wealth tax, and we naturally define $\ln\left(1-\left(T\left(\mathbf{w}_{i,b},\boldsymbol{\tau}_t\right)/NW_{i,b}\right)\right)$ to be zero.
[8] If a household controls more than one firm with between 1 and 100 employees, then we include only the largest one.
[9] We also present results based on the complete (non-trimmed) dataset. Online Appendix A explains in more detail how the baseline dataset has been constructed.
[10] We compute total net wealth by reversing the various tax valuation rebates built into the tax system; see Table 1. For the years before 2010, we first estimate the 2009 housing value by assigning a relative increase in taxable share (taxable value in percentage of market value) from 2009 to 2010 equal to the observed change in the median tax value within each census tract. We then calculate the value for earlier years based on the annual adjustment factors reported in Table 1. However, we are not able to compute market values for non-listed firms; hence the measure of net wealth used in our analysis will underrate the true value of wealth for most business owners. The only change in tax valuation for which we are not able to account is the change in valuation of real estate owned through unlisted firms (which affects the taxable wealth of the shareholders).
[11] Note that the regressions on past outcomes entail a simultaneity problem, as previous employment growth is likely to have influenced the base year's wealth and the imputed wealth tax. However, the resultant correlation between the potential wealth tax and the error term is controlled for by the counterfactual tax variables. Note also that the interpretation of a positive coefficient in a year prior to the base year would indicate a decline in employment. In Online Appendix F, we report the results from an alternative event study where we have defined the outcomes symmetrically as annual changes in employment.
[12] We report results without winsorization in Online Appendix B.
[13] In Online Appendix B, we also present results based on categorization of both the explanatory tax variables and the outcome variable.
[14] There is also some empirical evidence suggesting that taxes paid by a firm owner may negatively affect the earnings growth of employees (Risch 2020).
[15] To ensure comparability with the results for wage costs, we still normalize the tax variables (the right-hand side of the equation) with initial wage costs.
[16] Adding in the actual tax liability in $b+2$ as part of the savings outcome does not change the estimates to any noticeable extent, however.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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