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DISCUSSION PAPER SERIES

IZA DP No. 13766

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# ABSTRACT

## Does the Wealth Tax Kill Jobs?

Fueled by increasing inequality and rising fiscal deficits, the interest in wealth taxation has increased over the last years, both in the public debate and in academia. Yet, knowledge about the behavioral effects of a wealth tax is limited. 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' investment and employment decisions. Identification of causal effects is based on a generalized difference-in-differences strategy. We find no empirical support for the claim that a moderate wealth tax adversely affects investments and employment in firms controlled by the taxpayers. To the contrary, our results indicate a positive causal relationship between the level of a household's wealth tax and subsequent employment growth in the firm it controls. The rationale behind this result appears to be that the tax value of a given wealth can be reduced by being invested in a non-traded firm, and that this incentive becomes stronger the higher is the wealth tax.

**JEL Classification:** H21, J23, G11

**Keywords:** wealth tax, capital taxation, labor demand, investment

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# 1 Introduction

After the abolishment 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; Atkinson, 2015; OECD, 2018; Saez and Zucman, 2019; Scheuer and Slemrod, 2020; Bastani and Waldenström, 2020). Wealth inequality is far greater than income inequality. It tends to be self-reinforcing, because wealthy people can more easily set aside funds for new investment, because they have access to better investment opportunities, and because wealth is transferred across generations. From an egalitarian perspective, there are therefore good reasons to maintain or reintroduce some form of a wealth tax. However, as all redistributive taxes, a wealth tax creates behavioral distortions. A particular concern is that it discourages savings and investment and drags down economic growth. Existing empirical evidence indicates indeed a considerable negative impact of the wealth tax on reported taxable wealth, but also that this effect primarily reflects tax avoidance rather than real changes in wealth accumulation (Seim, 2017; Zoutman, 2018; Brülhart et al., 2020; Jakobsen et al., 2020). Recent evidence from Norway actually 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 is the wealth tax's possible influence on entrepreneurship and growth of small businesses. Although the wealth tax is levied on individuals, it will partly be based on firm level assets, and since it has to be paid regardless of current profits, it may force firm owners to extract dividends from the firm in order to pay their personal wealth tax. In a world of asymmetric information and liquidity constraints, the wealth tax could therefore have a direct negative effect on entrepreneurship, employment and investments, and eventually also on productivity growth.

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 to some extend undermine the redistributive purpose of the wealth tax and distort the allocation of resources toward lower-valued (or hard-to-evaluate) assets. However, although the wealth tax therefore represents a source of social inefficiency (in an otherwise undistorted economic environment), it is far from obvious that it reduces investment and employment in family-owned businesses. The difficulty associated with assessing the true value of non-traded assets, such as unlisted and closely held companies, may actually encourage entrepreneurship through such firms, as they provide some scope for tax reduction. This effect is of course strengthened if the tax system gives a tax-rebate on such assets.

Norway is one of the very few countries that still has an annual net wealth tax levied on individuals. The tax is highly controversial, though, and it was one of the main topics in the public debate leading up to the Norwegian parliamentary election in 2017. The opponents of the tax argued that it is detrimental to the establishment and growth of small and medium sized businesses, as the owners are forced to drain them for resources that could otherwise have been invested in the firm. Moreover, since the tax on non-traded assets is based on their historical book values and not affected by annual returns, it fails to share the entrepreneurs' income risk. Thus, it is argued that the wealth tax increases owners' own risk and, as a result, their required return on equity. As forcefully argued by Johnsen and Lensberg (2014), and later emphasized by a government-appointed commission proposing the abolishment of the wealth tax (NOU, 2018), this implies that the wealth tax discourages profitable investment projects from being carried out and reduces output and growth. This argument has been challenged by Sandvik (2016) and Bjerksund and Schjelderup (2019), who point out that a wealth tax also reduces the expected return on alternative investments and thus the investors' discount rate, such that the willingness to invest in a firm is unaffected. Moreover, variations in the returns to capital are not only a matter of uncertainty and risk; they also arise due productivity differences across entrepreneurs and projects. Compared to a tax on the returns to capital, the wealth tax then shifts the tax burden toward the less productive entrepreneurs and projects, and thus encourages investment among the productive ones (Guvenen et al., 2019). This may enhance output and growth.

The purpose of the present paper is to examine empirically the influence of the wealth tax on (potential) taxpayers' investment and job creation/destruction in small and medium sized firms. To identify the causal effects of the wealth tax, we exploit a number of recent reforms that have modified the wealth tax through three different margins; i.e., the lower threshold, the valuation rules, and the tax rate. In particular, as we describe in detail in Section 2, there has been two waves of wealth tax reforms, during 2005-2011 and 2013-2017, respectively. While the first period brought increases in the wealth taxation for the wealthy, combined with increased thresholds that reduced the taxes at lower wealth levels, the second period was characterized by reductions in the wealth taxation across the wealth distribution. Our identification strategy is based on a difference-in-differences approach, where we regress the outcomes of interest on predicted future wealth tax liability derived from the initial wealth level and the actually existing 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 regimes in all years, but identify the causal part as the "extra" effect associated with the wealth tax actually applying.

Our results do *not* indicate that the wealth tax kills jobs in companies owned by the taxpayers. To the contrary, we identify a positive relationship between the wealth tax level and employment growth in small and medium sized family-run businesses. The positive employment effect of higher wealth taxes can be rationalized by a positive income effect on household savings, as well as by the fact that the wealth tax strengthens economic incentives to invest in assets that reduce taxable wealth. Family-owned businesses appear to be useful for tax avoidance purposes. Given the difficulty of assessing the true value of non-traded economic activities, the existence of a wealth tax provides an incentive to allocate wealth into such activities rather than into other (and more easy-to-assess) assets. Since the human capital embedded in employees does not enter into a firm's balance sheet, raising employment in a family-owned business appears to be a particularly convenient strategy for wealth tax reduction. However, our finding of a positive employment effect on average does not mean that liquidity problems created by the wealth tax are irrelevant. For a small subset of liquidity-constrained business owners, we indeed identify negative employment effects.

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 Hurst and Lusardi (2004), who find that the positive relationship between entrepreneurship and wealth in the US 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; Fairly 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 for 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 behavior. A notable exception is Berzins et al. (2020), who examine the effect of the Norwegian wealth tax based on regulatory changes in the tax value of shareholder's 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 highlights that a wealth tax may affect the owners' contribution to investment and employment thorough different mechanisms, and thus that the effects of, say, a rise in the wealth tax, may critically depend on the way it is raised. If it is raised such that the incentives for wealth reallocation becomes stronger (e.g., a pure increase in the marginal tax rate), 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 the 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 provide answers to the question of how elimination of the wealth tax would have affected investment and employment. Such a question would in any case involve specification of alternative taxes and general equilibrium effects, given some fiscal budget constraint. Hence, the evaluation of the wealth tax as one element of a nation's overall tax system entails the comparison of complete tax systems. Hansson (2008) makes an attempt in this direction, by exploiting the variation in the existence of a wealth tax across countries to examine its influence on the 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 found 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 individual level net wealth tax levies an annual tax on the individual's net taxable wealth, which consists of total taxable wealth net of debt. The tax applies to the worldwide net wealth of all Norwegian residents. Domestic assets and debt are mostly third-party reported, while assets held abroad are self-reported.

The wealth valuation for tax purposes varies across asset classes, and for some classes the tax value is substantially below the market value, in particular for housing, while debt is in most cases deductible at market value. This renders many individuals with low or negative taxable wealth, even though they can have substantial positive wealth measured at market value. Negative wealth tax liability is not forwarded to future years, but transferred to the spouse for deduction if the spouse has positive payable wealth tax.

Listed shares are valued at their end-of year values. Unlisted shares are valued based on firm's underlying assets and distributed to the individual owners according to ownership shares. However, the valuation of unlisted shares is challenging, and, for example, human capital embedded in its employees is not included in the tax valuation of a firm for owner-level wealth tax purposes. 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 rebate for such firms is 68%. Looking at newly listed firms, they estimate that the rebate 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, such that some of the countries' richer individuals has low or no taxable wealth. In particular, regardless of the initial tax value of a firm, a wealth-tax-exposed person/household can reduce the tax by investing in an unlisted company in the form of employment growth, as the firm's human capital does not contribute directly to its tax value. If the initial tax value of a firm is negative (debt exceeds the tax value of assets), 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.

**Table 1: Wealth tax rates, thresholds, and valuation rules. By tax year.**

| | **Tax rates and thresholds** | | | | **Valuation of assets for tax purposes**<br>PY: % adjustment of previous year's tax value<br>MV: % of assessed market value[1] | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | Tax rate 1 % | Threshold 1 | Tax rate 2 % | Threshold 2 | Primary home[2] | Leisure home[2] | Secondary home[2] | Business property | Listed and unlisted shares |
| 2005[3,4] | 0.90 | 151 000 | 1.10 | 540 000 | PY: 0 | PY: 0 | PY: 0 | PY: 0 | MV: 65 |
| 2006[4] | 0.90 | 200 000 | 1.10 | 540 000 | PY: 25 | PY: 25 | PY: 25 | PY: 25 | MV: 80 |
| 2007[4] | 0.90 | 220 000 | 1.10 | 540 000 | PY: 10 | PY: 10 | PY: 10 | PY: 10 | MV: 85 |
| 2008[4] | 0.90 | 350 000 | 1.10 | 540 000 | PY: 10 | PY: 10 | PY: 10 | PY: 10 | MV: 100 |
| 2009 | 1.10 | 470 000 | | | PY: 10 | PY: 10 | PY: 10 | PY:60/MV:40[5] | 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 | removed | | 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[6] | MV: 80[6] | MV: 90[6] |
| 2018 | 0.85 | 1 480 000 | | | MV: 25 | PY: 0 | MV: 90[6] | MV: 80[6] | MV: 80[6] |
| 2019 | 0.85 | 1 500 000 | | | MV: 25 | PY: 0 | MV: 90[6] | MV: 75[6] | MV: 75[6] |
| 2020 | 0.85 | 1 500 000 | | | MV: 25 | PY: 0 | MV: 90[6] | MV: 65[6] | MV: 65[6] |

[1] Since 2010, assessed market values of housing are based on sales 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 real properties). A "safety valve" applies to all real estates, 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) minus debt.

[2] The division between residential property (primary and secondary home) and leisure home is not based 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.

[3] 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 the double of what is shown in the table.

[4] In 2005-2008, 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 mill.

[5] 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 pct.

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

The Norwegian wealth tax is levied in a setting with dual income tax; a progressive tax on labor 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 (currently 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-free).

Over the years, there have been numerous changes in the wealth tax rules. Table 1 presents wealth tax rates, thresholds, and asset valuation rules for tax purposes for the period 2005-2020.

The changes in the wealth tax rules can be divided into three types:

1. Reduced rates. The top marginal wealth tax rate was kept constant at 1.1% from 2005 through 2013. It was then reduced to 1% in 2014 and further to 0.85% in 2015. Until 2008, there was a progressive tax rate schedule, with a first tax rate of 0.9% applying at a relatively low wealth levels.

2. Increased thresholds (basic allowances). There has been a substantial increase in the lower tax threshold, from NOK 151,000 net taxable wealth in 2005 to NOK 1,500,000 in 2019 (in nominal terms). From 2006, this is an individual level deduction, such that married couples have a double threshold on their joint net wealth.

3. Changes in valuation. The valuation rules have been changed over the period, initially with an aim of more equal treatment of different asset types. First, a new (increased) valuation of real estate was introduced in 2010. Prior to that, tax valuation of housing was based on historical cost, with an annual stepping up of previous year's tax value, leaving in particular older houses at a very low tax value relative to market value. From 2010 and onwards, the market value of housing is assessed by the Statistics Norway based on market transactions in the same area and on characteristics of the house. For primary housing, the tax value was set to 25% of estimated market value. For secondary housing, the tax value has been raised from 40% in 2010 to 90% in 2017. The valuation discount for shares was 35% in 2005, and it was gradually reduced until it was fully removed in 2008. The discount was then reintroduced for shares, operating assets (included commercial property) and associated debt with 10% in 2017, and increased gradually to 35% in 2020.

Based on the tax rules that applied in 2011, Halvorsen and Thoresen (2020) 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, potentially causing some liquidity problems. However, when evaluated against lifetime rather than annual income, it is shown that the wealth tax is largely born by high-income taxpayers, such that the tax indeed fulfills its redistributive purposes.

# 3 Data and identification strategy

Our analysis is based on administrative register data covering the period from 2005 through 2017 (2015 for data on individual wealth). We combine four blocks of data. The first block contains information about taxable wealth (total wealth and its components) for all adult residents (and households) in Norway. This facilitates accurate computation of the wealth tax according to all the tax rules that have existed in our data period. The second block contains annual accounting data for all limited liability firms in Norway and data on self-employment earnings for sole proprietorships. The third block contains a list of owners of limited liability companies in Norway, including owner shares. And the fourth contains accounts of all employees in Norway, including the identity of their employers and their annual salaries.

As the primary purpose of the analysis in this paper is to examine the impacts of the wealth tax on employment and investments in small and medium sized family-controlled firms, we combine these four data blocks to establish an analysis data set consisting of firms and owners that fall into this category. More specifically, we establish analysis datasets based on two criteria. The first, which we apply throughout our empirical analysis, is that a firm is controlled by a single person or household (owner share at least 50%), with less than 100 mill. NOK (approximately 10 mill. Euros) in net (market-valued) wealth. The second is that the firm has at least one year with employment between 1 and 100 person-years (the lower threshold requires an annual wage cost exceeding NOK 500,000, measured in 2015-value). In the baseline version of the model, we do not include the owner's self-employment income in our definition of wage cost, implying that most sole proprietorships are dropped from the analysis. We then end up with 460,585 firm-household-year observations to be included in our empirical analysis; see the next section for descriptive statistics. In robustness analyses, we provide results for models with sole proprietorships included in the analysis and for models based on a range of alternative (initial) firm-size limitations. In Appendix A, we describe in more detail how we have constructed our main dataset, and show descriptive statistics for the alternative samples used in the robustness analyses

By construction, there is in our analysis data a perfect correspondence between households and firms. It is instructive, however, to think of the household as the unit of observation, as the wealth tax is imposed at the household level. All firm variables (including employment and investment) 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.

Our empirical model portrays an owner *i* considering some economic decision (e.g., new investments or hiring/firing) over a period *t*. This decision is potentially influenced by many factors, including the size of *i's* initial wealth and the way it is subjected to taxation. Hence, we will set up regression models where various firm and owner outcomes are functions of future wealth tax liability, given the initial level and structure of the wealth. Our model is framed in terms of a base-year and a series of outcome years. The base-year is the year in which the owner's actual wealth and ownership share is measured, and the year in which we define the criteria for being included in the dataset. In this context, it is essential that the wealth characteristics entering into our model as explanatory variables are exogenous with respect to the tax functions used to identify causal effects. As we measure wealth at end-of-year value, this will not necessarily be the case for the tax function applying for the first year after the base-year. The reason is that this tax function is announced in the base-year, giving the taxpayer some room for adaptation before the end of the year. As the tax-value of non-listed shares is determined based on start-of-year book-value, there may indeed be some incentives for doing that. Hence, the first tax function that can be considered strictly exogenous with respect to the end-of-year wealth measured in the base-year is the tax schedule applying for year 2 after the base-year. We will therefore use the potential wealth tax calculated for the second year after the base-year as the key explanatory variable in our model, and we will investigate its effects on outcomes in that same year and in the subsequent two years.

Note that we are *not* seeking to identify the effects of actually paid wealth tax, as the actual tax liability is endogenously influenced by the agents' own savings and investment decisions. Rather, we focus on how a particular tax regime superimposed on a given predetermined wealth affects subsequent economic decisions, such as investment and employment in the owner's firm.

Given the heavily skewed distribution of owners' wealth as well as of outcomes such as employment and investment, a regression analysis needs to deal with challenging functional form issues and outlier problems. Our main strategy will be to normalize all variables with the firm's (owner-weighted) total wage bill in the base-year, such that both explanatory variables and outcomes are measured per unit of the initial wage cost. Alternative strategies will be presented in Appendix.

Since the level of taxable wealth in the base-year, as well as its portfolio composition, is likely to have its own direct effects on future outcomes, and also to be correlated with a range of other unobserved variables with such effects (such as entrepreneurial ability and risk-preferences), we face a serious identification problem. Within a regression framework, we can of course control for initial wealth characteristics, but, without variation in the tax regime, it is clear that separate identification of the influences of the wealth itself (and its correlates) and the influences of the wealth tax will have to rely on functional form assumptions. This is a feeble source of identification, as we have little prior knowledge about the functional form relationship between wealth characteristics and the outcomes of interest.

To deal with this identification problem, we exploit a series of tax reforms in order to isolate the exogenous reform-initiated variation in the wealth tax from all other influences of wealth characteristics and its correlates. To do this, we compute the wealth tax that *would have applied* for the second year after the base-year under all the tax regimes that have existed in our data period, and include them as controls in the regression models.

Let $y_{it}$ be some outcome measured for person/household *i* in year *t* after the base year, let $\mathbf{w_{i0}}$ be a vector characterizing the size and portfolio composition of the (predetermined) base-year wealth, and let $WB_{i0}$ be the wage bill attributed to business owner *i* in the base-year (i.e., the total wage bill of the controlled firm multiplied by owner share). Furthermore, let $T_{\mathbf{s}}(\mathbf{w_{i0}})$ be the wealth tax calculated for the base-year wealth composition according to tax rules applying in year *s*, and let *BY* indicate base-year fixed effects. The models we estimate will then have the following structure

$$\frac{y_{it}}{WB_{i0}} = \delta \frac{T_2(\mathbf{w_{i0}})}{WB_{i0}} + \sum_{s=2007}^{2017} \pi_s \frac{T_{\mathbf{s}}(\mathbf{w_{i0}})}{WB_{i0}} + BY + \text{other controls} + \varepsilon_{it}, \quad t = 2,3,4 \tag{1}$$

We then have – by construction – that $\varepsilon_{it}$ is orthogonal to the potential tax liability $T_2(\mathbf{w_{i0}})$, provided that any unaccounted for relationships between the tax variables and the influence of (or spurious correlation with) wealth characteristics $\mathbf{w_{i0}}$ are stable over time. If this assumption holds, we have ensured that any misspecification of the direct wealth effects and its correlates will be captured by the hypothetical tax functions in their capacity as controls. Equation (1) 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 operates with a lag), the spurious effects will be there regardless of outcome year. This is a kind of generalized or continuous difference-in-differences identification strategy, since we allow the outcome to be affected by wealth taxes calculated according to all possible tax regimes, but identify the causal part as the "extra" effect associated with the wealth tax actually applying.

A similar identification strategy has previously been used 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 labor 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, e.g., 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 has 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.[1]

Our identification strategy relies on the assumption that spurious associations between wealth characteristics and outcomes are stable over time. To assess the validity of this assumption, we can include additional (time-varying) controls in Equation (1). In the empirical analysis, we will first present some "baseline" results based on models with only un-interacted base-year fixed effects, and

[1] Also, while the taxable income literature often use predicted tax rates (based on initial income) as instrument 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 the potential wealth tax, calculated for the initial structure of wealth. The actually paid tax is instead considered an outcome.

then move on to a range of robustness analyses, based on the use of additional control sets as well as different data cuts.

# 4 Descriptive statistics

Table 2 presents descriptive statistics for the sample of households/firms used in our analysis. Approximately 64% of the owners are married couples, 29% are single men, and 7% are single women. On average, these households hold approximately NOK 2.7 million (roughly € 270,000) in net taxable wealth, NOK 6.7 million in (imperfectly) market-evaluated net wealth, and pays 29,000 NOK in wealth tax.[2] The average tax rate is 0.17%, and constitutes approximately 1.6% of the firm's total (owner-weighted) wage costs. However, averages are not particularly informative in this case, as the distributions are heavily skewed. Figure 1 provides a more illuminating picture of the distributions of the wealth tax and its size relative to the (market-evaluated) net wealth as well as to the wage costs in the taxpayers' firms. To give some insight to the consequences of the tax reforms, we show the distributions for three different years; i.e., 2007, 2012, and 2017, in all cases based on the wealth reported two years before. It is notable that the fraction of owners paying any wealth tax at all has declined from approximately 55% in 2007 to 38% in 2017. Relative to wage costs, the wealth tax liability appears to be small; throughout the period covered by our analysis, the fraction of owners paying more than 5% of wage costs in wealth tax has been well below 10%.

To provide some intuition on the variation in tax liability created by the tax reforms, Figure 2 shows, for each percentile in the (market-evaluated) net wealth distribution in 2015, the distribution of differences between the highest and the lowest tax liabilities that can be calculated based on all the tax regimes that have existed between 2007 and 2017. It is clear that the reforms have indeed generated considerable variation in tax liabilities, particularly at higher wealth levels. There is also substantial variation within each wealth percentile. Even at the highest wealth levels, there are owners with zero tax change across the tax regimes (i.e. they do not pay wealth tax under any tax regime). This is due to the fact that some assets are valued well below market value, while debt is in most cases deductible at market value. For the vast majority of firm owners, the difference between the "best" and the "worst" wealth tax regime is well below NOK 50,000.

[2]We compute market values 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 percent 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 market value used in our analysis will underrate the true value of wealth for most business owners. As market values do not play a direct role in the empirical analysis, this has no consequences for the regression analysis. The only change in tax valuation we are not able to account for is the change in valuation of real estate owned through unlisted firms (which affects the taxable wealth of the shareholders).

**Table 2. Descriptive statistics analysis data**

| | Mean/fraction | Median | Standard deviation |
|---|---|---|---|
| **A. Type of owner/household (N=460,585)** | | | |
| Married couples | 0.64 | | |
| Single male | 0.28 | | |
| Single female | 0.07 | | |
| **B. Household characteristics (N=460,585)** | | | |
| Gross wealth, stipulated market value (1,000 NOK) | 9,447 | 6,180 | 11,427 |
| Gross wealth, tax value (1,000 NOK) | 5,448 | 2,890 | 8,807 |
| Net wealth, stipulated market value (1,000 NOK) | 6,710 | 3,856 | 10,259 |
| Net wealth, tax value (1,000 NOK) | 2,712 | 700 | 8,265 |
| Potential wealth tax (1,000 NOK) | 28.7 | 0 | 76.1 |
| Liquid assets (1,000 NOK) | 918 | 304 | 2,419 |
| Potential wealth tax rate (% of net taxable wealth) | 0.17 | 0 | 0.26 |
| Potential wealth tax relative to (owner-weighted) wage costs (%) | 1.61 | 0 | 5.06 |
| **C. Firm characteristics (weighted by owner share) (N=460,585)** | | | |
| Total wage bill (1,000 NOK) | 2,206 | 1,260 | 3,120 |
| Total employment (fulltime equivalents) | 5.05 | 3.07 | 6.59 |
| **D. Firm characteristics limited liability companies only (weighted by owner share) (N=409,412)** | | | |
| Fixed assets (1,000 NOK) | 1,177 | 217 | 7,116 |
| Liquid assets (1,000 NOK) | 1,415 | 304 | 3,402 |
| Dividend payments to owner (1,000 NOK) | 240 | 0 | 873 |
| Salary to owner (1,000 NOK) | 622 | 576 | 450 |

Note: 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.

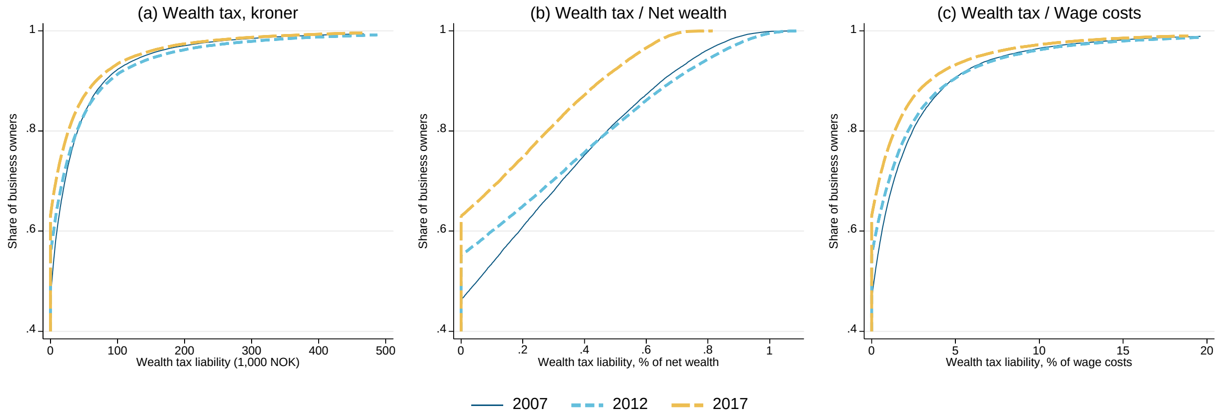


**Figure 1. The distribution of potential wealth tax liability among owners of family controlled firms**

Note: The cumulative density functions show wealth tax liabilities that are based on the level and composition of wealth two years before the respective tax years. Panel (a) shows potential wealth tax measured in kroner (NOK), panel (b) shows the tax rate; i.e., the wealth tax divided by the taxpayers net taxable wealth, and panel (c) shows the wealth tax divided by the total wage costs in the taxpayer’s firm (weighted by owner share).

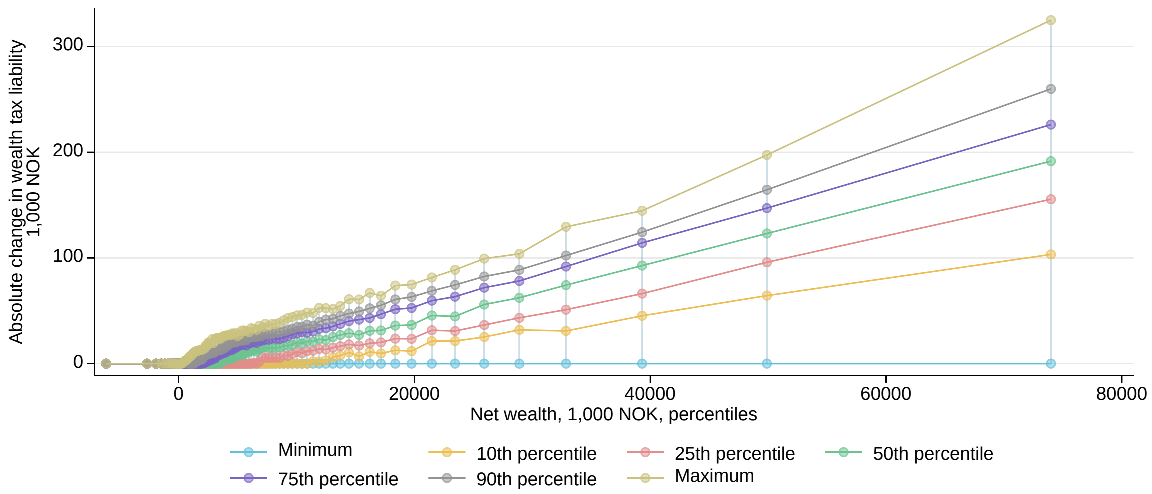


**Figure 2. The reform-generated variation in wealth tax liabilities among owners of family controlled firms 2007-2017, based on the 2015 net wealth distribution**
Note: For each percentile in the observed (market-evaluated) wealth distribution in 2015, the figure show statistics for the distribution of the difference between the highest and the lowest tax liability based on all the tax systems that have been in operation from 2007 through 2017.

# 5 Empirical analysis

Before we turn to the outcomes of primary interest, we use our empirical model (Equation (1)) to explore the relationship between the potential and the actually paid wealth tax. Recall that the potential tax liability calculated for a given year is based on the wealth reported two years before; hence, it will deviate from the actually paid tax for two reasons: First, individual wealth fluctuates considerably from year to year for reasons unrelated to the wealth tax. Second, the design of the wealth tax may entail portfolio reallocation strategies, e.g., in terms of tax-minimizing asset composition. Both these mechanisms imply that we expect the empirical relationship between potential and actual tax liability to be characterized by a coefficient considerably below unity.

Our identification strategy relies on the idea that any spurious correlation between the residual in Equation (1) and the potential tax liability two years after the base-year $(T_2(\mathbf{w}_{i0}))$ is absorbed by the controls for hypothetical tax liabilities calculated according to all tax regimes that existed during the estimation period. If this assumption holds, we expect the estimated impact of potential on actual tax liability to be stable with respect to the inclusion/exclusion of other control variables. To evaluate the solidity of the identifying assumption, we estimate a number of alternative models, characterized by large differences in the sets of control variables allowing for differential time trends along multiple dimensions. In a "baseline" model, we control for base-year fixed effects only; whereas the alternative models include individual/household characteristics as well as base-year (11 categories) interacted with indicators for firm size (7 categories), industry (84 categories), and municipality (430 categories). Given that we estimate several effect parameters, we use a graphical presentation form for the results. The estimated effects on actually paid tax are shown in Figure 3; for the same year as the calculated potential tax liability (two years after the base-year), as well as for the two subsequent years (year +1 and +2).

A first point to note from Figure 3 is that there is a remarkable stability across models with different conditioning sets. Focusing on the relationship between potential and actual wealth tax measured in the same year, the estimated coefficient is 0.5, regardless of the choice of control variables. A second point to note is that there appears to be a positive relationship between the potential wealth tax in a given year, and the actually paid wealth tax in the subsequent two years also. A plausible explanation is that the tax regime changes slowly; hence, since our model does not incorporate the potential tax liability in years +1 and +2, the potential wealth tax calculated for "same year" picks up an element of tax regime persistence. We are interested in these lagged effects because the employment effects of the wealth tax are likely to materialize gradually and, hence, need to be interpreted in light of concurrent as well as lagged influences of the potential tax liability calculated for a specific year.

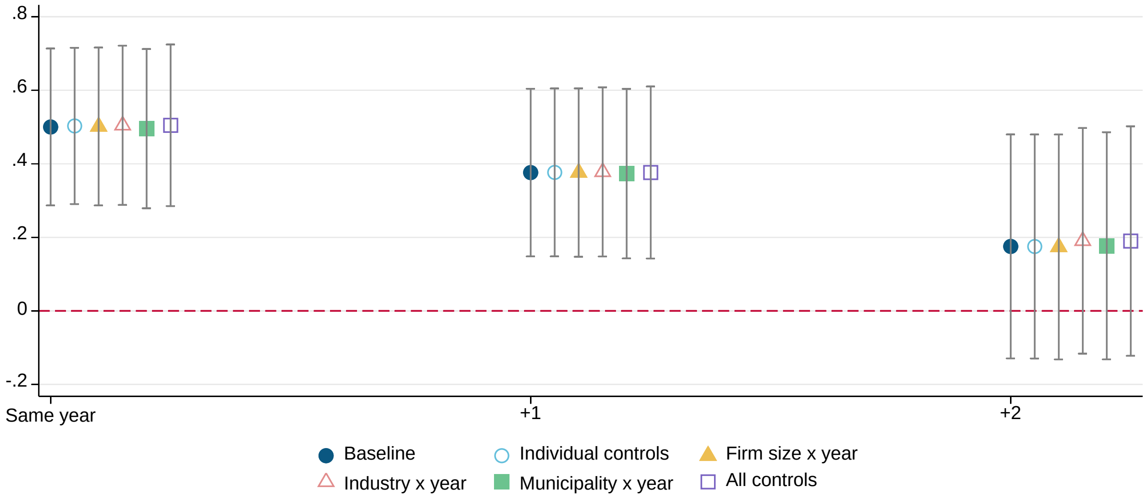


**Figure 3. The estimated effects of potential wealth tax on actually paid tax**
Note: The outcome in this regression is the actually paid wealth tax divided by the total wage bill in the base-year. The reported estimates are the delta-coefficients in Equation (1). Individual controls include dummy variables for household type (couple, single male, single female), age (five categories, the man's age in households), earnings components in base-year (wages, self-employment income, dividends), and immigrant status (native, other Western country, Eastern Europe, rest of world; status of the man in households). Controls for firm size include 7 firm-size dummy variables, all interacted with base-year (11 dummy variables). Controls for industry are based on two-digit NACE and contain 84 dummy variables, also interacted with base-year dummy variables. Controls for municipalities include dummy variables for each of Norway's 430 municipalities, again interacted with base-year dummy variables. The model with "all controls" include all the above listed controls (including interactions) at the same time. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level.

## 5.1 Effects on employment

If a firm is credit constrained – e.g., due to asymmetric information – the owner's allocation of own economic resources into the firm becomes important for the firm's development. A possible impact of a wealth tax then comes from two different sources. The first is that the wealth tax affects the taxpayer's overall wealth accumulation. This effect is normally considered negative, both due to the tax payment itself and due to a negative substitution effect arising from the reduced returns on savings. However, there is also a positive income effect, arising from the need to save more today in order to pay for future taxes. If the income effect is sufficiently large and the substitution effect sufficiently small, a positive impact on overall after-tax wealth accumulation is possible, and, according to Ring (2020b), even empirically relevant. The second source of wealth tax influence comes through a portfolio composition effect: A higher wealth tax gives the taxpayer stronger incentives to place economic wealth into assets with lower tax-value relative to market-value. An unlisted firm serves this purpose, particularly if the added capital is used to expand employment.

In this subsection, we examine empirically how the business owners' potential wealth tax affects the change in productivity-adjusted employment in their tightly held firms. Assuming that the wage level reflects the marginal productivity of labor, productivity-adjustment is achieved by using the firms' total wage bill as the employment variable, such that $y_{it} = WB_{it} - WB_{i0}$. The outcome, $\frac{y_{it}}{WB_{i0}}$, can then be interpreted as the relative change in (productivity-adjusted) employment from the base-year to the respective outcome years; conf. Equation (1). It is defined at the firm level (weighted with owner-share in the base-year), irrespective of any change in ownership occurring after the base-year. To avoid excessive influence of outliers, we have top-coded the dependent variable at 3, such that an increase in employment larger than 200% is set to 200%.[3] Using the total wage bill as a measure for (productivity-adjusted) employment also has the advantage that it is considered to be a very reliable piece of information. However, as we cannot rule out that the owner's wealth tax also influence the wage level among employees, we also perform the analysis based on an employment measure that simply counts work-hours reported to the administrative employer-employee register (although information on hours is considered less reliable than information on wage costs). The results from this exercise are presented in Appendix B. They turn out to be very similar to those based on the total wage bill.

Figure 4 illustrates the distribution of the outcome variable, for the same year as the calculated potential tax liability (two years after the base-year), and for the two subsequent years (year +1 and +2). In the year of the potential tax liability, 10% of the firms no longer have any employees. Approximately 30% have the same employment as in the base-year (+/- 10% in total wage bill). Only around 1% of the firms have increased employment by 200% or more, such that the outcome is affected by the top-coding. For the subsequent years, the changes become somewhat larger in both directions, but the fraction of top-coded observations remains as low as 2%.

[3] We present result for models without top-coding in Appendix. It turns out that point estimates are very similar to those based on top-coded data, but that standard errors become larger.

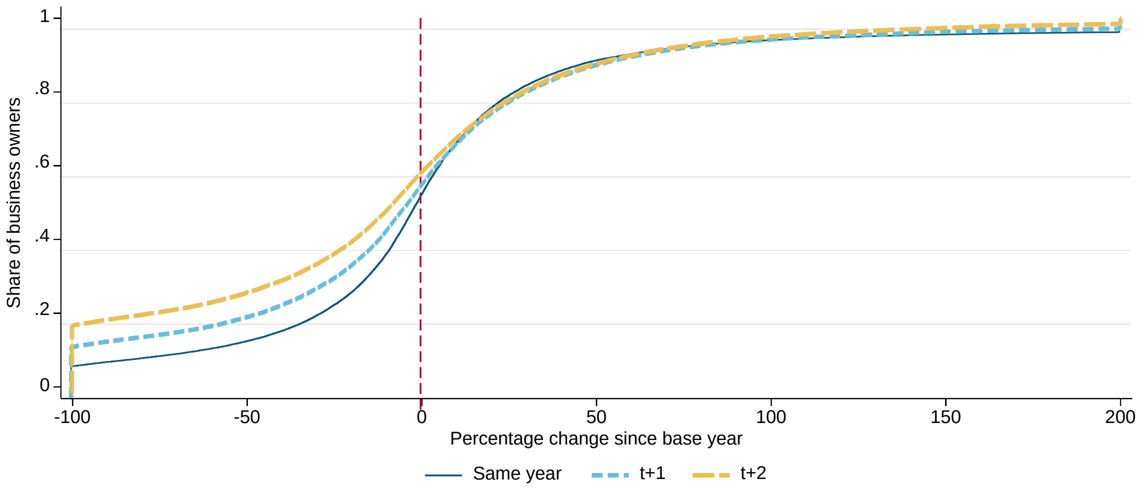


**Figure 4. Distribution of the percentage change in productivity-adjusted employment (total wage bill) from the base-year to the outcome year**

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

A central result of our paper is presented in Figure 5. Here, we show point estimates for the employment effect for each of the three outcome years, with 95% confidence intervals. Estimates are based on Equation (1) and presented for a baseline version of the model (with no additional controls), as well as for the models with added control variable sets interacted with base-year dummies. The explanatory tax variable of interest is the wealth tax that will apply in the second year after the base-year, provided that the base-year wealth is kept unchanged. Given the structure of the model (Equation (1)), the reported coefficients can be interpreted as estimated effects on the number of money units used to pay wages per unit potentially paid in wealth tax. Hence, the baseline estimates presented in Figure 5 imply that a 1 unit increase in the potential wealth tax increases the money spent on wages in the tax-payer's firm with 0.30 units in the same year, and with 0.55 and 0.65 units, respectively, in the subsequent two years. As expected – and in accordance with our identifying assumption – the estimated effects change very little when we add in various control variable sets interacted with base-year dummy variables. The effects estimated for the two subsequent years may reflect both that it takes some time before changes in the capital available to the firm result in changed employment and that the potential wealth tax in one year will be correlated with the tax liability in subsequent years; conf. Figure 3. The identifying variation in tax liability is too small to facilitate a disentanglement of these two mechanisms.

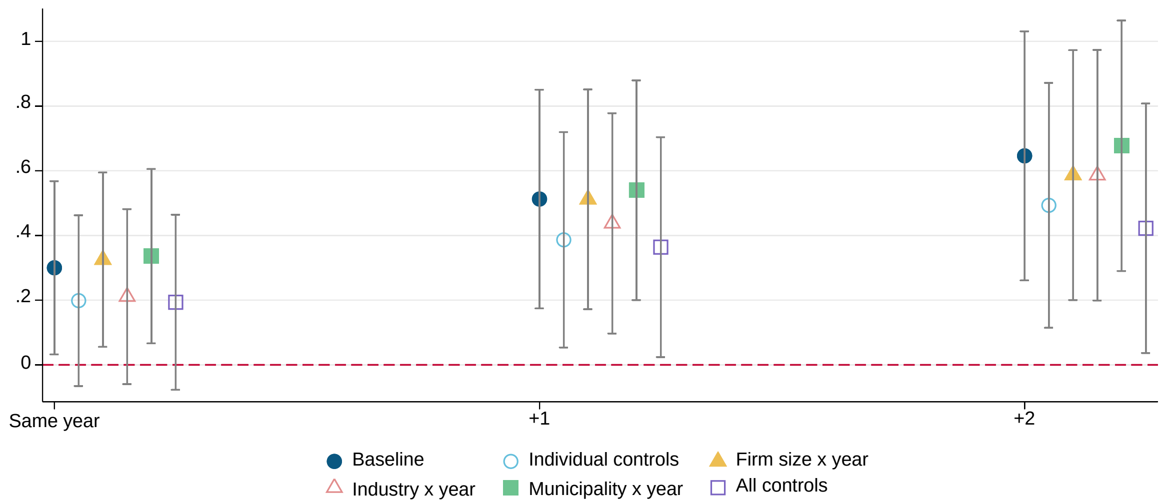


**Figure 5. The estimated effects of potential wealth tax on productivity-adjusted employment**
Note: The number of observations is 460,585. The dependent variable is the relative change in the owner-weighted total wage-bill from the base-year to the outcome-year. The reported estimates are the delta-coefficients in Equation (1). To avoid excessive outlier influence, the dependent variable has been top-coded at 3. For the definition of the various control variable sets; see note to Figure 3. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level.

Considering the sizes of the reported effect estimates in light of the typical sizes of the wealth tax and its reform-generated changes, it seems probable that the wealth tax has played a very small role in explaining employment fluctuations in Norway. According to Figure 1, as much as 90% of the business owners in Norway pays less than NOK 100,000, and our estimation results suggest that a tax cut equal to this number is expected to reduce wage costs by approximately NOK 50,000, which corresponds to only a tenth of an average fulltime employee.

While we have used a sample restriction ensuring that the wage bill (not including self-employment income) in the base-year exceeds NOK 500,000 in the baseline model (approximately corresponding to one full-time-full-year employee), we present in Figure 6 results based on alternative data restrictions on the initial firm size, including a version where we include self-employment income in the definition of the wage bill. Despite considerable changes in size as well as the composition of the estimation samples, with sample sizes varying from 107,108 to 1,037,406 (see Appendix A for details), the main results are remarkably stable across the different data cuts. Point estimates indicate somewhat bigger effects in the largest firms. In Appendix B, we present a range of additional robustness exercises, based on an alternative (non-parametric) specification of the explanatory tax variable, alternative specifications of the outcome variable (without top-coding and with a categorical employment growth variable), and with an alternative scaling of the tax liability (dividing by the owner's net wealth instead of by the total wage bill). The main findings remain similar across the alternative specifications.

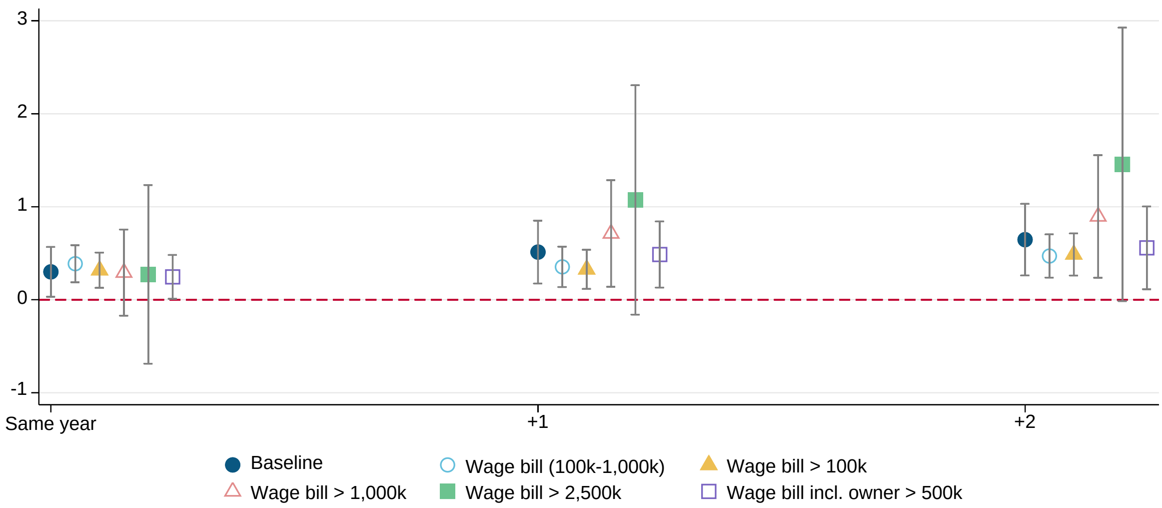


**Figure 6. The estimated effects of potential wealth tax on productivity-adjusted employment, based on alternative data cuts defined by the size of the firm's initial (owner-weighted) wage bill.**
Note: Sample sizes vary across the different data cuts. For the same year effect, they are as follows: Baseline model: 460,585; Wage bill between 100,000 and 1,000,000: 329,307; Wage bill above 100,000: 611,672; Wage bill above 1,000,000: 282,365; Wage bill above 2,500,000: 106,412; Wage bill including owner's self-employment income above 500,000: 809,476. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level.

The apparent dominance of positive income and/or portfolio composition effects does not imply that liquidity constraints are irrelevant for all firms. For owners with little liquid wealth, the tax liability may still generate a negative association between the wealth tax level and the firm's employment growth, as the owner may be forced to pull savings out of a credit-constrained firm in order to pay the tax. In Figure 7, we report separate estimates for owners with low and high liquidity. Low liquidity is in this case defined as the potential wealth tax exceeding 10% of the owner's liquid assets according to at least one of the tax regimes that have existed in our estimation period, and based on this criterion, only 12.9% of the business owners are considered to have low liquidity. All others are considered to have high liquidity. For owners with high liquidity, the positive effects of the wealth tax liability become considerably larger than in the baseline model, whereas for owners with low liquidity, the estimates become negative, particularly in the year of the tax liability. Hence, a negative liquidity effect does seem to be important for some owners.

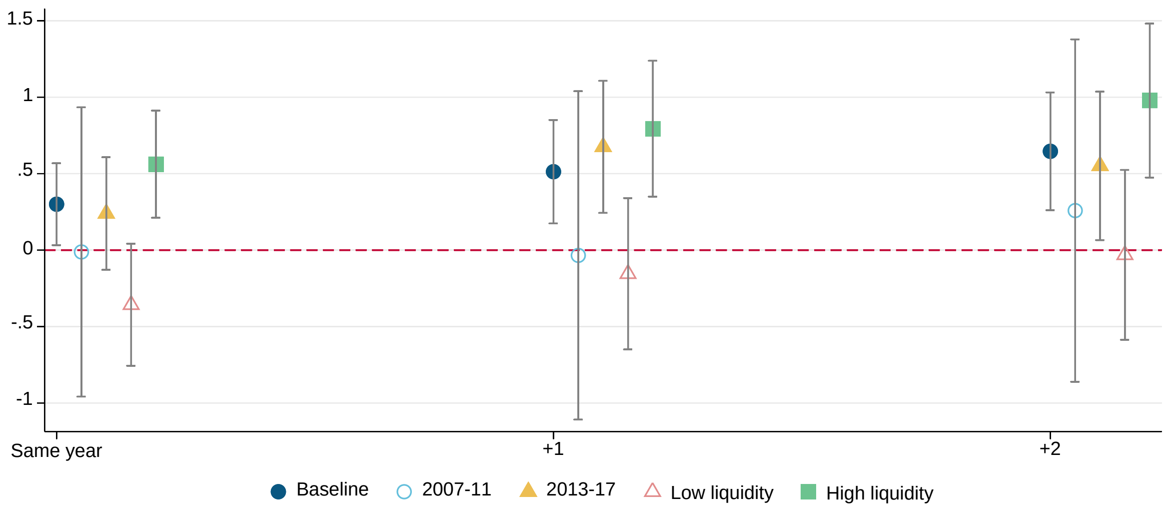


**Figure 7. Heterogeneous effects of the potential wealth tax on firm employment: By time-period and owner liquidity.**
Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level. Sample sizes (same year) are as follows: 2007-11: 195,589; 2013-17: 221,034; Low liquidity: 59,570; High liquidity: 401,015.

Given that the wealth tax influences firm investment through both income, substitution, and portfolio composition effects, we should not expect all sources of wealth tax changes to have the same effect. In particular, while the substitution and portfolio reallocation effects primarily are related to marginal tax rates, the income effect (and the direct effect of the tax liability) will have full force also for tax changes caused by manipulation of the lower tax threshold. Hence, by comparing estimated effects across periods characterized by different blends of identifying reform-based variation, we may shed some light on the underlying mechanisms.

While the tax reforms during the first part of our data window were dominated by increased lower tax thresholds and variations in valuation rules for homes and shares, the reforms in the later part were dominated by cuts in the marginal tax rate. Hence, if our estimated positive effect of the wealth tax level primarily is driven by a large positive income effect, we would expect the estimated effect to be similar across the two periods. If it is driven by portfolio composition effects, it should be larger in the second period.

In Figure 7, we show estimates built on the tax-years from 2007 through 2011 and from 2013 through 2017, respectively, representing each of these periods. While the estimated effects during the period of threshold increases are close to zero and statistically insignificant, the effects during the period dominated by changes in the marginal tax rate are large and positive. Hence, it appears that the estimated positive effect of the wealth tax comes thorough the composition of wealth more than through overall wealth accumulation.

## 5.2 Effects on capital flows between firm and owner

In order to take a closer look at the mechanisms behind the identified relationships between the wealth tax and employment growth, we use in this section the model in Equation (1) to examine outcomes capturing the flow of capital between the owner and the firm, measured at the firm or at the household level. This analysis requires access to accounting data, and it can therefore be implemented for limited liability companies only.

Figure 8 first shows the distribution of the outcome variables in question; i.e., a) net investments in fixed assets in the firm (fixed assets in outcome year minus fixed assets in base-year), b) investments in liquid assets in the firm (liquid assets in outcome year minus liquid assets in base-year), c) dividends paid to the owner, and d) salary paid to the owner; in all cases relative to the firm's wage bill in the base-year ($WB_{i0}$).[4] Except for the owner's own salary, all these outcomes have a concentration around zero, particularly investment in fixed assets and dividend payments. Dividends are actually zero in almost 70% of the business-years.

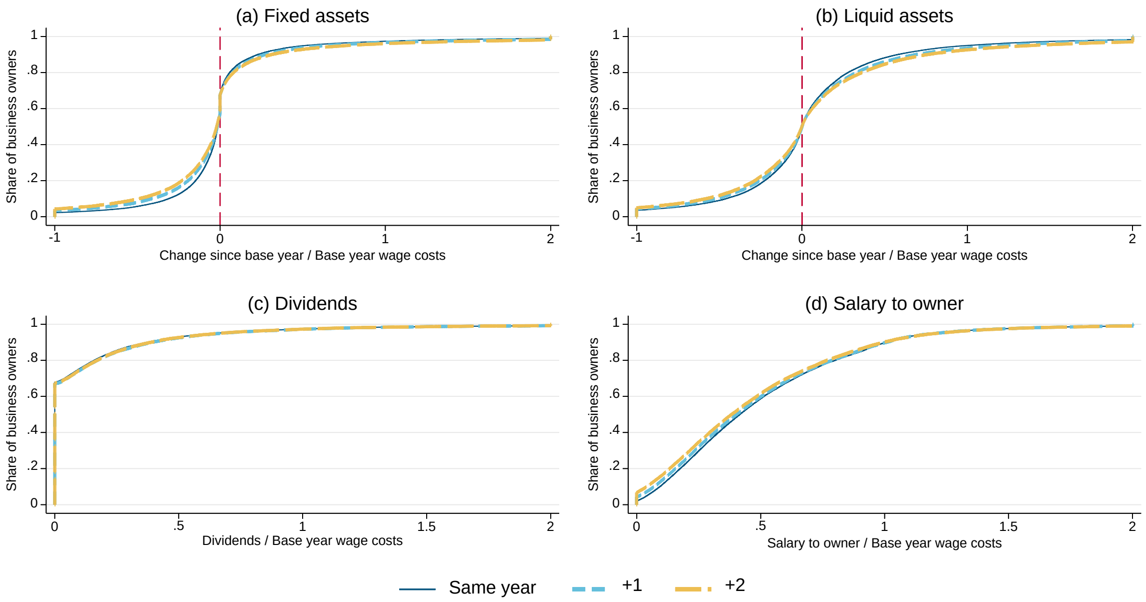


**Figure 8. The distribution of outcomes related to firm investments, owner dividends and salary, measured in percent of initial wage bill.**
Note: In panels (a) and (b), the variables are defined as changes from the base-year to the year in question, divided by the total wage bill in the base-year. In panels (c) and (d), the outcome variables are defined as the capital flow in the year in question, again divided by the total wage bill in the base-year.

The estimation results are summarized in Figure 9. The point estimates for the total sample indicate that the firm's investment in fixed assets is completely unaffected by the wealth tax, whereas point

[4] To reduce the influence of outliers, we have censored extreme observations, such that reductions corresponding to more than 100% of the initial wage bill are set to -100, while increases corresponding to more than 200% are set to 200. We note from Figure 8 that all the outcomes have a large concentration around zero, and that the censoring affects only a small fraction of the observations.

estimates indicate a positive effect on the firm's liquid assets and a contemporary negative effect on dividends and salary to the owner. Again, estimates are stable across models with different conditioning sets. The pattern displayed in Figure 9 is consistent with the findings for employment. On average, higher wealth tax liability induces owners to allocate more capital into the firm, and the added capital is used to invest in human rather than physical capital.

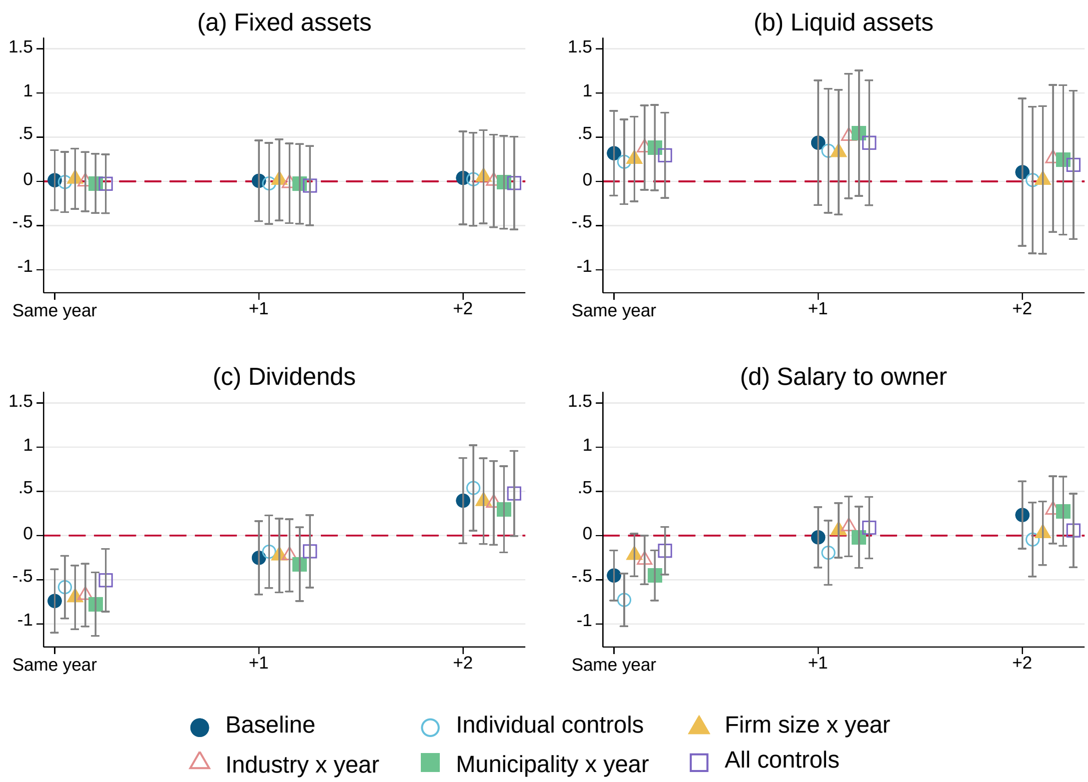


**Figure 9. The estimated effects of potential wealth tax on investment in firm assets, dividends, and owner takeout.**

Note: The sample comprises owners of limited liability companies only, and the sample size is 409,412. In panels (a) and (b), the outcome variables are defined as changes from the base-year to the year in question, divided by the total wage bill in the base-year. In panels (c) and (d), the outcome variables are defined as the capital flow in the year in question, again divided by the total wage bill in the base-year. For the definition of the various control variable sets; see note to Figure 3. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level.

We also report separate estimates for the alternative data cuts, obtained by manipulating the size requirement in the base-year. The results, shown in Figure 10, indicate a similar effect pattern across the different samples, with a possible exception for the negative longer-term liquidity effect estimated for the largest firms. It is notable, though, that the negative liquidity effect in large firms is matched by a particularly large positive employment effect; conf. Figure 6.

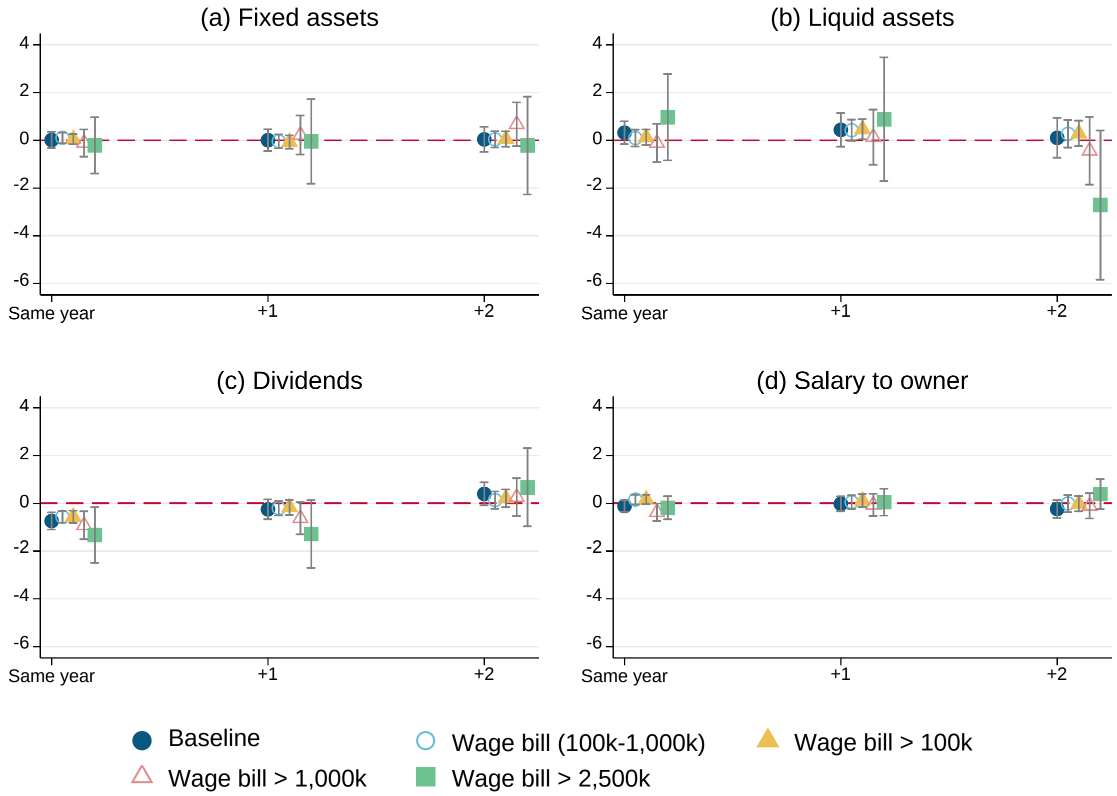


**Figure 10. The estimated effects of potential wealth tax on investment in firm assets, dividends, and owner takeout, based on alternative data cuts defined by the size of the firm's initial (owner-weighted) wage bill.**

Note: Sample sizes vary across the different data cuts. For the same year effect, they are as follows: Baseline model: 409,412; Wage bill between 100,000 and 1,000,000: 230,319; Wage bill above 100,000: 493,982; Wage bill above 1,000,000: 263,663; Wage bill above 2,500,000: 103,587. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level.

Finally, Figure 11 reports separate effects for the different reform periods and for owners with low and high liquidity. Again, the effect pattern appears to be consistent with the corresponding pattern identified for employment effects. During the first reform period, dominated by the rise in the lower tax thresholds, we estimate negative effects on firm liquidity (though not individually statistically significant), suggesting that the direct liquidity effect of the higher tax payment dominated in this period; see panel (b). This negative effect is also identified for taxpayers with low liquidity. Hence, while owners with high liquidity increase the level of liquid firm assets in response to higher wealth tax, owners with low liquidity do seem to drain the liquid assets of the firm. However, as shown in panels (c) and (d), a wealth-tax-generated reduction in liquid assets in firms owned by liquidity-constrained owners is not matched by higher dividends or salary to owner. Changes in the wealth tax for this group of owners thus seem to affect the firm primarily through its paid-in equity.

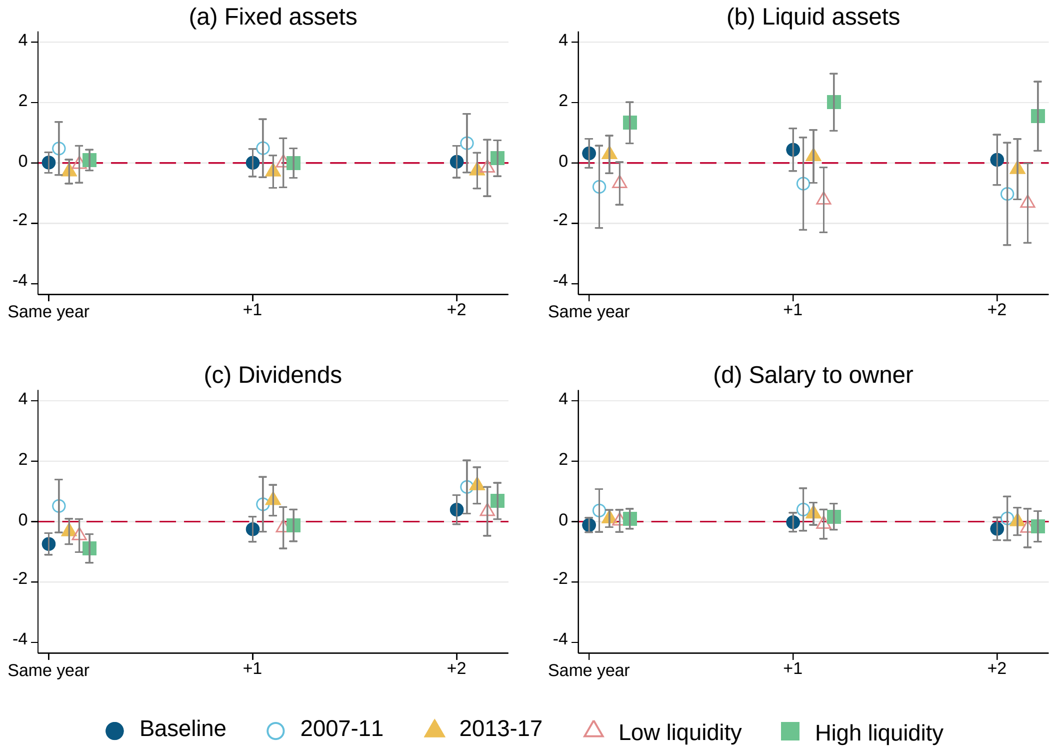


**Figure 11. Heterogeneous effects of the potential wealth tax on investment in firm assets, dividends, and owner takeout: By time-period and owner liquidity.**
Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level. Sample sizes (same year) are as follows: 2007-11: 195,589; 2013-17: 221,034; Low liquidity: 58,151; High liquidity: 351,261.

# 6 Concluding remarks

As all redistributive taxes, the wealth tax creates behavioral distortions. The research literature has primarily focused on how a wealth tax distorts decisions regarding consumption and saving, through income and substitution effects. 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, drag down growth, and reduce employment. In the present paper, we have examined the empirical relationship between a wealth tax imposed on owners of small and medium sized firms and subsequent firm growth. 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 significant positive relationship between wealth tax liability and employment. A positive employment effect can be explained by a strong income effect (the taxpayer saves more now in order to prepare for future taxes). However, it appears that the estimated employment effect of a given change in the wealth tax is much larger when the identifying tax reforms are associated with changes in the marginal tax rate than when they primarily are associated changes in the lower tax thresholds. This finding highlights an additional channel for the influence of the wealth tax imposed on firm owners, namely a portfolio composition effect. 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 to place their wealth in the firm, particularly by increasing employment, and this incentive becomes stronger the higher is the (marginal) wealth tax. In Norway, the tax-incentive to invest in non-listed firms has, in some periods, been deliberately strengthened by the provision of a tax rebate on "working capital."

Consistent with this story, we find little effect on employment in small and medium sized family-controlled firms of the wealth tax reductions that were implemented up to around 2013, primarily thorough increased thresholds, while we find negative employment effects of the reductions implemented after 2013, primarily through reductions in the marginal tax rate. However, although the portfolio composition effect appears to dominate the aggregate picture, our analysis confirms that credit constraints may generate negative employment effects in firms owned by household with poor liquidity. Hence, there is not a single and unambiguous answer to the question of how changes in the wealth tax influences employment in small and medium sized business. Rather, the answer depends on the source of the changes as well as of other existing features of the tax system.

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# Appendices

## Appendix A: Construction of analysis samples and descriptive statistics

Table A1 provides a description of how we constructed the baseline dataset used in the empirical analysis. The starting point is the set of all firms controlled by individual households. However, the vast majority of them have (from our perspective) negligible economic activity, with total earnings/wages below the level required for being the main source of income for any owner-family or employees. By requiring at least one employee (potentially including the owner) the number of firm-household-year observations (over 11 years) is reduced from 3.7 to 0.8 million observations. The sample is further slightly reduced (by 657 observations) by setting an upper limit of 100 employees and somewhat more reduced (by 3,031observations) by removing owners with net wealth above NOK 100 million. Finally, in the baseline sample, we require that the firm has wage costs consistent with at least one employee (full-time-full-year-equivalent) in addition to any self-employment income, and end up with the sample described in Table A1, row E.

**Table A1. Construction of analysis samples and the number of firm-household-year observations (2005-2015)**

| | All | Limited liability firms | Self-employed |
|---|---|---|---|
| A. All firm-household-year observations with positive turnover in the firm (at least NOK 1000) | 3,717,302 | 794,462 | 2,922,840 |
| B. Plus a requirement of at least one employee, including a self-employed owner. | 813,164 | 412,689 | 400,475 |
| C. Plus a requirement of no more than 100 employees | 812,507 | 412,038 | 400,469 |
| D. Plus a requirement that the owner's net wealth (market value) does not exceed NOK 100 mill. | 809,476 | 409,412 | 400,064 |
| E. As in row D, but with the requirement in row B modified such that it requires at least one employee in *addition* to the self-employed owner (baseline sample) | 460,585 | 409,412 | 51,173 |

Note: The minimum employment requirement is implemented by requiring total annual wage costs to exceed NOK 500,000, including or excluding self-employment income (rows B and D, respectively). This threshold corresponds approximately to the average full-time-full-year earnings in Norway.

Figure 6 reported main estimation results for å number of alternative data cuts, defined by different initial conditions on the firms' initial (base-year) wage bills. In Table A2, we show descriptive statistics for each of the resultant samples.

**Table A2. Descriptive statistics for the alternative sample cuts used in Figures 6 and 10**

| | Size of wage bill in base year (1,000 NOK) (means/fractions) | | | | | |
|---|---|---|---|---|---|---|
| | > 500 (baseline) | 100-1,000 | > 100 | > 1,000 | > 2,500 | > 500 (incl. self-emp. inc.) |
| **Number of observations** | 460,585 | 331,878 | 616,055 | 284,177 | 107,108 | 809,476 |
| **A. Type of owner/household** | | | | | | |
| Household with more than 1 individual | 0.64 | 0.58 | 0.62 | 0.67 | 0.70 | 0.61 |
| Single male | 0.28 | 0.31 | 0.30 | 0.28 | 0.26 | 0.33 |
| Single female | 0.07 | 0.10 | 0.08 | 0.05 | 0.04 | 0.07 |
| **B. Household characteristics** | | | | | | |
| Gross wealth, stipulated market value (1,000 NOK) | 9,447 | 6,506 | 8,567 | 10,974 | 14,991 | 8,071 |
| Gross wealth, tax value (1,000 NOK) | 5,448 | 3,226 | 4,796 | 6,629 | 9,833 | 4,366 |
| Net wealth, stipulated market value (1,000 NOK) | 6,710 | 4,264 | 5,971 | 7,964 | 11,419 | 5,472 |
| Net wealth, tax value (1,000 NOK) | 2,712 | 984 | 2,200 | 3,619 | 6,261 | 1,767 |
| Potential wealth tax (1,000 NOK) | 28.7 | 12.3 | 23.9 | 37.4 | 63.1 | 20.0 |
| Liquid assets (1,000 NOK) | 918 | 694 | 849 | 1,029 | 2,738 | 866 |
| Potential wealth tax rate (% of net taxable wealth) | 0.17 | 0.10 | 0.15 | 0.21 | 0,29 | 0,14 |
| Potential wealth tax relative to (owner-weighted) wage costs (%) | 1.61 | 2.74 | 2.09 | 1.33 | 1.19 | 7.28 |
| **C. Firm characteristics (weighted by owner share)** | | | | | | |
| Total wage bill (1,000 NOK) | 2,206 | 531 | 1,730 | 3,129 | 5,707 | 1,674 |
| Total employment (fulltime equivalents) | 5.05 | 1.56 | 4.08 | 7.03 | 1.41 | 3.44 |
| **D. Firm characteristics limited liability companies only (weighted by owner share)** | | | | | | |
| Number of observations (limited liability companies only) | 409,412 | 230,319 | 493,982 | 263,663 | 103,587 | 409,412 |
| Fixed assets (1,000 NOK) | 1,177 | 444 | 1,042 | 1,563 | 2,715 | 1,177 |
| Liquid assets (1,000 NOK) | 1,415 | 696 | 1,264 | 1,756 | 2,738 | 1,415 |
| Dividend payments to owner (1,000 NOK) | 240 | 82 | 206 | 315 | 514 | 240 |
| Salary to owner (1,000 NOK) | 622 | 453 | 564 | 693 | 794 | 703 |

Note: 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)

## Appendix B: Additional robustness exercises

### Employment outcome based on reported hours worked

In the baseline results for employment effects reported in Figure 5, the employment outcome is defined as the relative change in a firms (owner-weighted) the total wage bill from the base-year to the outcome year. To ensure that the estimated effects are not dominated by any influence on wage levels, we also estimate the model based on an outcome variable defined as the relative change in fulltime-equivalent man-years (constructed from reported work hours in the employer-employee register). The results are presented in Figure B1. They indicate somewhat stronger positive employment effects than what we found in the baseline model. A plausible interpretation of that is that the marginal employees (new hires and separations) on average have lower wages than stable employees.


**Figure B1. The estimated effects of potential wealth tax on employment based on reported wage costs (baseline model) or reported hours.**
Note: The data-points marked "Wage costs" repeat the baseline estimates from Figure 5. The data-points marked "Fulltime equivalent man years" repeat estimates for the same model (Equation (1)), but the relative change in man-years (from base-year to outcome year) as the dependent variable instead of the relative change in total wage costs. The number of man-years is computed based on the reported workhours in the employer-employee register. The number of observations is 460,585. To avoid excessive outlier influence, the dependent variable has been top-coded at 3. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level.

### Non-parametric model specification

The baseline model is based on a linear relationship between the wealth tax (including the counterfactual tax-rate controls) and the outcome variable. To examine the validity of this assumption we code all tax variables categorically and re-estimate the model. As in the baseline model, all tax-variables are specified relative to the wage bill in the base year. The categories we use are: (0-1%], (1-2.5%], (2.5-5%], (5-10%], (10-15%] and (15%+). The excluded reference category is 0%, which constitutes approximately 50% of the sample. The results are displayed in Figure B2, where we also include the linear baseline model for comparison. For the baseline model we plot the linear coefficient (as displayed in Figure 5) multiplied with the mean wealth tax / WB within each of the categories.

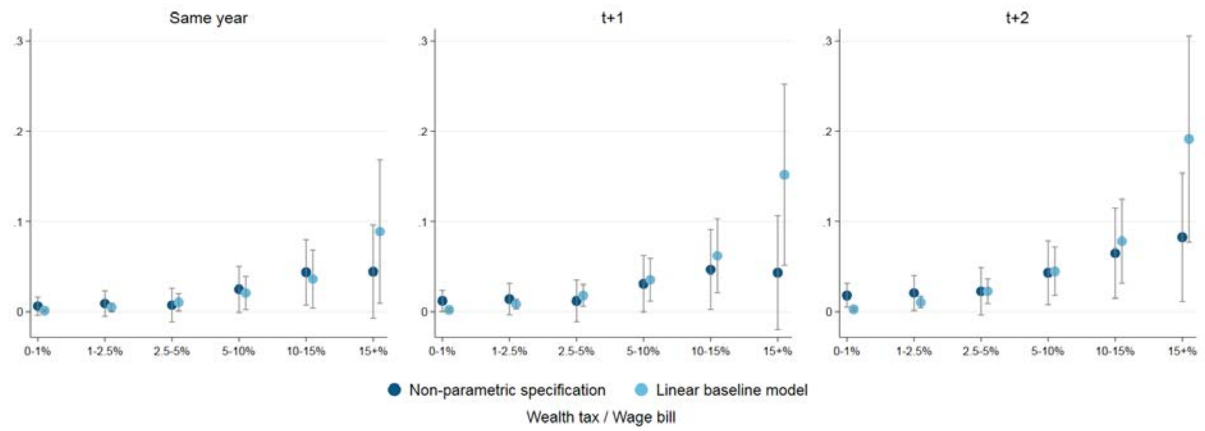


**Figure B2. The estimated effects of potential wealth tax on productivity-adjusted employment Estimation result from non-parametric model specification**

Note: The data-points marked "Non-parametric specification" show estimates attached to indicator variables for potential tax relative to wage costs in the intervals (0-1%], (1-2.5%], (2.5-5%], (5-10%], (10-15%] and (15%+), with 0 as the reference. Both the actual tax ($T_2$) and all the counterfactual tax rates ($T_s$) are coded this way. The data-points marked "Linear baseline model" show, for comparison, the corresponding estimates resulting from the baseline (linear) model, based on average tax rates computed within each interval. The number of observations is 460,585. To avoid excessive outlier influence, the dependent variable has been top-coded at 3. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level

### Alternative specifications of the outcome variable

In the baseline specification, the outcome variable is growth in the total wage bill since the base year. This variable has a natural lower limit of zero, but to reduce the problem with outliers we top-code this outcome at 3 (equal 200 percent growth). In Figure B3 we display results also without this top-coding. We can see that our main findings do not hinge on this top-coding at all. In the same graph, we also display results for a similar model but with a categorical outcome. We divide all outcome years in three groups: Those with a reduction in the wage bill of more than 10% are coded -1, and those with an increase of more than 10% is coded 1. The rest are coded 0. We see that our findings are robust also to such manipulation of the outcome. Overall, we conclude from this that neither outliers in the outcome variable nor functional form issues appear to drive our main findings.

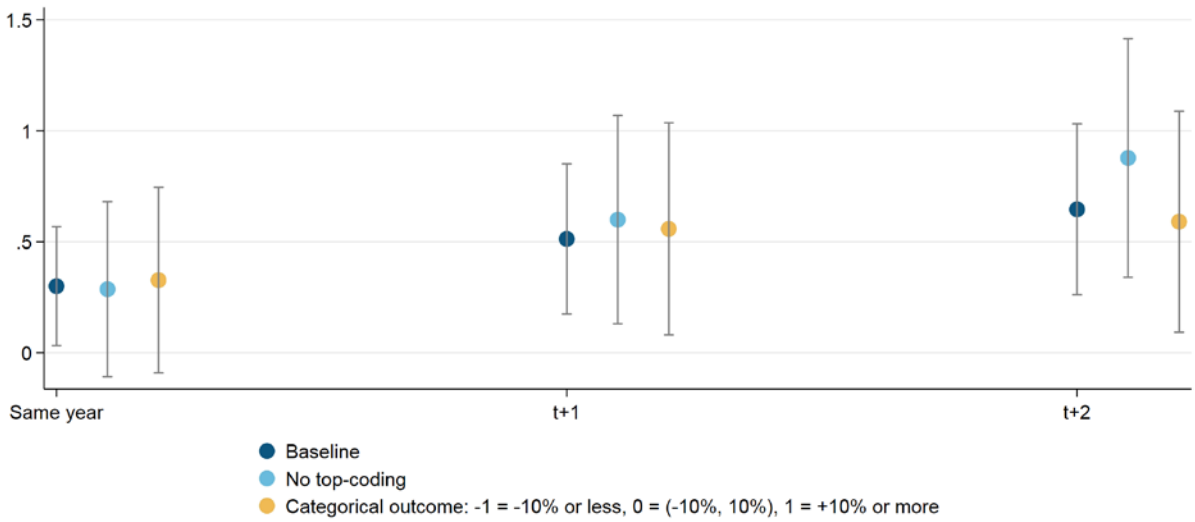


**Figure B3: The estimated effects of potential wealth tax on productivity-adjusted employment Estimation results from alternative specifications of the outcome variable**

Note: The data-points marked "No top-coding" display results for the baseline model without censoring the outcome variable at 3. The data-points marked "Categorical outcome" report results from a model where the outcome variable takes three discrete values only; i.e., -1 (with at least 10% decline in the total wage bill), 0 (with less than +/- 10% change in the wage bill), and 1 (with more than 10% increase in the wage bill). The number of observations is 460,585. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level

### Alternative model based on average tax-rates

In the baseline model, we divide both taxes and the outcome variables by the firms' wage bill in the base-year. An alternative way to scale the wealth tax is to divide by net wealth to obtain what we can think of as an *average tax rate* for wealth. We display the distribution of average wealth tax in Figure 1. In Figure B4 we show estimation results from a model where we relate the growth in the total wage bill, i.e. the same outcome as in the baseline mode, to the average tax rate for wealth. We present results from a linear model as well as a non-parametric model, following the same strategy as in Figure B2.

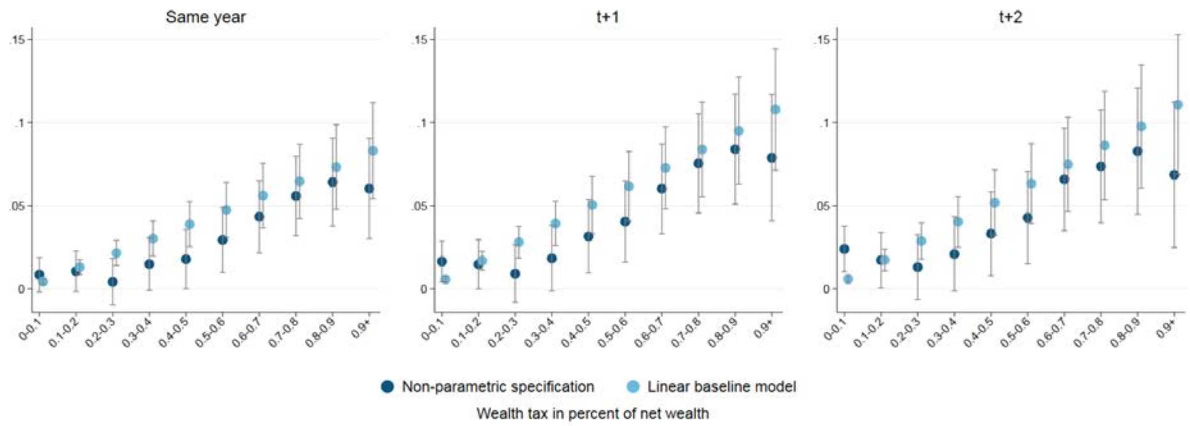


**Figure B4: The estimated effects of potential wealth tax on productivity-adjusted employment Estimation results from alternative model based on average tax-rates**

Note: The graphs show estimates defined in the same way as in Figure B2, but with the potential wealth tax divided by net wealth in the base-year instead of by total wage costs. The number of observations is 460,585. Point estimates are reported with 95% confidence intervals. Standard errors used to compute these confidence intervals are clustered at the person/household level