Norwegian University of Life Sciences
School of Economics and Business

Philosophiae Doctor (PhD)
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Taxation of Income and Wealth in an Imperfect World: Rates Matter, Rules Decide

Skatt på inntekt og formue i en imperfekt verden: Satser teller, regler avgjør

Marie Bjørneby

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Marie Bjørneby, Ås, June 2023

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Paper 2: Alstadsæter, A., Bjørneby, M., Kopczuk, W., Markussen, S., & Røed, K. (2022) Saving effects of a real-life imperfectly implemented wealth tax: Evidence from Norwegian micro data. *AEA Papers and Proceedings* (Vol. 112, pp. 63-67).[1]

Paper 3: Bjørneby, M. (2022) Hvordan virker formuesskatten? *Samfunnsøkonomen*, 2, 11-25.

Paper 4: Bjørneby, M., Alstadsæter, A., & Telle, K. (2021) Limits to third-party reporting: Evidence from a randomized field experiment in Norway. *Journal of Public Economics*, 203, 104512.

2 Abstract

Rising inequality and tightening government budgets have brought taxation high up on the agenda, both in the public debate and in academia. In particular, this has spurred a renewed interest in wealth taxation. To raise tax revenue fairly and efficiently, it is crucial to understand how taxpayers respond to different forms of taxation, given the practical limitation in implementing and enforcing them. In this thesis, I demonstrate empirically the importance of different response margins that are not captured in standard models, but that turn out to be central given the practical implementation of the tax rules.

The first chapter (with Markussen and Røed) study the effect of taxing the wealth of firm owners on the economic development of their closely-held firms. While it is often claimed that the wealth tax hampers the growth of small businesses, we document a *positive* effect on the capital available to the firm and firm's subsequent employment growth. In practice, investment in unlisted firms is tax favoured due to the exclusion of firms' intangible assets from the tax base. Our findings indicate that firm owners respond to wealth tax by reallocating their portfolio towards (tax-exempted) intangible firm assets.

In the second chapter (with Alstadsæter, Kopczuk, Markussen and Røed), we look at how a wealth tax affects the level and composition of savings. While most studies have focused on effects on taxable wealth, including both real and reporting responses, we study effects on real wealth accumulation. By exploiting rich variation generated by various valuation discounts in the wealth tax, we shed light on how behavioural responses depend on the actual implementation of the tax. We document that the saving response is weaker under a less comprehensive system. This is consistent with taxable wealth responding more strongly than actual wealth accumulation.

In the third chapter, I study the "anatomy" of the Norwegian wealth tax. Exploiting the rich administrative data on wealth in Norway, I perform a descriptive analysis of the distributional properties of the wealth tax and how this has changed over time.

To isolate the effect of rule-changes from underlying changes in wealth, I develop a microsimulation model and calculate the static (non-behavioral) wealth tax changes for all Norwegian households with different counterfactual rulesets. I show how significant changes in valuation rules, as well as the rate structure, have affected the distribution of the tax burden over time, both across assets and across the wealth distribution.

In the fourth chapter (with Alstadsæter and Telle), we focus on the limits of third-party reporting when taxpayers collude to evade taxes. Using a randomized audit experiment, we document that firms in certain sectors underreport wages on behalf of their employees. Our results highlight that, even in a developed country with fully implemented third-party reporting and withholding taxes, tax administrations need to consider that employers and employees can collude, especially within small firms.

3 Norsk sammendrag

Økende ulikhet og svekkede offentlige finanser har satt skatt høyt på dagsorden, både i samfunnsdebatten og i akademia. Spesielt har dette ført til en fornyet interesse for formuesbeskatning. For å hente inn skatteinntekter rettferdig og effektivt, er det avgjørende å forstå hvordan skattebetalere responderer på ulike former for beskatning, gitt praktiske begrensninger i å implementere og håndheve dem. I denne oppgaven demonstrerer jeg empirisk betydningen av ulike responsmarginer som ikke fanges opp i standardmodeller, men som viser seg å være sentrale gitt den praktiske implementeringen av skattereglene.

Det første kapittelet (med Markussen og Røed) studerer effekten av å skattlegge formuen til bedriftseiere på den økonomiske utviklingen til selskapene de kontrollerer. Mens det ofte hevdes at formuesskatten hemmer veksten til små bedrifter, dokumenterer vi en *positiv* effekt på kapitalen som er tilgjengelig for bedriften og bedriftens påfølgende sysselsettingsvekst. I praksis er investering i unoterte foretak skattefavorisert på grunn av at immaterielle eiendeler ikke inngår i skattegrunnlaget. Våre funn indikerer at bedriftseiere responderer på formuesskatt ved å reallokere porteføljen til (skattefrie) immaterielle eiendeler.

I andre kapittel (med Alstadsæter, Kopczuk, Markussen og Røed) ser vi på hvordan formuesskatten påvirker nivået og sammensetningen av sparing. Mens de fleste studier har fokusert på effekter på skattepliktig formue, som inkluderer både realøkonomiske responser og omgåelse, studerer vi effekter på reell akkumulering av formue. Ved å utnytte rik variasjon skapt av ulike verdsettelsesrabatter i formuesskatten, belyser vi hvordan atferdsresponser avhenger av den faktiske implementeringen av skatten. Vi dokumenterer at spareresponsen er svakere når skattegrunnlaget er smalere som følge av rabatter. Dette samsvarer med at skattepliktig formue er mer elastisk enn faktisk sparing.

I tredje kapittel studerer jeg «anatomien» til den norske formuesskatten. Med bakgrunn i rike administrative data for formue i Norge, beskriver jeg formuesskattens fordelingsegenskaper og hvordan dette har endret seg over tid. For å isolere effekten av regelendringer fra underliggende endringer i formuen, utvikler jeg en mikrosimuleringsmodell og beregner den statiske (ikke-atferdsmessige) formuesskatten for alle norske husholdninger med ulike kontrafaktiske regelsett. Jeg viser hvordan betydelige endringer i verdsettelsesregler, samt satsstrukturen, har påvirket fordelingen av skattebelastningen over tid, både på tvers av eiendeler og over formuesfordelingen.

I fjerde kapittel (med Alstadsæter og Telle) fokuserer vi på begrensningene ved tredjepartsrapportering når skattebetalere samarbeider om å unndra skatt. På bakgrunn av et eksperiment med randomiserte kontroller, dokumenterer vi at bedrifter i visse sektorer underrapporterer lønn på vegne av sine ansatte. Resultatene våre peker på at selv i et land med fullt ut implementert tredjepartsrapportering og forskuddstrekk, må skattemyndighetene ta høyde for at arbeidsgivere og ansatte kan samarbeide om å unndra skatt, spesielt i små bedrifter.

4 Introduction to the thesis

4.1 Introduction

The “state capacity” to successfully raise tax revenue is a key aspect of advanced economies. Modern tax systems collect 30-40% or more of GDP in tax. How tax revenues are collected, from which tax bases and taxpayers, clearly has huge direct implications on welfare. By altering relative prices, raising taxes also influences people’s behaviour and thus the overall allocation and use of limited resources and output in the economy.

For decades, public economists have devoted their time to guide the design of an optimal tax system: A tax system that achieves a desired level of revenue and redistribution while minimizing the welfare-reducing impact on people’s behaviour. Traditionally, the focus has been on “real” responses due to altered relative prices – e.g. how taxes on labour income distorts labour supply (the choice between consumption and leisure), how taxes on capital distorts savings (the choice between present and future consumption), or how domestic taxes can induce labour and capital to move abroad. According to standard theory, optimal tax rates and progressivity depends inversely on the responsiveness (the “elasticity”) of the tax base. Ultimately, the size of the elasticities, and hence the efficiency cost, is an empirical question. The optimal tax rate can be determined by balancing these efficiency cost against distributional concerns captured by the welfare weights.

In more recent times, there has been an increased focus on the practical parts of implementing and enforcing tax systems, and also on the importance of behavioural responses beyond “real” responses (see e.g. Slemrod and Yizhaki, 2002). Recognizing, for instance, that the tax base of labour income taxes is not actual labour supply but yearly reported labour income, opens “non-standard” behavioural responses to taxation such as timing of transactions, misreporting, shifting income across taxpayers, tax bases or jurisdictions (e.g. cross-border profit shifting or relabelling labour income as capital income) and changing the legal form of organization (e.g. wage earner or self-employed). When taxpayers respond to taxation by changing their reporting without changing any real economic activity, this is often referred to as reporting responses, or avoidance (legal) and evasion (illegal) responses.

Tax avoidance and evasion, as well as real responses, incur real resource costs that adds to the excess burden of taxation. First, resources are used directly on both implementing and combating noncompliance. Secondly, resources are allocated to activities that facilitates noncompliance instead of where they generate the highest social value. Feldstein (1999) argued that if taxpayers respond up to the point where the underlying costs on the margin equals the potential tax saving (i.e. the marginal tax rate), the distinction between real and reporting responses is irrelevant for evaluating the excess burden. This implies that the elasticity of the tax base is a sufficient statistics for welfare analysis. Later, this result has been questioned. Chetty (2009) argues that it is the marginal social cost that matters for the excess burden, and that this is not necessarily equal to the tax rate. Therefore, a highly elastic tax base does not always imply a high efficiency cost.

Moreover, it is important to keep in mind that, while real responses depend mainly on an individual's preferences (e.g. for work and leisure), avoidance and evasion responses can to some extent be influenced by the tax system in place (Slemrod and Kopczuk, 2002). Policymakers can reduce elasticities by e.g. strengthening enforcement, broadening the tax base and making use of third party reported tax information. Large reporting responses could be an argument for improving a poorly designed tax system, instead of reducing tax rates.

Taxes that are equivalent in standard models, can differ a lot when it comes to the possibilities to enforce them. Governments need to have credible information on the tax bases in order to impose taxes on them. Taxes that are based on arms-length transactions are often considered the easiest both for taxpayers to comply with and for tax authorities to enforce. Kleven et al. (2016) argue that a governments' capacity to levy taxes depends on the adoption of "modern taxes" that rely on third-party information or accounting books of firms, such as VAT, CIT and personal income taxes, as opposed to "traditional taxes" that often rely on self-reported information such as property and wealth taxes.

Understanding the underlying mechanisms of behavioural responses is therefore crucial for designing an optimal tax system. The credibility revolution has led to an explosion of empirical research on how structures of tax systems influence people's behaviour. Still, a fundamental challenge is that taxes are endogenous (depends on e.g. taxable income), and that both the outcome (e.g. future income) and tax changes due to tax reforms are typically correlated with initial income or wealth (see e.g. Jakobsen and Søgaard, 2022). Furthermore, in econometric analysis, it is generally hard to separate real and reporting responses. As most studies are based on data reported to the tax authorities, they measure the overall response of the tax base.

The aim of this thesis is to contribute to our understanding of how taxes, given the limitations of actual implementation, affect taxpayers' decisions. The first three chapters focuses on wealth taxation. While existing empirical literature on wealth taxes has mostly studied the effects on taxable wealth, i.e. the combined real and reporting responses, the focus in this thesis is on the response of critical real variables such as investment, employment and the level and composition of savings. In paper 1 and 2, we also propose an approach to deal with the identification issues, that has not previously been used in the tax literature. The fourth paper focuses on tax evasion and limits to third-party reporting when firms collude in evading taxes.

4.2 Background

The main focus of this thesis is on wealth taxation. While most countries taxes capital through capital income taxes, property taxes and estate or inheritance taxes, few countries levies an annual wealth tax.

Rising wealth concentration and tightening government budgets have spurred a renewed interest in wealth taxation, both in the public debate and in academia, and with that a need to advance our understanding on how wealth taxes work. How does a wealth tax affect the level and composition of savings? How should it be designed to minimize avoidance opportunities and the costs of administering and enforcing the tax?

The wealth tax is an annual tax on individuals' net worth, i.e. the sum of all assets minus debt. As they are typically levied progressively on net wealth above a relatively high threshold, wealth taxes shift the tax burden toward the wealthiest households. There is little doubt that the wealth tax is an efficient tool to target wealth inequality. At the same time, a common concern is that wealth taxation hampers investment and drags down economic growth.

Still, the most pressing issue is perhaps the practical difficulties with administering and enforcing a wealth tax. While most taxes are levied on transactions taking place between two parties (e.g. employer and employee, buyer and seller), a wealth tax requires a regular reassessments of values that are not always directly observable. While the market value of assets like bank deposits, funds and listed shares could be based on arms-length transactions and third-party reporting, some assets are inherently hard to value. This is especially the case for real estate and shares in non-listed (non-traded) firms. Taxing such assets that are non-liquid and typically non-frequently traded, also raises liquidity concerns. In practice, existing and former wealth taxes have typically contained a lot of exemptions, valuation discounts and assessed tax values that differs from real market values. This again opens for avoidance and evasion opportunities.

In general, taxing capital entails distortions to saving and investments decisions, and inherently imposes an equity-efficiency tradeoff. Traditionally, capital taxes have been regarded as undesirable either because capital supply is assumed to be infinitely elastic in the long run (Chamley 1986; Judd 1985), or by asserting that the same distribution can be achieved, without distorting saving decisions, by only taxing labor income (Atkinson and Stiglitz 1976). Later works have questioned the underlying assumptions behind these results (e.g. Straub and Werning 2020) and highlighted that in practice there are many arguments in favor of taxing capital (see e.g. Bastani and Waldenström 2020). Still, the optimal taxation of capital poses a tradeoff between efficiency and equity.

A tax on wealth pushes this tradeoff to its extreme. It is directly targeted at reducing wealth concentration and could be made highly progressive. At the same time, it targets wealth accumulation which is a key element in economic growth. The relative merits of wealth taxation hinges on how it performs on both dimensions compared to other forms of capital taxation. Important questions are still unsettled, such as whether a wealth tax achieve given distributional objectives better than the alternatives. And furthermore, whether it is at all possible to achieve the same redistribution by reforming capital income taxes.

4.3 Existing literature

This section discusses the current literature and key institutional details relating to the three papers on wealth taxation in the thesis.

4.3.1 Taxing wealth compared to taxing the return to wealth

A tax on accumulated wealth is closely related to a tax on the return to wealth. For a given rate of return *r*, a tax on initial wealth W, can be expressed as $W(1+r)*t$. This is identical to a capital income tax ($W*r*\tau$) with tax rates harmonized such that $\tau = t*(1+r)/r$. For an asset generating a return of, say, 5 percent, levying a 1 percent wealth tax is equivalent to a 21 percent tax on return. Hence, if all assets had the same rate of return, and the tax applied to all assets, the two would be equivalent.

However, rates of return generally differ across assets and owners, both systematically and unexpectedly. Under a wealth tax, the tax liability is proportional to wealth independently of the return it generates. In that sense, a wealth tax could be seen as a tax on expected or imputed return to capital, with an effective rate (in pct. of return) decreasing with actual return (see e.g, Scheuer and Slemrod 2021). The higher is the rate of return, the lower is the equivalent capital income tax. As discussed below, this has implications for both equity and efficiency considerations regarding the choice between wealth taxes and capital income taxes.

To understand the economic implications of taxing imputed as opposed to actual return, it can be useful to think of capital income as comprising two elements: the normal return and excess returns, where the latter can reflect both economic rents, risk premium and the returns to effort and skill. Normal return can be described as the return to capital that just compensates for a delay in consumption. Taxing the normal return distorts savings by increasing the price on future consumption relative to current consumption and leisure. When after-tax return to savings is reduced, taxpayers are incentivized to reduce savings (a negative intertemporal substitution effect).[2]

[2] Like all other taxes, capital taxation also incurs income (or wealth) effects. Taxpayers need to finance their taxes due, e.g. by working more (consuming less leisure) or by reducing current or future consumption. From standard economic theory, the income effect is positive: As future consumption becomes more expensive, people will need to save more (before tax) to maintain a given level of future relative to current consumption. Thus, the overall effect of capital taxation on saving is theoretically ambiguous.

An argument often made against wealth taxes, compared to capital income taxes, is that it shifts the tax burden from economic rents, which is less likely to be distortionary, toward the normal return (Kopczuk, 2019; Adam and Miller, 2021). Favoring wealth holders with high rates of return might also seem undesirable from a distributional perspective. Thus, taxing capital income, which also captures excess returns, might seem more desirable for both efficiency and redistribution. However, an important feature of wealth taxation is that it is easily (and often) made highly progressive, targeting the wealthiest households.[3] With a high exemption threshold, the life cycle savings of most taxpayers can be exempted. It is not clear how distortive a tax on the normal return is to savings of the wealthiest, given that they have a lower propensity to consume (Fagereng et al. 2019). The size of the deadweight loss from taxing normal return depends on the taxpayers' elasticity of savings, which ultimately is an empirical question.

Guvenen et al (2023), on the other hand, find significant efficiency gains from shifting to wealth taxation as it reallocates capital to more productive investors. Ultimately, the efficiency effects depend on the source of excess profits – whether high returns results from market-failure or luck, or it reflects investors' productivity. Heterogeneity in investors abilities is usually assumed away in economic models, even if it is prevalent in data and an important factor to explain the observed patterns of wealth accumulation and concentration (Guvenen et al, 2023). Fagereng et al. (2020) documents persistent heterogeneity in rates of return across the wealth distribution, even within asset classes, with rates of return increasing with initial wealth holdings. Even if progress have been made in understanding return heterogeneity, more knowledge is needed to understand the relative importance of productivity differences versus economic rents.

[3] Progressivity is harder to obtain with capital income taxation, which is based on yearly realized income. A majority of OECD countries (30 of 38) tax most, or all, types of capital income at flat rates. Furthermore, with a wealth tax, the effective tax rate could in principle exceed 100 pct. of returns, as in the Warren proposal (see Saez and Zucman 2019a, Saez and Zucman 2022).

4.3.2 Accrual- or realization-based capital taxation

In the academic discourse, a main argument against wealth taxation is the practical difficulties in yearly assessment of assets, which implies both administrative costs and avoidance opportunities. Compared to other, more common tax bases, wealth is inherently difficult to value. This often leads to the conclusion that a wealth tax is not "worth the cost" (Advani et al., 2020; OECD, 2018; Bastani and Waldenström, 2020).

However, the fact that wealth tax is not based on realization is perhaps the most important reason the wealth tax has gained interest. In many countries there has been an increased focus on the fact that the wealthiest pay relatively little tax on their accrued capital income (see e.g. Saez and Zucman, 2019b). Taxation of gains is usually postponed until realization (and in many countries at lower rates than other income). Thus, earnings retained within the corporate sector is not taxed at the personal level. Wealth holders can easily retain profit within private holding companies, thereby accumulating wealth from untaxed earnings.

One may argue that if wealth accumulation only reflects a transfer of consumption between time periods, and wealth has no intrinsic value beyond financing consumption, there is little reason to discourage life cycle saving by taxing (Adam and Miller, 2021). However, the growing accumulation of wealth among the wealthiest cannot be explained by consumption smoothing over the lifetime or even between generations. This underlines that wealth has some value beyond financing consumption, which again could make the case for taxing it (Saez and Standcheva, 2018).

The wealth tax has to be paid regardless of cash flow. This raises potential liquidity issues for asset-rich but cash-poor taxpayers. Different solutions had been tried to alleviate liquidity issues. Several countries have exempted business assets from the wealth tax base or set an upper limit for the total tax (on income and wealth) as a share of income. Such tax ceilings and exemptions undermine the role of the wealth tax as an accrual based tax independent of realized return, opens up for avoidance opportunities and significantly reduce the effective wealth tax rates on the wealthiest (Alvaredo and Saez, 2009; Durán-Cabré et al, 2019).

Liquidity issues can partly be alleviated by setting a relatively high exemption threshold. In Norway, a common concern in the public debate used to be that people with low income and expensive houses struggled to pay the wealth tax. However, after a ten-fold increase in the exemption threshold from 2005 to 2015, this is a less pressing issue.[4] Wealth taxes are also often considered particularly harmful for young, capital-constrained firms which depend on financing from their owners. Thoresen et al. (2022) find that liquidity issues are relevant for only a small fraction of firm-owners under the current Norwegian wealth tax, and that young firms often have low book-values and hence are considerably less exposed than older firms. Liquidity is challenging to measure, especially to disentangle taxpayers with real liquidity problems from taxpayers who are "voluntary" illiquid by retaining earnings in holding companies. Halvorsen and Thoresen (2019) find that the Norwegian wealth tax is mostly borne by people with high lifetime income.

A well-designed wealth tax can serve as a complement to capital income taxation, ensuring a recurring tax on capital independent of realization.[5] Furthermore, not all capital income is taxed under the current income tax systems. In most countries, imputed income and capital gains on owner-occupied housing go untaxed. A wealth tax can in some senses be more comprehensive than a capital income tax, as it generally also taxes assets that do not generate monetary returns (OECD, 2018). Capital income taxation is also prone to avoidance and evasion, e.g. through private consumption within firms (Alstadsæter et al., 2014) or by "virtual realization"[6]. By accelerating tax liability, a wealth works like a backstop, ensuring some taxation on accrual basis. More generally, given the imperfect nature of tax design in practice, depending on multiple tax bases could be beneficial.

[4] The exemption threshold is still relatively low in Norway (aprox. EUR 170,000) but owner-occupied housing, which is the most important asset for most households, is valued at 25 pct. of assessed market value. The share of the population paying wealth tax is around 10 pct., compared to 30 pct. in 2005.

[5] In principle, capital gains could be taxed on accrual basis, i.e. by taxing the yearly change in asset values as capital income (mark-to-market). Such a tax would raise the same valuation and liquidity issues as a wealth tax. Compared to accrual-based income taxation, a wealth tax only differs in that it is proportional to wealth instead of returns (cf. part 2).

[6] Deferring or eliminating the tax by borrowing against an appreciated asset, and thereby monetizing the asset appreciation without triggering a taxable realization event.

Another alternative to taxing wealth is to tax wealth transfers at death. Besides different implementation issues, the main difference between a wealth tax and inheritance tax is that a wealth tax would also tax wealth that is accumulated and consumed through the life cycle. Ozkan et al. (2023) study lifecycle wealth dynamics and find that the wealthiest 0.1% group on average start their lives substantially richer, save at higher rates and earn higher returns. Their excess wealth is about equally accounted for by these three factors. From an equal opportunity perspective, there are arguments for taxing inherited wealth more than self-made wealth. However, parental wealth can provide benefits beyond the inheritance received. Berg and Hebous (2021) find that children of wealthy parent also have higher labor income. Such benefits would not be taxed under an inheritance tax.

4.3.3 Behavioral response margins and existing empirical literature

In a perfectly enforced, residence-based and comprehensive wealth tax setting, taxpayers can only affect their tax liability through either reducing actual wealth (savings) or by emigrating from the country. However, like most other taxes, wealth taxes are not perfectly implemented in the sense that taxable wealth is not equal to accumulated savings. This opens for other response margins in addition to real savings response.

When assets are treated differently for tax purposes, taxpayers can reduce their taxable wealth by reallocating portfolio without altering their total saving. Taxable wealth can be further reduced by taking on debt to finance investments in tax favored assets. Different tax treatment of different assets can result directly from the fact that assets like housing and closely held firms are hard to value (the latter is typically valued based on book values, as discussed in section 4.3.4). The favorable tax treatment of closely held firms is the focus of the first chapter in this thesis, were we study how firm owners responds to wealth taxation.

In addition, valuation issues and liquidity concerns have often led to exemptions and valuation discounts for such assets. In such a setting, the wealth tax can distort portfolio composition, resulting in overinvestment in assets that are valued below market value. The second chapter of this thesis utilizes the numerous changes in valuation discounts in the Norwegian wealth tax to identify how the level and composition of savings responds to both changes in tax rates and tax bases. In the third chapter, I study how these changes have affected the overall level and distribution of the tax burden.

In settings with weak enforcement and the lack of third-party reporting, taxable wealth can easily be reduced simply by underreporting the true value of assets, or overreporting debt. Even when third-party reporting is used extensively for domestic wealth, taxpayers can escape wealth taxes and other forms of capital taxation by offshoring wealth to tax havens. Alstadsæter et al. (2019) have documented that a significant share of financial wealth is held in tax havens, and that this wealth is mainly not reported for tax purposes. Offshore evasion may also involve investing directly in real assets in tax havens, such as unlisted corporations and real estate. Alstadsæter et al. (2022) use leaks on property ownership in Dubai, and document that a high share of Norwegian residents in these leaks did not report these assets to the tax authorities.

Behavioral effects of wealth taxation include both real responses in wealth accumulation and composition of savings, as well as avoidance and evasion responses. These response margins are not independent, and it is often difficult to disentangle the different margins of response in the data. The existing empirical literature has mainly focused on the elasticity of taxable wealth, i.e. measuring the overall (both real and reporting) responses.

Existing studies finds partly substantial responses in taxable wealth, but estimated effects vary tremendously across studies, see table 1. In the following, I discuss how these discrepancies are related to differences both in methods across studies, and in the design of wealth taxes (rate schedules, tax bases, reporting requirements and enforcement measures) across countries.

Several studies find that taxable wealth is very responsive to the wealth tax rate. Brüllhart et al. (2022) take advantage of variations in wealth tax rates over time across Swiss cantons, and find that a 1 percentage point increase in the wealth tax reduces taxable wealth by 43 pct. The authors argue that about a third of the total response is due to mobility, and half of the response is likely due to misreporting (evasion). In Switzerland, even financial wealth is self-reported which provides large scopes for evasion. This is different from other countries, such as Norway, where most financial assets (except foreign holdings) are third-party reported.

Table 1 Estimates of taxable wealth elasticities

Paper	Country	Method	Elasticity
Ring (2020)	Norway	Bunching	0,05
Seim (2017)	Sweden	Bunching	0,2
Jakobsen et al. (2020)	Denmark	Bunching	0,3
Brüllhart et al. (2020)	Switzerland	Bunching	0,8
Londoño-Velez and Àvila-mahecha (2020)	Colombia	Bunching	2
Agrawal et al. (2020)	Spain	Diff-in-diff	6 - 9
Jakobsen et al. (2019)	Denmark	Diff-in-diff	6 - 11
Durán-Cabré et al. (2019)	Spain	Diff-in-diff	15 - 32
Brüllhart et al. (2022)	Switzerland	Diff-in-diff	18 - 43

Bunching estimates are typically interpreted as measuring avoidance and evasion rather than real responses, as the true value of wealth depends on asset prices and therefore not easy to fine-tune. Bunching estimates are in general small. Seim (2017) estimates bunching elasticities between 0.1 and 0.27, mainly due to underreporting of assets that are not third-party reported. Ring (2020) finds a bunching elasticity of 0.05 for Norway. One exception is Londoño-Velez and Àvila-mahecha (2020), who find substantial bunching to the Colombian wealth tax where enforcement is weak. Garbinti et al. (2023) find no bunching at pure tax rate thresholds, but substantial bunching at thresholds where reporting requirements also changes, suggesting that reporting requirements are of key importance to the behavioral responses to wealth tax. Bunching at kink points may understate the overall responsiveness of taxable wealth if individuals near the threshold (often relatively low levels of taxable wealth) respond less than individuals well above the threshold.

Migration elasticities tends to be high when tax rates vary within countries, such as in Switzerland and Spain. Agrawal et al (2020) studies the effect of a decentralized wealth tax on interregional migration in Spain. They estimate that a 1 percentage point decline in the average tax rate, increases the numbers of filers in the region by 5-8 pct. and reported wealth by 6-9 pct. The moving patterns points towards reported, rather than real, migration. Furthermore, they find that Spain foregoes 5 pct. of the tax revenue due to tax-induced migration as the tax base shifts to the zero-tax region of Madrid.

While within-country migration is highly responsive to wealth taxation, there is little support for the claim that emigration poses a significant obstacle for nationally levied taxation (see Kleven et al. (2020) for a review of the empirical literature on migration responses to personal income taxes). Jakobsen et al (2023) find that wealth tax-induced migration is surprisingly small in Scandinavia; a one percentage point increase in the effective wealth tax rate is estimated to decrease the population of the very wealthy by 2 percent in the long run.

Taxable wealth can also be affected by asset composition responses, especially when the tax base is narrowed by exemptions and valuation discounts. Fagereng et al. (2023) document strong portfolio allocation responses to a valuation discount for shares in the Norwegian wealth tax. Durán-Cabré et al (2019) studies the effect of a reintroduction of wealth tax in Spain, and find that a 0.1 percentage point increase in the wealth tax rate reduces taxable wealth by 3.24 pct. They find no evidence of reduced savings. The response is mostly due to a substantial change in asset composition towards exempted (business-related) assets. In Spain, taxpayer could arrange their business to satisfy the exemption requirements for closely-held businesses, and in principle include any kind of wealth. Hence, exempting certain assets can also facilitate avoidance by shifting the form in which wealth is held.

There is little empirical evidence of a negative effect on real saving in response to wealth taxation. On the contrary, there is evidence that total wealth seems to be unaffected or decreases by less than the mechanical wealth-effect (Seim, 2017; Brüllhart et al., 2020; Durán-Cabré et al, 2019). Ring (2020) even finds that wealth taxation has a positive effect on saving, supporting a strong positive wealth effect that dominates the negative substitution effect. Jakobsen et al. (2020) study the effect of wealth taxation on wealth accumulation. They exploit a doubling in the exemption threshold only for married couples and a tax ceiling that limited the total tax relative to income in Denmark. They estimate a long-term elasticity of 9 pct. for the moderate wealthy and 11 pct. for the wealthiest. These elasticities likely also include evasion and avoidance responses, as half of the wealth taxpayers in Denmark were business owners and business wealth were self-reported, thus representing an upper bound on real wealth accumulation responses.

A key takeaway is that the elasticity of the tax base depends on policy choices. Different tax design over time and across countries makes it difficult both to measure changes in effective tax rates correctly (the explanatory variable), and to generalize the findings to other settings.

Even if both real and reporting responses incur real resource costs from taxation, understanding the underlying mechanisms of behavioral responses is crucial for policy implications. Existing evidence suggests that responses to wealth taxes are mostly in terms of reporting responses (legal avoidance or illegal evasion), and less about real responses in savings and wealth accumulation. The partly very high elasticities do not necessarily imply that wealth taxes are inherently very distortionary. As emphasized by Slemrod and Kopczuk (2002), elasticities are not structural parameters but depend somewhat on policy choices like the definition of the tax base and enforcement measures. Some avoidance and evasion opportunities can be removed by better design and enforcement. The existing evidence suggests that maintaining a broad tax base and extensive use of third-party reporting is of key importance when designing a wealth tax.

Advani and Tarrant (2021) summarizes the empirical literature on how taxable wealth adjusts in response to wealth taxation for the UK Wealth Tax Commission, and estimate that a well-designed (broad tax base and well enforced) wealth tax of 1 pct. would reduce taxable wealth by 7-17 pct. depending on the level of international migration. Assuming a 5 percent rate of return, this translates into an elasticity with respect to the net-of-tax rate of return of 0.33-0.81, cf. section 4.3.1.

As most wealth taxes in place have been plagued with poor design, uncertainty remains about what the magnitude of real responses would be if there were fewer avoidance opportunities. When opportunities to avoid or evade the wealth tax is large, one would expect the wealth tax to have sizable effects on reported wealth mitigating real effects on wealth accumulation, consistent with the hierarchy of behavioral responses discussed by Slemrod (2001). If evasion and avoidance opportunities were to be shut down, the real effects could be somewhat larger, but the overall response to taxable wealth is likely to decrease.

The empirical literature focuses on the effect of wealth taxes on those who pay the tax. Wealth taxation may also discourage risk-taking and entrepreneurship by those who are not currently wealthy but aspire to become wealthy in the future. This effect is much harder to identify by the quasi-experimental methods used in most studies, but it is clearly relevant for the long-run elasticity of capital supply with respect to capital taxes, which is a key parameter in the efficiency-equity trade off posed by capital taxes.

4.3.4 The valuation of firms

A key challenge when imposing a wealth tax is to estimate how much a private business is worth (see e.g. Daly and Loutzenhiser, 2021). While shares in listed firms can be valued based on the stock price, the value of shares that are not frequently traded needs to be estimated. In practice, private businesses are typically valued at book value, which is often well below market value due to the lack of inclusion of most intangible assets.

The difficulties in valuing firms are not specific to taxation. Also in standard accounting, book values mostly include tangible assets such as property, plant, and equipment, and largely exclude intangible assets, especially those that are internally generated (Corrado et al., 2022). From an economic point of view, all spendings that are expected to yield a return in a future period, and therefor adds to the value of the firm, can be viewed as investments. This includes spendings far beyond what is currently classified as R&D, e.g. spendings on brands, designs, software, new products, customer relations, human resources and business practice. According to estimates for the UK, investment in intangible assets exceeds tangible investments, where the largest component is in firm-specific training (Martin, 2019). As modern economies become more knowledge-intensive, the gap between accounting values and real market values of of firms is likely to increase.

Given that it is challenging to determine the market value of unlisted firms, a wealth tax will inherently favour intangible assets in such firms. This provides taxpayers an incentive to alter portfolio allocation, by investing more of their wealth in intangible assets within private businesses. The first chapter of this thesis explores how taxpayers respond to this incentive. To our knowledge, we are the first to document that the wealth tax causes firm owners to allocate more of their wealth into the firm.

4.4 Data

This thesis is based on high quality Norwegian register data provided by Statistics Norway.

In the first three papers we use annual data on taxable net wealth from individuals' tax returns. As Norway is one of few countries levying a wealth tax, tax returns contain detailed information on assets subject to the tax for the entire population of Norwegian taxpayers. This enables us to measure saving (changes in net wealth), as well as accurately computing the hypothetical wealth tax that would have applied under different tax regimes on a given net wealth (e.g. rule-driven changes in the wealth tax).

Most assets and liabilities are reported by third parties (e.g. bank deposits and listed and unlisted shares) or based on assessed values (e.g. real estate and cars). Still, there are three limitations to the data when it comes to measuring actual savings. First, we only observe the book value of unlisted shares, and thus notoriously underestimate their true economic value. This means that our measure of savings is imperfect, as a reallocation toward unlisted shares will show up as negative saving. However, since there have not been any rule-driven changes in this undervaluation over time, this does not contaminate our hypothetical tax measures. Second, there is a break in the data on real estate values in 2010. Prior to 2010, real estate tax values were based on historical cost (leisure homes still is). From 2010, tax values are assessed based on market values on comparable properties. We impute changes in housing values from before to after the based on observed changes in median tax values within each census tract, assuming that market values follow the local housing price indexes. Third, our data exclude pension wealth, which are not subject to wealth tax. The amount that can be invested in tax-preferred pension accounts is strictly limited in Norway, and directly-owned private retirement assets makes up less than 0.5 pct. of pension wealth. This suggests that the exclusion of pension wealth is not a major issue in our setting.

In the first paper, where we study the effect of firm-owners' wealth tax on their firms, we link firms and ultimate owner using the detailed Norwegian shareholder register. This enables us to merge our wealth data with firm-level accounting and tax data, as well as reported salaries on firm-employee-level.

In the fourth paper, we accessed annual data on reported salaries merged with data from our randomized experiment. Also, we add firm characteristics variables from the firm registry, such as organizational form, age of firm, and registry status.

4.5 Methodological approaches

At the heart of the credibility revolution in empirical economics is the quality of empirical research design and access to data (Angrist and Pischke, 2010). A good research design seriously addresses identifying assumptions and potential threats to validity.

Identifying causal effects of taxation requires that one can credibly identify the counterfactual outcome, i.e. what would have happened in the absence of the tax. The papers in this thesis use both a controlled randomized field experiment (paper 4) and quasi-experimental methods (paper 1 and 2).

Randomized experiments offer a powerful tool to identify causal effects. Random assignment prevents spurious correlation (and reverse causation), i.e. ensures that the causal variable (the "treatment") is independent of confounding factors. However, in many settings random assignment is not possible, desirable or too costly to implement. The power of randomized field experiments can also be weakened by non-compliance to the random assignment, attrition bias and the "Hawthorne effect" (if people know that they are part of an experiment, it can affect their behavior).

In the fourth paper of this thesis, we study tax evasion and deterrence effects of audits using at randomized audit experiment. In cooperation with the Norwegian Tax Administration, we designed and implemented a field experiment where firms were randomly assigned to audit and non-audit groups. To balance the need for efficient use of scarce audit resources, which prescribes a risk-based audit strategy, and the methodological need for credible identification, we used a stratified experiment design.[7] Firms were divided into strata with different probabilities of being audited. The risk-based stratification causes outcome variables to be imbalanced in the overall sample pre-treatment. In principle, we have multiple separate experiments, one within each stratum.

Since both treatment assignment and treatment effects differ between strata, controlling for stratum additively would not provide a consistent estimator (Rubin

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[7] In other settings, a common motivation for such a design is that stratification could increase precision if it is based on characteristics that are correlated with the outcome variable.

and Imbens, 2015). We therefor estimate the effect of audit by comparing outcomes across the treatment and control group post-treatment *within* each stratum: [8]

$$y_i = \sum_{j=1}^{J} \beta_j \times C_{ij} + \sum_{j=1}^{J} \tau_j \times Treat_i \times C_{ij} + u_i$$

where y_i denotes our outcome variable, $Treat_i$ is a dummy variable indicating whether firm i was randomly assigned an audit, C_{ij} are strata dummies and u_i is the error term. The population average treatment is the weighted average of the within-stratum average treatment effects, β_j, with weights being the share of firms within each stratum.

To increase precision and correct for any random imbalance, we also apply a difference-in-difference method, where we compare changes in the two groups from before to after the intervention. This design does not require the two groups, absent the intervention, to be equal in levels, but rests on the somewhat weaker assumption of a common trend (that the change in the outcome variable in the control group is representative for the counterfactual change in the treatment group).

As compliance with the random assignment is non-perfect, our estimate is measuring the "intention-to-treat"-effect (ITT). We also perform a 2SLS, using the random assignment as an instrument to estimate the local average treatment effect (LATE) of actually being audited.

Natural field experiments, or quasi-experiments, exploit variations over time or across populations to get at causal relationships. In the first and second paper of this thesis, we exploit a series of tax reforms to identify effects of the wealth tax on savings and firms' investment and employment.

Our identification strategy has similarities to a difference-in-differences approach, as we use the interaction of time and treatment for causal inference, controlling for time and treatment separately. However, in our setting there is neither well-defined

[8] This could also be done by transforming the parameter vector, cf. Imbens and Rubin, 2015.

treatment and control groups (treatment is continuous) nor a pre- and post-period (reforms occur every year). Rather, we analyze many years of stacked data where there have been multiple reforms to assess whether there is an “extra effect” associated with the changes in tax rules. This provides rich variation in tax rules over time and across taxpayers (both increases and decreases) and enables us to control flexibly for initial wealth and other predetermined characteristics without absorbing the independent variation in tax rules. However, the identifying assumption is less transparent than in a standard difference-in-differences, and it is not possible to assess the validity by examining pre-reform trends. In paper 1, we provide graphical validation in an event study where we estimate “effects” both prior to and after the base year.

While the tax rules are exogenous with respect to individual’s behavior, the tax of each individual obviously is not. The tax depends on factors, like the level of composition of wealth, which can be correlated with the outcome (spurious correlation) and also in itself can be affected by the tax rules (reversed causality). To isolate the causal effect of the tax reforms, we take two measures. First, to deal with potential reversed causality, we regress the outcomes of interest on *predicted* future tax on predetermined wealth. This ensures that our explanatory variable is predetermined, and not affected by the changes in the tax rules. This is similar to the approach commonly used in the taxable income literature estimating the elasticity of taxable income on the basis of tax reforms (see e.g. Gruber and Saez, 2002). Differently from the ETI-literature, however, we use the predicted tax itself as the causal variable instead of an instrument for the tax actually paid. The actual tax paid is not an independent variable, as it is measured after the “treatment” and thus is affected by the behavioral response we are trying to measure. [9]

Secondly, to deal with the spurious correlation or omitted variable problem, we compute and control for all the counterfactual wealth taxes that would have applied under all the tax regimes in the period we study. Thus, we identify the causal effect as the extra effect associated with the relevant tax schedule each year. This is different from what has commonly been done in the tax literature, where one has

[9] A similar argument is made by Weber (2014a).

controlled for base-year income or wealth in flexible ways.[10] We argue that our solution is more robust, as wealth enters with the exact same functional as in the causal tax function (the explanatory variable). Hence, our identifying assumption is that any spurious correlation with the outcome does not co-vary with the rule-driven tax changes.

4.6 Synthesis of papers

This thesis comprises four empirical papers analysing different behavioural responses to taxation.

The first three papers focus on real responses to wealth taxation, in terms of effects on both investments and the level and composition of savings. In the first paper, we look at how the wealth tax on firm-owners affects closely held businesses. The second paper looks more generally at the overall saving effects of wealth taxation for the whole population. In both papers, we use a series of tax reforms that provides rich quasi-experimental variation in the Norwegian wealth tax and calculate potential wealth tax with future tax rules given predetermined wealth. In the third paper, I use this potential wealth tax framework to study how changes in the Norwegian wealth tax have affected the overall level and distribution of the tax burden.

The last paper focuses on tax evasion responses and limits to third-party reporting. Using data from a randomized experiment linked with administrative micro data, we study how firms wage-reporting responds to audits targeted at detecting undeclared work.

Paper 1: An imperfect wealth tax and employment in closely held firms
Co-authored with Simen Markussen and Knut Røed. Published in Economica.[11]

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[10] See e.g. Weber (2014b). However, the "saturated control function" approach we use, is not uncommon within other fields of economics, see Bousyak and Hull (2021).
[11] This project was initially part of a research assignment from the Ministry of Trade, Industry and Fisheries.

In this paper, we utilize a series of wealth tax reforms in Norway to study how the wealth tax on business owners affects small firms' investments and employment decisions. A key concern in the public debate, has been that taxing the wealth of business owners may reduce the amount of capital available to their closely-held firms and drag down their value creation. However, wealth taxes typically favor investments in private businesses compared to other firm (and non-firm) investments. Since the true market value of such businesses is often not observable, they are typically valued at book value. This means that intangible assets, such as brands, customer relations, expertise and ideas are not included in the tax value of the firm. The wealth tax thus provides incentives to invest *more* in such assets. In line with this, we find that the wealth tax overall causes firm owners to allocate more of their wealth into the firm and that it has a positive effect on the firms' employment growth. Only for a small fraction of liquidity-constrained owners, we find indications of a negative, although not statistically significant, effect on firm's employment growth, suggesting that the owner is forced to pull capital out of the firm to pay the tax.

Paper 2: Saving effects of a real-life imperfectly implemented wealth tax: Evidence from Norwegian micro data

Co-authored with Annette Alstadsæter, Wojciech Kopczuk, Simen Markussen and Knut Røed. Published in AEA Papers and Proceedings

In this paper, we exploit variation in the Norwegian wealth tax rules to estimate the effect on overall saving. Also, we study how the saving response depends on the comprehensiveness of the tax base. The wealth tax is characterized by special valuation rules and discounts, resulting in different effective tax rates on different assets that varies over time and across taxpayers. When assets are treated differently for tax purposes, taxpayers can reduce their taxable wealth by reallocating portfolio without altering their total saving. Also, taxable wealth can be further reduced by taking on debt to finance investments in tax favored assets. Such reallocation opportunities could dampen the real saving response. We find that active saving responds negatively to wealth tax, but that this response becomes weaker under less comprehensive tax base. Furthermore, when the tax base is not comprehensive, we find that debt increases in response to higher wealth tax rate suggesting that debt is being used for tax avoidance. In our data, taxable net wealth constitutes on average 52 pct. of actual net wealth. Evaluated at this level, we find that a 1 percentage point increase in the wealth tax reduces savings by 4.9 pct. Our estimated elasticity is low compared to previous studies examining behavioral responses to wealth tax. While most studies analyze effects on taxable wealth, which includes reporting responses, our study seek to estimate the effect on the level and composition of actual saving.

Paper 3: How does the wealth tax work? (In Norwegian: "Hvordan virker formuesskatten?")
Published in Samfunnsøkonomen

This paper, which is in Norwegian, provides a descriptive analysis of the Norwegian wealth tax and how changes in the tax rules (as opposed to underlying changes in actual wealth) have affected the overall level and distribution of the tax burden. I also discuss potential economic effects of taxing wealth and review the existing literature on behavioral responses to wealth taxation. Over the last fifteen years, there has been a considerable reform-generated variation in wealth tax liability in Norway, due to changes in both tax rates, thresholds and special valuation rules for housing, real estate and business shares. With 2022-rules, I estimate that two thirds of total wealth tax is paid by the top 1 percent wealthiest households whose wealth is mostly held in unlisted shares. For the wealthiest taxpayer, I find that taxable wealth and wealth tax liability have varied by 60 pct. over the years 2005-2022 due to changes in tax rates and especially the valuation discount for business shares.

Paper 4: Limits to third-party reporting: Evidence from a randomized field experiment in Norway
Co-authored with Annette Alstadsæter and Kjetil Telle. Published in Journal of Public Economics

To collect the taxes due, tax administrations need credible information on whatever triggers tax liability. Taxpayers, on the other hand, have incentives to hide this information or underreport the true value of their tax base. Therefore, third party information reporting and employers' tax withholding is often seen as essential for modern tax collection (see e.g. Kleven et al., 2011 and Kleven et al., 2016). However, firms acting as intermediaries only curb tax evasion if the firms comply with their reporting and remittance obligations instead of colluding with the taxpayer. Such collusion may be important, especially in small firms, and obviously cannot be detected by comparing the two colluding parts' reporting in desk audits. In this paper, we find that on-site audits specially targeted at detecting undeclared work by (mostly small) firms in certain service sectors in Norway, led to a substantial increase in the number of employees and total wage reported by the firms. To our knowledge, we are the first to document the existence of unreported labor and collusive tax evasion in a developed country with fully implemented third-party tax reporting and withholding. In cooperation with the Norwegian Tax Administration, we designed and implemented a field experiment where firms were randomly assigned to on-site audits. We find that firms assigned to be audited on average increased their subsequent wage reporting by 18 percent relative to firms assigned to the control group. Furthermore, we find that the effect is decreasing with firm size, consistent with evasion being more easily coordinated in firms with few employees. Although our results are specific to our setting, they suggest that there are limits to third-party reporting, and that on-site audits can be a useful supplement to enforce taxes in situations where employees are likely to collude in tax evasion.

4.7 Final thoughts

Designing and implementing tax systems that are fair and efficient is, and will continue to be, a key issue for countries around the world. Tax systems are complex and involves detailed definitions of tax bases that are often difficult to measure and monitor. This thesis contributes to the demanding but important task of tax design, by shedding light on how taxpayers respond to different forms of taxation, given the practical limitation in implementing and enforcing them.

Taxes on labor income is often regarded as one of the easiest taxes to implement and enforce, given its' dependence on third-party reporting and employers tax withholding. In the last paper of the thesis, we show that there are limits to third-party reporting as a self-enforcing mechanism when taxpayers collude to underreport. Even in Norway's state-of-the-art system of information reporting, other measures might be needed to prevent tax evasion. Further research is needed to estimate the size of such evasion and to unravel the mechanisms behind it.

The wealth tax, on the other hand, is considered especially prone to implementation issues. The lack of observable market values, especially of unlisted shares, poses an inevitable challenge to the implementation of a broad based, uniform wealth tax. Furthermore, countries levying a wealth tax have often chosen to introduce different exemptions and valuation discounts for different assets.

Due to the lack of good data on wealth in most countries, as well as credible identification strategies, the evidence on real responses to wealth taxation is scarce. Most empirical research on wealth taxation studies the effect on taxable wealth, which is found to be highly elastic, mostly due to avoidance and evasion responses.

The research conducted for this thesis has uncovered multiple real responses to wealth taxes, given the actual implementation. The Norwegian setting provides both high quality, mostly third-party reported, wealth and ownership data and quasi-experimental variation in wealth tax rules, that enables the estimation of causal effects. In the first chapter, we find that the favorable tax treatment of unlisted businesses implies that the wealth tax induces a reallocation of wealth toward such assets. In the second chapter, we document that the effect on overall savings depends on the comprehensiveness of the tax base. When the tax base is not comprehensive, the wealth tax is easier to avoid, and saving response becomes weaker.

It should be noted that our results are partial, in that they capture the effects on those who pay the wealth tax. They do not provide answers to more general questions about the effects on overall investment, employment, and economic growth. To answer such questions would demand the inclusion of complete tax systems and general equilibrium effects.

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5 Papers

I Paper 1: An imperfect wealth tax and employment in closely held firms

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ORIGINAL ARTICLE

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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

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

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 ($j = 1, \dots, 6$), 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:

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

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

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

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

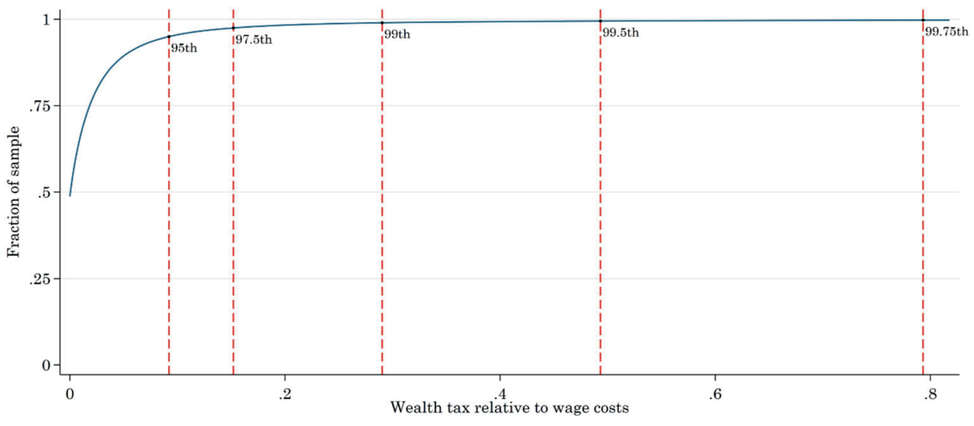

FIGURE 1 The distribution of owners' maximum wealth tax liability relative to the owner-weighted total wage cost in a closely held firm *Notes*: The figure shows the distribution of the highest possible wealth tax (out of all the regimes applying during 2007–17) that can be calculated given the observed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

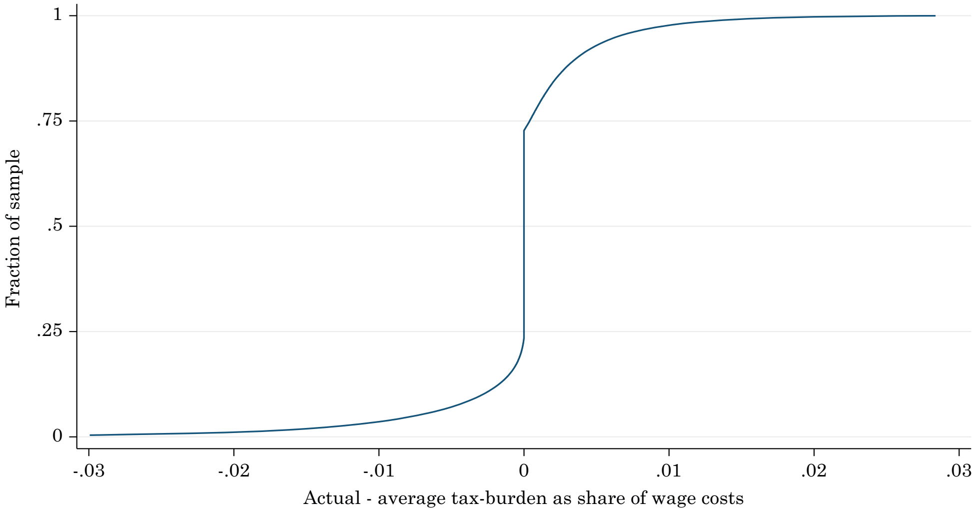

FIGURE 2 The reform-generated variation in wealth tax liability relative to the closely held firm's owner-weighted wage costs *Notes*: The reform-generated variation is defined as the difference between the actual and the average wealth tax liability, where the average is taken over all the tax regimes 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]

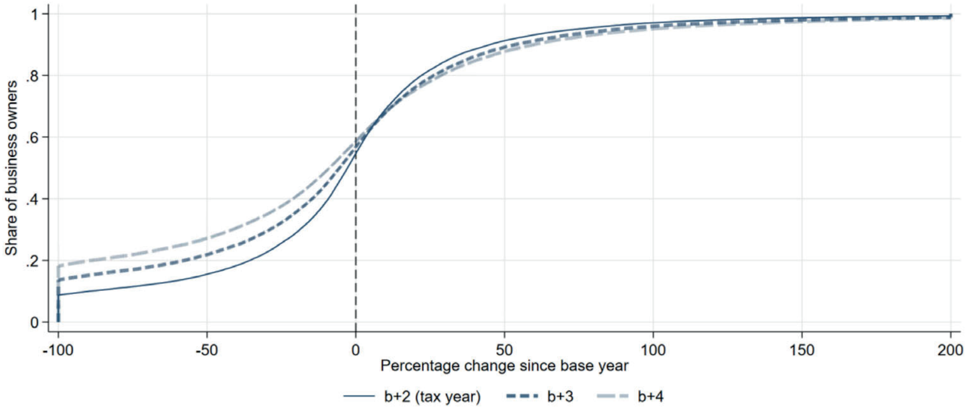

FIGURE 3 Distribution of the percentage change in total wage costs from the base year to the outcome year
Notes: The figure shows the cumulative density function of the relative change in the owner-weighted total wage bill from the base year to the potential tax year (two years after the base year),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

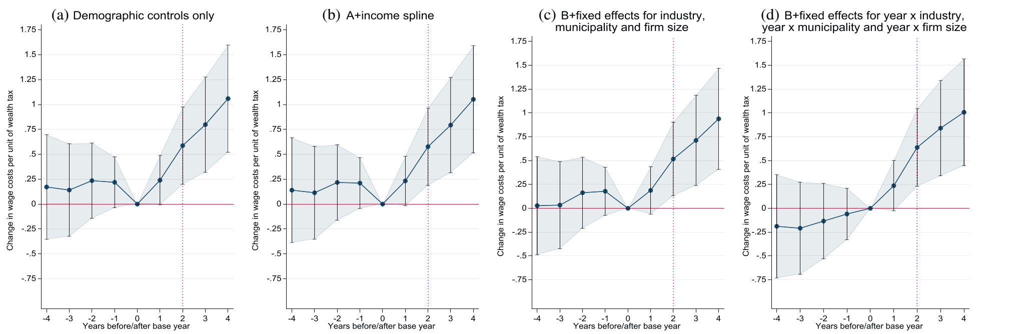

FIGURE 4 The estimated effects of potential wealth tax in year $b+2$ on total wage costs in years from $b-4$ to $b+4$. *Notes*: The graphs show the estimated δ coefficients from equation (2) when the dependent variable is the relative change in the owner-weighted total wage bill from the base year to 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

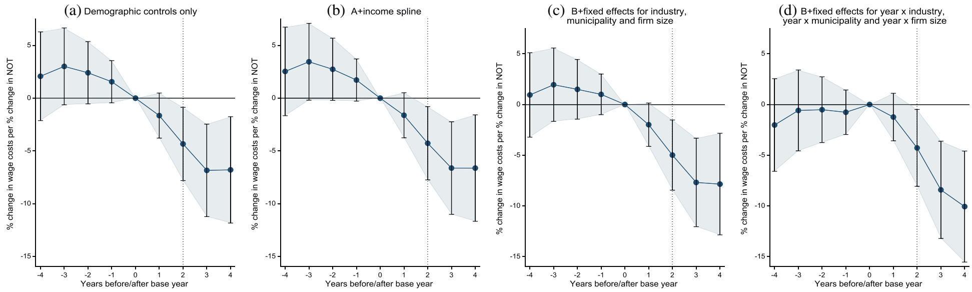

FIGURE 5 The estimated effects of potential net-of-tax rate in year $b+2$ on total wage costs in years from $b-4$ to $b+4$. *Notes*: The graphs show the estimated δ coefficients from equation (2) when the dependent variable is the relative change in the owner-weighted total wage-bill from the base year 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.

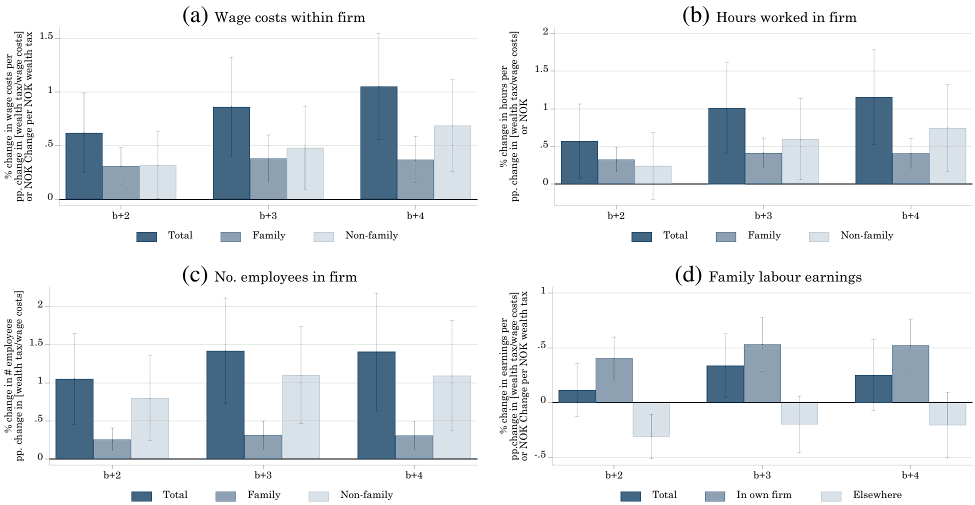

FIGURE 6 The estimated effects of potential wealth tax on employment in the family-controlled firm and elsewhere (with 95% confidence intervals) *Notes*: In panel (a), the estimates denoted "Total" are repeated from column (D) of Table 3. The estimates denoted "Family" and "Non-family" are based on the 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

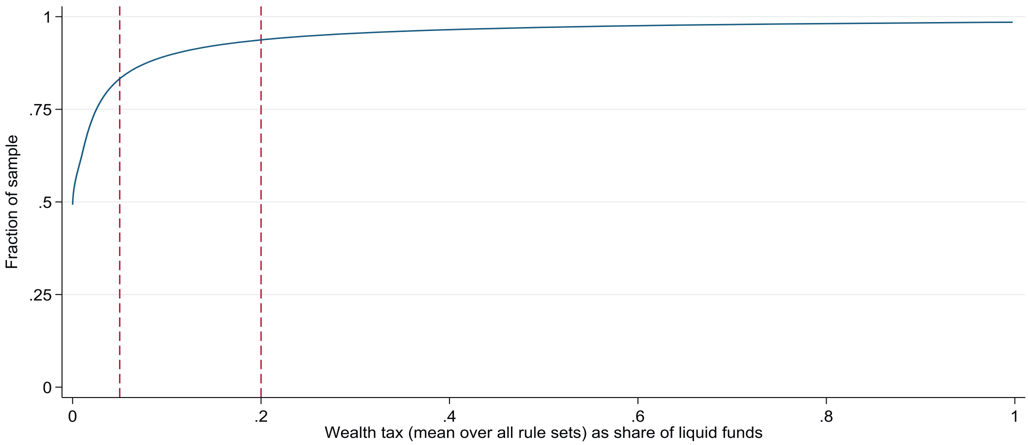

FIGURE 7 The distribution of owner illiquidity in relation to wealth tax exposure *Notes*: Illiquidity is defined as average expected wealth tax liability (calculated for all base years and over all the 11 tax regimes that have existed in our data period) relative to the taxpayer's liquid assets. The 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.

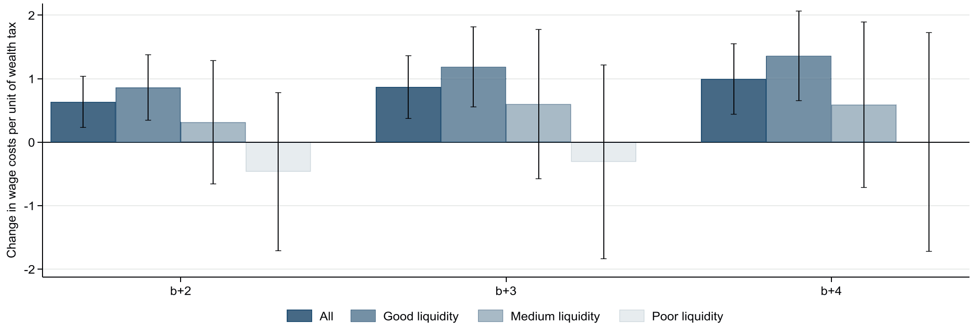

FIGURE 8 The estimated effects of potential wealth tax on total wage costs, by owner's liquidity *Notes*: The graphs show the estimated δ coefficients from equation (2) when the dependent variable is the relative change in the owner-weighted total wage bill from the base year to the outcome year, and 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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II Paper 2: Saving effects of a real-life imperfectly implemented wealth tax: Evidence from Norwegian micro data[12]

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Saving Effects of a Real-Life Imperfectly Implemented Wealth Tax: Evidence from Norwegian Micro Data†

By Annette Alstadsæter, Marie Bjørneby, Wojciech Kopczuk, Simen Markussen, and Knut Røed*

Despite their recent popularity in policy and academic circles, wealth taxes are currently used only in a few countries. This form of taxation is difficult to implement for two main reasons. First, it requires regular valuation of assets, often in absence of arms-length transactions or other means of easy assessment. Second, taxing assets rather than realized income raises liquidity concerns. In practice, policymakers may either push ahead, therefore leading to costly and difficult administration and discontent of taxpayers, or pursue practical compromises that make valuation and liquidity concerns easier to handle.[1]

We use the Norwegian context to illustrate the complexity of an actual implementation of a wealth tax and show that the sensitivity of saving to taxation depends on this complexity.

I. Complexity of Wealth Tax Implementation

Empirical evaluations of behavioral responses to wealth taxes naturally focus on the base of the tax as implemented in practice (Seim 2017; Londoño-Vélez and Ávila-Mahecha 2021; Jakobsen et al. 2020; Brülhart et al. forthcoming). However, each context corresponds to a different base that is never equivalent to taxpayers' net worth due to exemptions, valuation rules, or differences in effective tax treatment of different assets: there is not a single "wealth tax."

Figure 1 illustrates this issue in the context of the Norwegian wealth tax. Prior to 2013, the top statutory rate was set at 1.1 percent and then reduced to 0.85 percent by 2015; a lower rate of 0.9 percent applied until 2008, and the threshold for being subject to the tax evolved substantially from 151,000 NOK net taxable wealth in 2005 to 1,480,000 NOK in 2018, the last year that our data cover. These changes barely start to describe the tax system though, because the *base* of the tax changed repeatedly during that period. Special rules applied to housing, listed and unlisted shares, and business real estate.

Prior to 2010, valuation of housing was based on historical cost with annual adjustments; starting in 2010, it is assessed by Statistics Norway based on market transactions in the same area. Real estate is included in taxable wealth with a discount—75 percent for primary housing and a smaller discount for second houses that declined from 60 percent to 10 percent over time. Business real estate is assessed based on rental value and at a discount that evolved over time, mimicking treatment of second houses until 2016 and treatment of businesses since. Business shares were discounted before 2008 and since 2016, with additional changes over time, but there is also disparity between subclasses. While listed shares are taxed at market value, unlisted shares are included at book value, therefore leading to undervaluation (which is not reflected in Figure 1, because we do not observe the economic value). Finally, only since 2017, asset and associated debt are treated jointly for valuation purposes.

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[1] See Saez and Zucman (2019) for a wealth tax proposal and Kopczuk (2019) and Scheuer and Slemrod (2021) for discussions of problems with this approach.

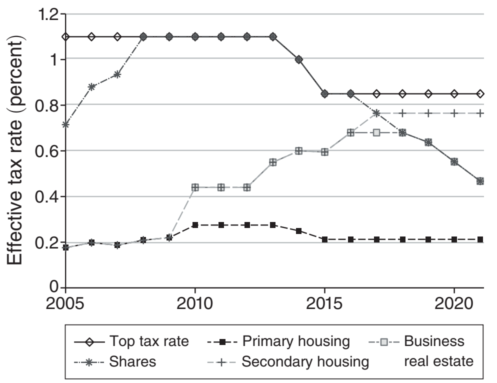

Figure 1. Wealth Tax Rates

Notes: Statutory tax rates taking into account asset-class-specific discounts and, for housing prior to 2010, effective tax rate accounting for undervaluation due to reliance on historical assessments, as described in the text.

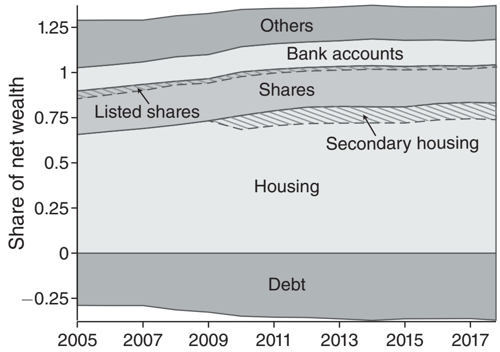

Figure 2. Composition of Net Wealth

Notes: The figure shows decomposition of net wealth, with positive and negative parts (debt) adding up to 100 percent, for those aged 40–75 with gross wealth over 100,000 NOK (the estimation sample, but without winsorizing or conditioning on reporting in year $t+2$). Separate information about secondary housing is only available starting in 2010.

In what follows, we will exploit variation generated by these rules to shed a light on behavioral responses to the wealth tax.

II. Data

We rely on detailed administrative tax data that contain information on assets subject to the wealth tax and demographic information and that cover the period from 2005 to 2018. In our estimation, we use the universe of all 40–75 old Norwegian residents with at least 100,000 NOK (in 2015 Norwegian kroner, using National Insurance inflation adjustments) in gross wealth. We impute pre-2010 values of real estate based on the observed change in the median tax value from 2009 to 2010 within each census tract and, for prior years, the annual rule-driven adjustments of tax values, assuming that market values follow housing price index. The largest data limitation involves valuing unlisted shares that we only observe at book value rather than their true economic value.

Figure 2 shows the underlying composition of assets as shares of net worth (assets minus debt, not accounting for discounts). Housing is by far the largest category. It increased in importance over time, and its growth has been driven (when we can separate it) by particularly tax-advantaged primary housing. Debt increased over time, in particular after 2007. Unlisted assets, despite undervaluation in our data, are a significant component, while listed assets are small and shrunk further over the years.[2]

III. Empirical Strategy

Our empirical strategy builds on the basic taxable income elasticity framework (Saez, Slemrod, and Giertz 2012). For a given outcome y, we relate it to the net-of-tax rate $\ln(1-\tau)$ and virtual wealth z, where τ and z can be calculated based on actual behavior and the tax system in place.[3]

There are three challenges to an approach like this. First, we study wealth, a stock, rather than income, a flow. Second, tax rate and virtual wealth are obviously endogenous. Third, as we have just discussed, describing the tax system by the tax rate alone misses other aspects of the

[2] Our definition of wealth does not include pension wealth. Directly owned private retirement assets are small in Norway (less than 0.5 percent of pension wealth).

[3] The virtual wealth is defined as $z = \max(0, \text{net taxable wealth}) \cdot \tau -$ actual tax liability and is interpretable as a wealth effect. Changes in the base have a potentially large effect on average tax rate and are reflected in virtual wealth. In particular, distinguishing between average and marginal tax rates has been shown to be important in the context of responses to the 2010 change in housing assessments (Ring 2020).

base and, in particular, base changes. We discuss how we tackle each of these issues in turn.

We study the impact on changes over a two-year period, $(y_{t+2} - y_t)/y_t^B$, where y is a variable of interest and y_t^B is a base year normalization variable (gross wealth). We present annualized results (divided by two) and use the same normalization for other Norwegian krone–denominated variables. This approach raises a question of how to think about the heterogeneity of the rates of return of assets in taxpayers' portfolio that gives rise to mechanical changes in the value of net worth. Such effects may be important. For example, the findings of Brülhart et al. (forthcoming) suggest that the observed response of the wealth tax base to local variation in wealth tax rates is partly due to market-level changes in the value of real estate. Heterogeneity in rates of return leads to different changes in the wealth of taxpayers with different portfolios, absent any action. To focus on *active* saving, our main strategy is to modify y_t to remove the mechanical effect (due to aggregate asset-specific rate of return) and include mechanical changes in portfolio components as controls; we show the results for total saving as a robustness check.

In order to isolate the exogenous impact of reforms, we first, as in the taxable income literature, calculate simulated tax system variables that use the period $t+2$ tax system but rely on information at time t. Still, information at time t is likely to be correlated with changes between t and $t+2$ for a variety of reasons, including mean reversion (a major concern in the taxable income literature) or persistence of the stock variable.

To deal with this identification issue, we follow the approach from the work on social welfare programs (Røed, Jensen, and Thoursie 2008; Fevang, Hardoy, and Røed 2017). We compute and control for simulated wealth tax parameters that *would have applied* in period t under each of the tax regimes during the data period (2007–2018). This corresponds to 12 different sets of tax system variables (indexed by calendar years and hence distinct from tax parameters of interest that are indexed by current t) that share association with the residual due to reliance on base year but do not reflect the t to $t+2$ tax change.

Finally, we deal with changes in the base by extending the approach of Kopczuk (2005), who studied the sensitivity of income to tax rate and tax base. We account for tax rate τ and a measure of tax base $1 - \gamma$, with the elasticity to the tax rate allowed to vary with γ. A simple implementation of this idea is to use the *actual* person-specific tax base—in our context, we define $1 - \gamma$ as the ratio of taxable wealth to total wealth. This variable varies between zero and one and can be constructed both at a point in time and as a simulated value using the tax system and information from another period. Thus, the approach applied to the tax rate easily extends to γ. Given specification

$$y = \varepsilon \cdot \ln(1 - \tau) + \beta \cdot \gamma \ln(1 - \tau) + \cdots,$$

our interest is in parameters ε and β, with the tax system characterized by the base of γ corresponding to the elasticity of $\varepsilon + \beta\gamma$. In particular, ε would be the elasticity under a comprehensive tax base, while $\varepsilon + \beta$ would be the elasticity under a system that effectively has a null base. Hence, a strong testable prediction of this approach (out of sample and assuming linearity) is that $\beta = -\varepsilon$.

Table 1—Response of Net Assets

	(1)	(2)	(3)	(4)
$\ln(1-\tau)$	1.991	3.928	7.369	6.609
	(0.032)	(0.061)	(0.180)	(0.173)
γ			−0.072	−0.030
			(0.003)	(0.003)
$\gamma\ln(1-\tau)$			−5.154	−5.261
			(0.201)	(0.197)
z		5.749	8.389	7.419
		(0.178)	(0.223)	(0.216)
N	14,424,284	14,424,284	14,424,284	14,424,284
R^2	0.071	0.072	0.104	0.074

Notes: Data are winsorized at 1 percent and 99 percent, by year. Standard errors are in parentheses, clustered at the individual level. Regression estimated has the form of $y = \varepsilon \ln(1 - \tau) + \beta\gamma \ln(1 - \tau) + \delta z + \xi\gamma + \sum_{i=2007}^{2018} (\varepsilon_i \ln(1 - \tau_i) + \beta_i \gamma_i \ln(1 - \tau_i) + \delta_i z_i) + \pi d + \epsilon$, where d are demographic and other controls. Specifications 1–3 show the effect on active saving and control for mechanical rate-of-return changes in asset values. As a robustness check, the dependent variable in specification 4 is total saving.

IV. Results

Table 1 shows the effect on net assets. Controlling for the tax rate alone (column

TABLE 2—COMPONENTS OF NET WEALTH

	Gross	Debt	Housing	Unlisted	Listed	Bank accounts
$\ln(1-\tau)$	7.730	0.361	8.726	−0.091	−0.601	−2.364
	(0.224)	(0.145)	(0.201)	(0.066)	(0.030)	(0.095)
γ	−0.059	0.013	−0.043	0.006	−0.007	0.002
	(0.004)	(0.003)	(0.004)	(0.001)	(0.000)	(0.001)
$\gamma\ln(1-\tau)$	−6.904	−1.750	−8.216	0.044	0.677	2.761
	(0.251)	(0.155)	(0.227)	(0.075)	(0.034)	(0.107)
z	7.751	−0.639	8.728	−0.162	−0.628	−2.004
	(0.276)	(0.171)	(0.254)	(0.073)	(0.035)	(0.125)
N	14,424,284	14,424,284	14,424,284	14,424,284	14,424,284	14,424,284
R^2	0.048	0.029	0.060	0.021	0.040	0.024

Notes: See notes under Table 1.

1) corresponds to the elasticity of about 2.[4] Controlling for virtual wealth (column 2) strengthens the effect, and adding the tax base (column 3) changes the results quite a bit. First, the elasticity under a comprehensive base is 7.4, much larger. This is not, though, the elasticity that characterizes the tax system—that parameter is $7.369 - \gamma \cdot 5.154$, reflecting the presence of a base effect. It can be evaluated for any particular year or situation by using the corresponding value of γ. When evaluated at the average value for individuals subject to the wealth tax in our data, $\gamma = 0.477$, it corresponds to the elasticity of 4.91.

While coefficients on $\ln(1-\tau)$ and $\gamma\ln(1-\tau)$ are not exactly equal in absolute values, they are of similar magnitude. The final column shows that focusing on total rather than active saving makes a minor difference.

Table 2 shows results for components of net worth. The effect is primarily driven by gross assets. Given a close-to-null direct effect on debt, the total tax effect at realistic positive values of γ is negative, indicating that debt increases in response to higher tax rates when the tax base is not comprehensive. This is consistent with debt being used for tax avoidance. Housing is the main driver of the response, possibly due to local price effects, with coefficients mimicking the overall effect on gross or net assets. The effects on listed and unlisted assets are generally small, while the effect on deposits goes in the unexpected direction but may be consistent with Ring (2020), who found small liquidity-motivated increases in saving using a different identification strategy.

The results imply a strong active saving response under a comprehensive system that becomes weaker under imperfect implementations. Note that we studied the effect on real active saving rather than on *taxable* wealth: a weaker response of saving to an easier-to-avoid tax is consistent with taxable wealth responding more strongly.

V. Conclusion

Actual wealth taxes are complex and cannot be characterized by tax rates alone. The Norwegian wealth tax, in particular, treats different asset classes differently, and it varied this disparate treatment over time. We sketched a strategy to parsimoniously incorporate both base and rate effects to study the behavioral impacts of the wealth tax.

[4] Noting that the tax of interest is on wealth rather than income helps in interpreting the magnitude. A 1 percent wealth tax is comparable to a 20 percent capital income tax when the rate of return is about 5 percent. Hence, a change in *capital income* tax rate by 1 p.p. is of the same order of magnitude as a 20-times-smaller change in the wealth tax, and thus—if the economic impact were similar—wealth tax elasticity should be 20 times larger. Adjusting by a factor of 20 makes the elasticity of 2 comparable to the elasticity of saving to capital income tax of 0.1.

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III Paper 3: Hvordan virker formuesskatten? (in Norwegian)

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Hvordan virker formuesskatten?[1]

Formuesskatten har vært gjenstand for kontinuerlig debatt og har vært en av de viktigste valgkampsakene gjennom minst tre Stortingsvalg. Ingen skatt har nok heller gjennomgått så store endringer over tid. I motsetning til andre skatter der det hovedsakelig er satser og innslagspunkt som endres, har formuesskatten gjennomgått en rekke endringer i selve skattegrunnlaget gjennom endringer i ulike verdsettelsesrabatter. I denne artikkelen viser jeg hvordan formuesskatten er fordelt i Norge – både mellom husholdninger og mellom ulike skatteobjekter. Ved å koble registerbaserte formuesdata med en detaljert beskrivelse av skattereglene for årene 2005–2022, viser jeg også hvordan formuesskatten har endret seg over tid som følge av regelendringer. Formuesskatten virker sterkt omfordelende: I 2022 anslår jeg at de 1 prosent mest formuende vil betale 2/3 av samlet formuesskatt. Men det er også denne gruppen som har størst glede av rabatten på såkalt «arbeidende kapital»: For topp 1 prosent domineres formuene fullstendig av ikke-noterte aksjer. Med det norske skattesystemet som bakteppe gir jeg en kortfattet oppsummering av den empiriske forskningslitteraturen knyttet til effekter av formuesskatt. Gjennomgangen viser at effektene i vesentlig grad avhenger av hvordan skatten utformes og håndheves i praksis. Jeg konkluderer med at litteraturen ikke kan gi noe entydig svar på hvorvidt de uheldige vridningseffektene er større for formuesskatt enn for andre former for kapitalbeskatning.

INTRODUKSJON

Få land har formuesskatt i dag, men skatten har de senere år fått en fornyet interesse internasjonalt. I flere land er innføring av formuesskatt lansert som et mulig tiltak for å styrke offentlige budsjetter og motvirke økende ulikhet. Samtidig er det uro for at formuesskatt kan ha uheldige virkninger på sparing, investering og økonomisk vekst.

[1] E-post: marie.bjorneby@nmbu.no. Artikkelen er del av forfatterens PhD ved Handelshøyskolen, NMBU og inngår i en større prosjektportefølje på formuesskatten i samarbeid mellom Frischsenteret og Skatteforsk ved NMBU og som del av NFR-prosjekt 315769. Artikkelforfatteren har i PhD-perioden hatt permisjon fra stilling i Finansdepartementet. Takk til Knut Røed, Annette Alstadsæter og en anonym fagfelle for nyttige innspill.

Den pågående Covid-19 pandemien har aktualisert behovene både for å styrke omfordelingen, sikre skatteinntekter og å fremme verdiskaping. Skattepolitikk handler i stor grad om å veie disse, dels motstridende, hensynene mot hverandre. Mens man i andre land har diskutert formuesskatt som et mulig tiltak for å bøte på det økonomiske sjokket som følge av pandemien, har man i Norge redusert formuesskatten på aksjer som et tiltak for å dempe de økonomiske konsekvensene for bedrifter. [2]

En hovedårsak til at formuesskatten igjen har kommet på dagsorden internasjonalt, er et økende fokus på at de rikeste har langt høyere reelle inntekter enn den inntekten de skatter av (Saez og Zucman, 2020).[3] Dette skyldes at selskapsoverskudd først skattlegges når det realiseres som utbytte eller gevinst på personlig hånd, og at en stor andel av selskapsoverskuddene ikke realiseres, men holdes tilbake i selskapssektoren.

Mens kapitalinntekter først skattlegges når de realiseres, sikrer formuesskatten en løpende beskatning uavhengig av realisasjon (Saez og Zucman, 2019a). Men her ligger også mye av kritikken mot formuesskatt: At skatten må betales uavhengig av (realisert) inntekt, kan skape likviditetsutfordringer med å betale skatten. Det at skattegrunnlaget ikke knyttes til en observerbar transaksjon, som utbytte eller realisert gevinst, gjør det også krevende å fastsette skattegrunnlaget (verdsettelse). Dette gjør at formuesskatten er krevende å implementere (Kopczuk, 2019; Scheuer og Slemrod, 2021).

I denne artikkelen diskuterer jeg mulige virkninger av formuesskatten, gitt kompleksiteten ved den faktiske implementeringen av skatten, og sammenholder disse med andre former for kapitalbeskatning. Kapitalbeskatning kan grovt sett deles inn i tre hovedgrupper: Skatt på avkastning fra kapital (renteinntekter, selskapsoverskudd, utbytte og gevinster), skatt på overføring av kapital (arveavgift og dokumentavgift) og skatt på kapitalbeholdning (formuesskatt og eiendomsskatt). Selv om Norge er et av få land som har formuesskatt, har andre land ofte høyere (og dels progressive) skatter på kapitalinntekt, fast eiendom og arv. I Norge ble arveavgiften fjernet fra 2014. Samlet skatt på beholdning og overføring av kapital (skatt på formue og arv, eiendomsskatt og dokumentavgift) utgjør 3,2 prosent av totalt skatteproveny i Norge, hvorav formuesskatten utgjør litt under halvparten. Dette rangerer Norge på 26. plass av 38 OECD-land, der gjennomsnittet er 5,5 prosent av totalt skatteproveny.[4]

Alle skatter på kapital gjør det mindre lønnsomt å spare og kan dermed bidra til å redusere samlet sparing. Størrelsen på denne type effekter er imidlertid omstridt. Forskningslitteraturen gir ikke noe utvetydig grunnlag for å fastslå om disse vridningseffektene alt i alt er større eller mindre om kapitalbeskatningen legges på selve formuen eller på avkastningen av den.

Selv om kapitalbeskatning reduserer lønnsomheten av å spare, er det ikke gitt at skatten påvirker investeringsnivået. Bjerksund og Schjelderup (2021) viser at en uniform skatt, som likebehandler alle eiendeler og gjeld, ikke påvirker lønnsomheten av en gitt investering. Verdien av en gitt investering bestemmes av forventet avkastning etter skatt sett opp mot den avkastningen som kan oppnås ved alternative plasseringer, justert for risiko. Når avkastningen skattlegges likt uavhengig av hvor formuen investeres, vil skatten redusere avkastningskravet (gitt ved alternativavkastning) proporsjonalt med avkastningen. Dermed vil lønnsomheten være upåvirket. Dette gjelder også lønnsomheten av å investere innenlands eller utenlands, forutsatt at utenlandsinvesteringene ikke skjules for skattemyndighetene. En uniform skatt kan likevel, dersom skatten fører til redusert sparing, svekke tilgangen på innenlandsk privat kapital. I hvilken grad dette påvirker det samlede investeringsnivået, avhenger av hvorvidt det fylles opp med kapital fra utlandet.

Siden formuesskatten ikke er uniform, vil den i praksis påvirke lønnsomheten og dermed hvordan man velger å investere formuen (som i bolig, bank eller aksjer mv.). Slike vridningseffekter kan medføre et effektivitetstap fordi kapitalen ikke kanaliseres dit den kaster mest av seg for samfunnet. Dagens formuesskatt forskjellsbehandler både som følge av eksplisitte rabatter/unntak i skattereglene og som følge av at noen eiendeler er vanskelig å verdsette. Formuesskatten kan dermed bidra til endret spare- og investeringssammensetning. I tråd med dette finner Bjørneby mfl. (2020) i en analyse av norske data at

[2] Aksjerabatten ble økt fra 25 til 35 prosent i 2020, jf. Prop. 126 L (2019–2020) Endringer i skatteloven (økonomiske tiltak i møte med virusutbruddet).

[3] For studier på norske data, se Alstadsæter mfl. (2019), Aaberge mfl. (2020) og Halvorsen og Thoresen (2020). Dette har også blitt trukket frem i en reportasjeserie i Dagens Næringsliv (https://www.dn.no/magasinet/dokumentar/skatt/formue/fritaksmetoden/norges-rikeste-far-80-milliarder-i-skattefrie-inntekter-i-aret/7-1-z_r7vzct) og i det britiske tidsskriftet The Economist (https://www.economist.com/briefing/2019/11/28/economists-are-rethinking-the-numbers-on-inequality).

[4] OECD Revenue Statistics 2021 (http://oe.cd/revenue-statistics)

formuesskatten fører til at majoritetseiere i små og mellomstore virksomheter i gjennomsnitt plasserer mer av formuen sin i virksomheten, som typisk har lav skattemessig verdsettelse. Hansen og Sandvik (2022) viser i en teorimodell at en formuesskatt som favoriserer aksjeinvesteringer sammenlignet med sikre investeringer (rentepapirer), øker lønnsomheten av aksjeinvesteringer med lav risiko (som er nære substitutter til sikre plasseringer), mens den reduserer lønnsomheten av mer risikable aksjer.

Formuesskatten har selvsagt også, i likhet med alle andre skatter, den effekten at skattyter sitter igjen med mindre penger etter skatt. Dette er ikke en samfunnsøkonomisk kostnad ved beskatningen, men en overføring fra privat til offentlig sektor.[5] Dersom dette skal være et gyldig argument for at formuesskatten bør reduseres (som for et gitt nivå på samlet skatt, betyr at andre skatter må øke), må det være fordi de som betaler formuesskatt alternativt ville brukt pengene på en måte som er mer verdifullt for samfunnet, sammenlignet med andre grupper av skattytere.

Et særtrekk ved formuesskatten er at skattebetalingen ikke er knyttet til en kontantstrøm. Dette kan skape likviditetsutfordringer med å betale skatten. I den offentlige debatten pekes det ofte på at tilgangen på kapital for små, nært eide selskaper svekkes når eieren må betale formuesskatt selv i år der virksomheten går med underskudd. Studier på norske data viser at slike likviditetsskranker i praksis er lite utbredt (Røed mfl., 2020; Thoresen mfl., 2021), men man kan ikke utelukke at enkelte eiere må ta penger ut av virksomheten for å betale formuesskatt.

Ulike land har forsøkt ulike løsninger for å demme opp for potensielle likviditetsutfordringer. Flere land, inkludert Norge fram til 2008, har hatt en øvre grense for samlet formues- og inntektsskatt som andel av inntekt. En slik takregel undergraver imidlertid formuesskattens rolle i å sikre en løpende beskatning i tilfeller der det rapporteres svært lav skattepliktig inntekt relativt til formue. Enkelte land, som Frankrike og Spania, har unntatt selskapsformue i nært eide selskaper, med det resultat at store formuer ble overført til slike selskaper og skattegrunnlaget ble uthulet (Alvaredo og Saez, 2009; Durán-Cabré mfl., 2019). En annen mulig løsning er å gi skattyter mulighet til å utsette skattebetalingen. I USA finnes det ordninger for å utsette betaling av skatt på arveoverføringer (mot en rente) inntil eiendelen selges, men i praksis er det svært få som benytter seg av ordningen (Saez og Zucman, 2019b). Tilsvarende erfaring har man i Norge hatt med ordninger som gir betalingsutsettelse for formuesskatt for eiere av virksomheter som går med underskudd (først innført for 2016/2017, gjeninnført fra 2020 som et tiltak for å dempe de økonomiske virkningene av korona).

En formuesskatt må i praksis delvis baseres på ikke-observerbare verdier, i motsetning til skatt på kapitalinntekter som i stor grad baseres på observerbare transaksjoner. Implementeringen av en formuesskatt må ta hensyn til dette, enten ved å forsøke å identifisere reelle (ikke-observerbare) markedsverdier med de administrative kostnadene det medfører, eller ved å kompromisse på prinsippet om å skattlegge reelle verdier.

Formuesskattens effekter henger nært sammen med hvordan skatten utformes i praksis. I de landene som har eller har hatt skatt på formue, har skatten vært preget av en rekke særordninger, verdsettelsesrabatter og unntak som gjør at skattepliktig formue avviker fra reell formue. I praksis er det dermed ikke én formuesskattesats, men mange ulike effektive skattesatser på ulike eiendeler. Dette gir insentiver til å omplassere formuen, enten reelt eller bare «på papiret», for å spare skatt. Dette uthuler skattegrunnlaget og svekker skattens fordelingsegenskaper. Avvikene mellom skattemessige verdier og reelle verdier av formuesobjekter gjør også at det er krevende å oppsummere virkningene skatten basert på erfaringer fra land som har hatt formuesskatt.

Desto større mulighet skattesystemet gir for å unngå skatt ved å omplassere formuen, desto mindre vil vi forvente at skatten påvirker samlet sparing. Alstadsæter mfl. (2022) studerer virkningene av endringer i den norske formuesskatten, og finner nettopp at effekten på sparing er svakere jo smalere skattegrunnlaget er (dårligere samsvar mellom skattepliktig og reell formue, som følge av verdsettelsesrabatter).

I denne artikkelen drøfter jeg mulige virkninger av formuesskatten i en norsk kontekst. I neste avsnitt gir jeg en oversikt over hvordan formuesskatten er utformet i Norge og en beskrivelse av regelendringer i perioden etter 2005. Med

[5] Alle skatter innebærer at skattekostnaden må bæres, enten av den som betaler skatten direkte (ved redusert konsum/etterspørsel, redusert sparing/investeringer eller redusert fritid), eller indirekte ved at det slår ut i økte priser eller lønninger. Begrunnelsen for skattlegging hviler nettopp på at denne overføringen fra privat til offentlig sektor samlet sett gir en samfunnsøkonomisk gevinst. Denne gevinsten må veies mot den «bivirkningen» at skatter påvirker skattyternes adferd (for eksempel at de jobber mindre, sparer mindre eller vrir investeringer på grunn av skatt), og at skattleggingen dermed medfører et effektivitetstap for samfunnet.

basis i norske registerdata beregner jeg hvordan regelendringene har påvirket fordelingen av formuesskatt i Norge, både etter størrelsen og sammensetningen av formuene. Videre gir jeg en kortfattet oversikt over empiriske studier på adferdsmessige effekter av formuesskatten og diskuterer hvilke lærdommer vi kan trekke av den.

DEN NORSKE FORMUESSKATTENS ANATOMI

Grunnlaget for formuesskatt er i utgangspunktet markedsverdien («omsetningsverdien») av alle eiendeler skattyter eier fratrukket gjeld ved utgangen av året, og dette utgjør da skattemessig nettoformue. [6] I Norge har man utstrakt bruk av tredjepartsrapportering av formuesverdier, som i stor grad sikrer at skattemyndighetene har tilgang på pålitelig informasjon om verdien av de enkelte eiendelene hver enkelt skattyter eier ved årsslutt. Likevel er verdsettelse en grunnleggende utfordring ved formuesskatten, ettersom det for en del eiendeler ikke eksisterer observerbare markedsverdier. For slike eiendeler benyttes ulike sjablongmessige verdsettelsesregler, som jeg redegjør for senere i dette avsnittet.

Det betales formuesskatt for den delen av nettoformuen som overstiger et bunnfradrag. De siste par tiårene har bunnfradraget blitt mer enn tidoblet, fra 151 000 kroner i 2005 til 1,7 millioner kroner i 2022 (3,4 millioner kroner for ektepar som lignes felles for formue), jf. Tabell 1. Skattesatsene er også endret over perioden, fra en høyeste skattesats på 1,1 prosent i perioden 2005–2013, via reduksjoner til 0,85 prosent i 2015, inntil den ble økt til 0,95 prosent i 2022. Fra 2022 ble det også innført en forhøyet sats på 1,1 prosent for formuer over 20 millioner. Med kun ett innslagspunkt kan formuesskatten for skattyter i defineres ved:

$$Formuesskatt_i = \begin{cases} 0, \; NSV_i \leq b \\ \tau^*[NSV_i - b], \; NSV_i > b \end{cases}$$

der τ er skattesatsen, b er bunnfradraget og NSV_i er personen eller ekteparets skattemessige nettoformue.

Et særtrekk ved formuesskatten er imidlertid at selve verdsettelsen av skattegrunnlaget er en helt sentral del av skattereglene, som også har vært endret mye over tid. Betalbar skatt bestemmes dermed ikke kun av satser og innslagspunkt, men også verdsettelsesreglene.

Den skattemessige nettoformuen kan defineres ved:

$$NSV_i = \sum_a \left(MV_{ia} * \gamma_{ia} * \left(1 - r_a\right)\right) - D_i$$

der MV_{ia} er faktisk markedsverdi, γ_{ia} er anslått («sjablongmessig») verdi som andel av faktisk markedsverdi (som kan variere fra skattyter til skattyter) og r_a er den formelle verdsettelsesrabatten som følger av regelverket (1-r_a = skattemessig verdi som andel av anslått verdi) for formueskomponent a (a=primærbolig, fritidseiendom, sekundærbolig, unoterte aksjer, noterte aksjer, bankinnskudd mv.) og D_i er gjeld.[7]

Tabell 1 viser hvordan verdsettelsesreglene er endret i perioden 2005–2022. De formelle verdsettelsesrabattene (r_a) omfatter fast eiendom, aksjer og driftsmidler. Andre eiendeler, som bankinnskudd, fordringer, obligasjoner, kjøretøy og innbo[8] (inkludert kunst mv.) verdsettes til markedsverdi. [9]

Fast eiendom ble tidligere verdsatt basert på historisk kostpris eller omsetningsverdi da bygget var nytt. Fritidseiendom verdsettes fortsatt etter denne metoden. Dette gjør at skatteverdiene ikke endrer seg i takt med utviklingen i markedsverdier, kun gjennom generelle prosentvise oppjusteringer enkelte år (som vist i Tabell 1). Fra 2010, da man gikk over til en ny verdsettelsesmetode for fast eiendom, skilles det mellom primærbolig (den boligen skattyter selv bor i) som verdsettes til 25 prosent, og sekundærboliger hvor skatteverdiene gradvis er økt fra 40 prosent til 95 prosent. Næringseiendom ble fra 2010 verdsatt til 40 prosent, men også her er skatteverdiene gradvis økt. Fra 2022 er det innført en forhøyet verdsettelse på 50 prosent for primærbolig for den verdien som overstiger 10 millioner kroner.

Rabatten for aksjer ble fjernet i 2008, men ble gjeninnført i 2017 for aksjer og driftsmidler i næring (inkludert næringseiendom). Samtidig ble det innført en redusert verdsettelse for den andel av gjeld som tilordnes rabatterte eiendeler, der gjelden fordeles proporsjonalt med eiendelenes andel av bruttoformue før verdsettelsesrabatter. Hensikten er å motvirke at skattyter kan oppnå en nettoreduksjon i skattepliktig formue ved å ta opp lån og plasserer midlene i rabatterte eiendeler.

6 Personer som er skattemessig bosatt i Norge, er som hovedregel skattepliktige for all formue uansett hvor i verden den er plassert.

7 Fra og med 2017 er det også redusert skattemessig verdsettelse av gjeld, som beskrevet under.

8 For innbo gjelder et skattefritt beløp på 100 000 kroner i samlet antatt salgsverdi (beregnet ved 10-40 prosent av forsikringssummen).

9 Pensjonsformue er unntatt formuesskatt.

Reglene om redusert verdsettelse gjelder imidlertid ikke den delen av gjelden som tilordnes egen bolig, som fortsatt kommer fullt til fradrag. Dette bidrar ytterligere til skattefavoriseringen av bolig, og medfører at for alle som har en gjeldsfinansiering av egen bolig på over 25 prosent, inngår boligen med negativ verdi i samlet nettoformue.

Figur 1 viser hvordan endringer i disse reglene for skattemessig verdsettelse medført ulike effektive skattesatser for ulike formuesobjekter. Disse effektive skattesatsene, gitt ved $(1\text{-}r_a)*\tau$, reflekterer kun verdsettelsesrabattene som følger direkte fra skattereglene, forutsatt at anslått verdi er lik markedsverdi ($\gamma_{ia}=1$).

Som det fremgår av Figur 1, skaper ulik verdsettelse store forskjeller i effektive skattesatser, både mellom ulike typer eiendeler og over tid. Dette gir insentiver til å vri investerings- og sparebeslutninger i retning av skattefavoriserte objekter. Disse forskjellene i effektiv skattesats mellom ulike objekter er i stor grad bestemt politisk gjennom vedtatte verdsettelsesrabatter.

Skattemessig forskjellsbehandling som følger av avvik mellom sjablongmessig verdsettelse og reell markedsverdi (γ_{ia}), fanges ikke opp i Figur 1. Dette er i større grad en iboende systemsvakhet ved formuesskatten, som følge av at enkelte eiendeler ikke har observerbare markedsverdier. Dette gjelder spesielt fast eiendom og unoterte aksjer, som beskrevet under.

I Norge er det lagt store ressurser inn på å utarbeide sjablonger for verdsettelse av fast eiendom. Boligeiendommer ble tidligere verdsatt til historisk kostpris, noe som over tid ga svært vilkårlig verdsettelse. Fra og med 2010 ble det

Tabell 1: *Skattesatser, bunnfradrag og verdsettelsesrabatter over tid.*

	Skattesatser og innslagspunkt				Skattemessig verdsettelse av ulike eiendeler PY: %-vis justering av foregående års skatteverdi MV: % av beregnet markedsverdi				
År	Skattesats trinn 1 (%)	Innslagspunkt trinn 1	Skattesats trinn 2 (%)	Innslagspunkt trinn 2	Primærbolig[1]	Fritidseiendom[1]	Sekundærbolig[1]	Næringseiendom	Aksjer
2005[2]	0.90	151 000	1.10	540 000	PY: 0	PY: 0	PY: 0	PY: 0	MV: 65
2006	0.90	200 000	1.10	540 000	PY: 25	PY: 25	PY: 25	PY: 25	MV: 80
2007	0.90	220 000	1.10	540 000	PY: 10	PY: 10	PY: 10	PY: 10	MV: 85
2008	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[3]	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	fjernet		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	MV: 80[4]	MV: 90[4]
2018	0.85	1 480 000			MV: 25	PY: 0	MV: 90	MV: 80[4]	MV: 80[4]
2019	0.85	1 500 000			MV: 25	PY: 0	MV: 90	MV: 75[4]	MV: 75[4]
2020	0.85	1 500 000			MV: 25	PY: 0	MV: 90	MV: 65[4]	MV: 65[4]
2021	0.85	1 500 000			MV: 25	PY: 0	MV: 90	MV: 55[4]	MV: 55[4]
2022	0.95	1 700 000	1.10	20 000 000	MV: 25/50[5]	PY: 25	MV: 95	MV: 75[4]	MV: 75[4]

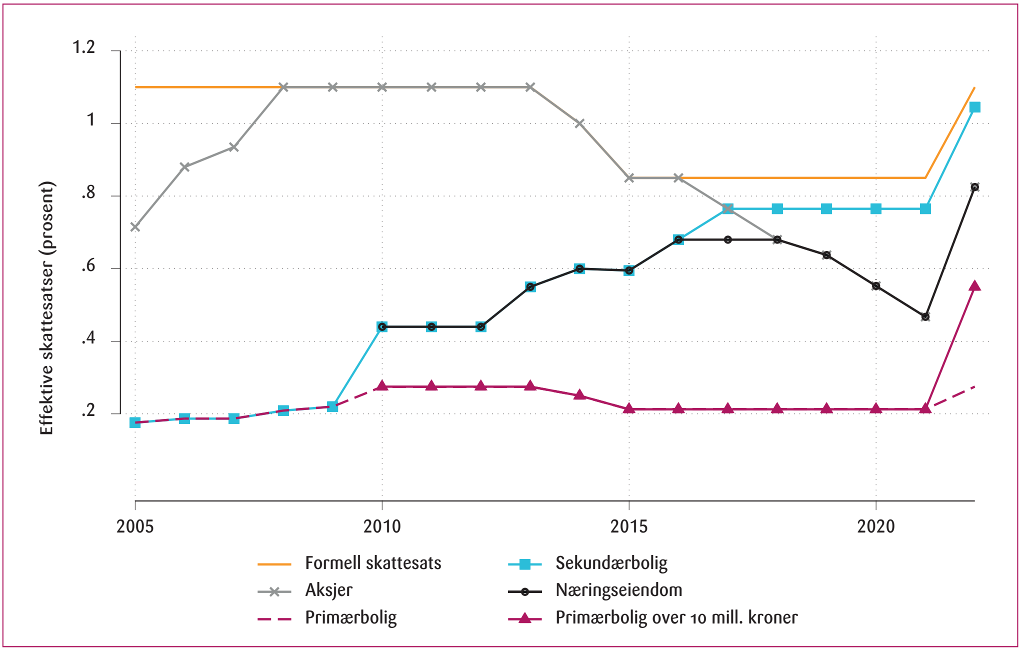
1 Skillet mellom bolig og fritidseiendom baseres ikke på faktisk bruk, men på hva eiendommen er regulert til eller egnet til. Primærbolig er den boligen skattyter bor i. Skattyter kan kun ha én primærbolig. Sekundærbolig er all annen boligeiendom unntatt primærbolig.

2 I 2005 delte ektepar ett bunnfradrag og et felles innslagspunkt i trinn 2 på 580 000 kroner. F.o.m. 2006 er innslagspunktene for ektepar (som lignes felles for formue) det dobbelte av hva tabellen viser.

3 I 2009 ble utleid næringseiendom verdsatt til 40 prosent av beregnet markedsverdi. For ikke-utleid næringseiendom ble skatteverdien oppjustert med 60 prosent.

4 Verdsettelsesrabattene gjelder for aksjer og driftsmidler mv. (inkl. næringseiendom) eid direkte av formuesskattepliktige, samt tilhørende gjeld.

5 Fra 2022 er det innført en forhøyet verdsettelse for primærboliger på 50 pst. for den delen av omsetningsverdien som overstiger 10 mill. kroner.



Figur 1: *Effektive marginalskatter på ulike eiendeler.*

De effektive skattesatsene på boligeiendom før 2010 er mine anslag basert på observert endring i median skatteverdi innen hver grunnkrets, jf. omtalen i neste avsnitt. Med det gamle systemet varierte verdsettelsen mye mellom boliger. I figuren har jeg vist gjennomsnittlige verdier på tvers av alle skattytere.

innført nye regler der boligeiendommer verdsettes basert på observerte omsetningsverdier per kvadratmeter på sammenlignbare boliger (etter kriterier som boligtype, areal, byggeår og beliggenhet). Næringseiendommer verdsettes basert på utleieverdi for den aktuelle eiendommen (dersom den er utleid) eller sammenlignbare eiendommer (dersom den ikke er utleid). Fritidsboliger verdsettes fremdeles basert på historisk kostpris, men Finansdepartementet vurderer nå nye metoder for verdsettelse av fritidsboliger basert på maskinlæring, der formålet er å få en skattemessig verdsettelse som ligger nærmere opp til markedsverdien.[10] På sikt kan det være aktuelt å ta denne metoden i bruk også for boliger. [11]

Den største utfordringen med verdsettelse gjelder eierandeler i selskaper som ikke omsettes. Unoterte aksjer verdsettes basert på aksjens andel av selskapets eiendeler (i utgangspunktet både fysiske og immaterielle[12]) fratrukket gjeld. Formuesverdien fanger dermed kun opp bokført verdi av identifiserbare eiendeler. Den reelle verdien selskapet har for eieren, i form av forventet fremtidig avkastning, vil ofte være høyere (differansen er det som kalles selskapets forretningsverdi eller «goodwill»).[13] Dette medfører at unoterte aksjer kan ha en stor implisitt verdsettelsesrabatt sammenlignet med børsnoterte aksjer (der forretningsverdi reflekteres i markedsverdien på aksjene).

HVEM BETALER FORMUESSKATT, OG PÅ HVILKEN FORMUE?

Jeg bruker registerdata fra Statistisk sentralbyrå for å beskrive formuesfordelingen og studere hvordan regelendringer i formuesskatten har påvirket fordelingen av formu-

[10] Et forslag til nytt verdsettingssystem er sendt på høring, men vil ta tid å implementere og vil tidligst kunne gjelde fra 2024, jf. Prop. 1 S (2021–2022) for Finansdepartementet.

[11] https://www.dn.no/innlegg/bolig/fritidsbolig/hytte/innlegg-maskinlaering-skal-gi-riktigere-boligverdier/2-1-1160603

[12] Patenter er ikke skattepliktig så lenge de er i opphavspersonens eie.

[13] Også ervervet forretningsverdi er ekskludert fra formuesverdien av unoterte aksjer, selv om denne bokføres i regnskapet.

esskatt mellom husholdninger[14] og mellom ulike formuesobjekter. Dataene omfatter formuesposter fra selvangivelsen for alle personlige skattytere for årene 2005–2018. Videre kobler jeg formuesposter fra selvangivelsen for unoterte aksjeselskaper til ultimat personlig eier ved hjelp av aksjonærregisteret.

Norge har gode formuesdata relativt til de fleste andre land. Ettersom de fleste land ikke skattlegger formue, har de heller ikke et krav om innrapportering av formuesverdier. Likevel har også de norske dataene begrensninger. De administrative dataene fra Skatteetaten undervurderer naturlig nok reell formue i tilfeller hvor eiendeler holdes skjult for skattemyndighetene. Sammenlignet med mange andre land, har Norge også mer utstrakt bruk av tredjepartsrapportering. Enkelte typer eiendeler, som utenlandsformuer, er fremdeles selvrapportert og kan dermed være underrapportert.

Som omtalt i forrige avsnitt, avviker skattemessige verdier fra markedsverdi av formue. I analysen korrigerer jeg formuesverdiene for de skattemessige verdsettelsesrabattene som følger av regelverket, jf. Tabell 1. Næringseiendom eies i all hovedsak av selskaper, og verdsettelsesrabattene for næringseiendom gir seg dermed utslag i lav verdsettelse av unoterte aksjer hos personer som eier disse selskapene. Jeg korrigerer for dette ved å oppjustere formuesverdien av selskapene, basert på informasjon om næringseiendommer hentet fra selskapenes selvangivelser, og kobler dette med ultimat personlig eier. Verdiene av boligeiendom med det gamle verdsettelsessystemet (før 2010) er beregnet basert på observert endring i median skatteverdi innen hver grunnkrets fra 2009 til 2010, samt årlige justering av ligningsverdiene i årene før, der antagelsen er at markedsverdiene følger utviklingen i boligprisindeksen. Verdiene av fritidseiendom er oppjustert basert på at verdsettelsen er 30 prosent, noe som er en øvre grense som følger av sikkerhetsventilen. Det vil si at skattyter kan klage dersom skatteverdien overstiger 30 prosent av dokumentert markedsverdi. For de fleste fritidseiendommer er verdsettelsen trolig langt lavere, som vil si at jeg undervurderer reelle verdier av fritidseiendom.

Justeringen for verdsettelsesrabatter gir et bedre mål på reelle formuesverdier, men det vil fortsatt være avvik for eiendeler som er sjablongmessig verdsatt eller unntatt formuesskatt. Den største begrensningen gjelder verdien av unoterte aksjer, hvor dataene kun omfatter bokførte verdier som ofte er langt lavere enn markedsverdi.

Jeg starter her med å vise nivå og sammensetning av formue blant norske husholdninger, både skattemessig verdier slik de inngår i personenes selvangivelser og verdier justert for skattemessige verdsettelsesrabatter («anslått markedsverdi»).

Hvordan er formuene fordelt?

Figur 2 viser ligningsverdi (LV) og anslått markedsverdi (MV) av ulike formueskomponenter, både for hele befolkningen og for ulike grupper husholdninger i formuesfordelingen.

Norske husholdninger hadde i 2018 en samlet skattepliktig bruttoformue på om lag 5400 mrd. kroner og en samlet gjeld på om lag 3500 mrd. kroner, som vist i Figur 2.[15] Markedsverdien av formuen er imidlertid langt høyere. Når verdsettelsesrabattene i formuesskatten korrigeres ut, utgjør bruttoformuen 10 800 mrd. kroner og sum positive nettoformuer 7700 mrd. kroner.

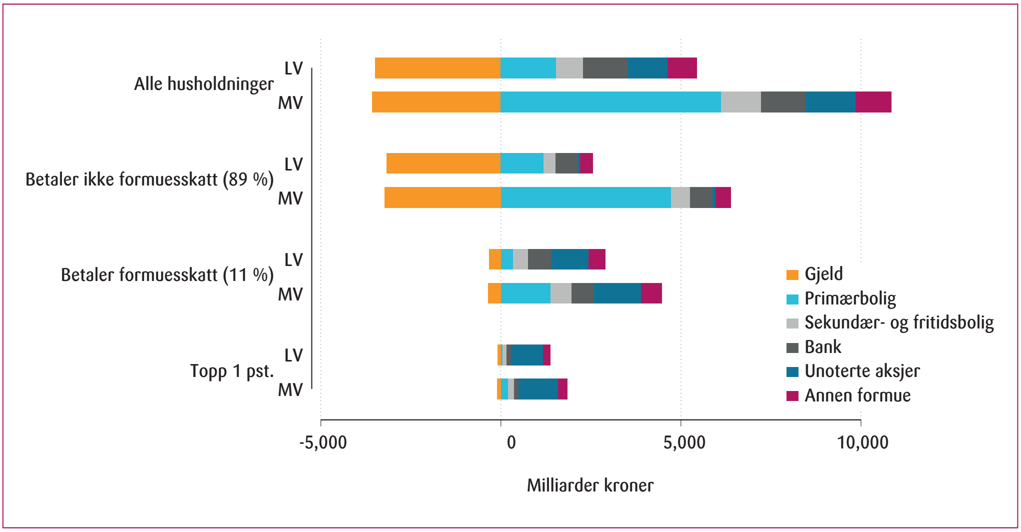
Skattepliktig formue er svært skjevfordelt. De om lag 11 prosent av husholdningene som har skattepliktig nettoformue over bunnfradraget (1,48 millioner kroner i 2018) og som dermed betaler formuesskatt, eier over halvparten av skattepliktig bruttoformue (2900 mrd. kroner). Disse har gjennomgående langt lavere gjeld enn andre husholdninger, og eier over 75 prosent av samlet positiv skattepliktig nettoformue (2600 mrd. kroner). Gjennomsnittlig skattepliktig nettoformue for denne gruppen er 6,9 millioner kroner. De 1 prosent med høyest skattepliktig nettoformue (over 10,9 millioner kroner) eier 25 prosent av samlet skattepliktig bruttoformue og 38 prosent av positiv skattepliktig nettoformue (1300 mrd. kroner samlet, 38 millioner kroner i gjennomsnitt per husholdning).

Hva består formuene av?

Boligformue er den klart største formueskomponenten. Primærboliger (bolig man selv bor i), sekundærboliger og fritidsboliger utgjør til sammen 2/3 av samlet bruttoformue målt i anslått markedsverdi, jf. figur2. Boligeiendom utgjør imidlertid en langt mindre andel av grunnlaget for formuesskatt. Som følge av den skattemessige favoriseringen (særlig primærbolig, som har en verdsettelsesrabatt på 75 prosent), utgjør boligformuen kun om lag 40 prosent av

[14] Husholdning er her definert som ektefeller, da disse blir lignet sammen for formuesskatteformål, slik at husholdningene da består av enkeltpersoner og par.

[15] Som følge av verdsettelsesrabatter kombinert med at gjeld tilordnet egen bolig inngår med full verdi, har en stor andel av husholdningene negativ skattepliktig nettoformue. Summen av alle *positive* skattepliktige nettoformuer utgjorde 3400 mrd. kroner (ikke vist i figuren).



Figur 2: *Sammensetning av formue i 2018, ligningsverdi (LV) og markedsverdi (MV), mrd. kroner.*

Figuren viser ligningsverdi (LV) og anslått markedsverdi (MV) av formueskomponenter og gjeld for henholdsvis alle husholdninger, husholdninger som ikke betaler og betaler formuesskatt og de 1 prosent med høyest skattepliktig nettoformue i 2018. Annen formue omfatter blant annet aksjer registrert i VPS (hovedsakelig børsnoterte aksjer), driftsmidler og næringseiendom eid direkte av personer, samt innbo og løsøre.

samlet skattepliktig bruttoformue.[16] Videre gjør lav verdsettelse kombinert med at lån til egen bolig trekkes fra med full verdi i beregningen av skattepliktig nettoformue, at de fleste boligeiere ikke betaler formuesskatt.[17] De som betaler formuesskatt, har en langt mindre andel av formuen plassert i boligeiendom.

Unoterte aksjer eies nesten utelukkende av husholdninger i formuesskatteposisjon. Faktisk eier denne gruppen 94 prosent av husholdningenes samlede ligningsverdi av unoterte aksjer.[18] Unoterte aksjer utgjør mer enn 1/3 av deres skattemessige bruttoformue. Ser vi på de aller mest formuende (topp 1 prosent målt i skattemessig nettoformue), utgjør unoterte aksjer hele 2/3 av bruttoformuen.

[16] Andre viktige formueskomponenter er bankinnskudd og unoterte aksjer, som hver utgjør om lag 20 prosent av samlet skattepliktig bruttoformue. Andre verdipapirer (børsnoterte aksjer, fondsandeler og aksjesparekonto) utgjør kun om lag 5 prosent.

[17] En person uten annen formue kan i 2018 ha en gjeldfri bolig til en markedsverdi av 6 millioner kroner uten å betale formuesskatt. Er boligen 10 prosent gjeldsfinansiert, øker grensen til 10 millioner kroner. For ektepar er grensene det dobbelte. I 2018 var det kun 67 000 husholdninger i formuesskatteposisjon som hadde brutto ligningsverdi av primærbolig over bunnfradraget (før boliglån er trukket fra).

[18] Det er verdt å påpeke at ligningsverdien av unoterte aksjer er en nettoverdi basert på selskapets bokførte eiendeler fratrukket gjeld. Selskaper der gjeld overstiger bokførte verdier, vil inngå med null verdi i eiernes selvangivelse.

Det er derfor interessant å se på hvilke underliggende verdier som befinner seg i disse unoterte selskapene. Når jeg ser på bokførte formuesverdier fra selvangivelsen[19] til alle unoterte aksjeselskaper som er direkte eid av personer som betaler formuesskatt, ser dette i stor grad ut til å være holdingselskaper. Av en samlet brutto formuesverdi på om lag 1400 mrd. kroner som kan tilordnes norske, private eiere i formuesskatteposisjon, utgjør varebeholdning og driftsmidler kun 3 prosent.[20] Fordringer og bankinnskudd utgjør 23 prosent, fast eiendom 11 prosent, mens aksjer og obligasjoner utgjør 63 prosent (850 mrd. kroner). Dette samsvarer med resultatene til Alstadsæter mfl. (2014, 2016) og Aaberge mfl. (2021) som viser at innføringen av aksjonærmodellen og fritaksmodellen rundt 2006 førte til stor økning i bruk av holdingselskaper og tilbakeholdte overskudd.

For å få et fullstendig bilde av hvilke underliggende verdier det betales formuesskatt på, må man se hva som igjen ligger bak aksjeverdiene gjennom flere selskapsledd. Selv om aksjonærregisteret gjør det mulig å koble eierskap i flere ledd, og dermed tilordne underliggende verdier i selska-

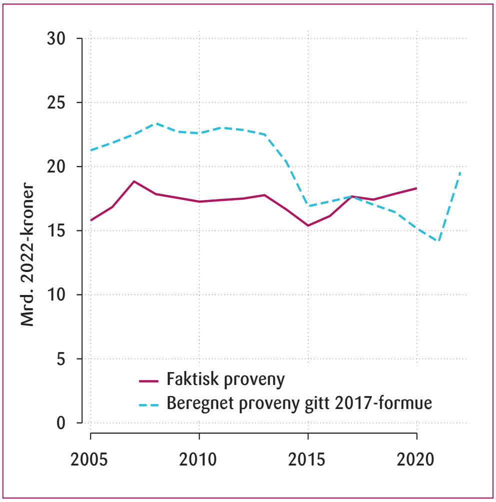
[19] Bokførte formuesverdier rapporteres i selskapenes selvangivelser og det er disse verdiene som danner grunnlag for eiernes formuesskatt.

[20] Netto formuesverdi (etter fradrag fra gjeld), som er det som inngår i eiernes selvangivelser, utgjør om lag 1100 mrd. kroner. Samlet gjeld utgjør om lag 300 mrd. kroner.

pene til ultimat personlig eier, er det ikke mulig å si hvor stor del av den samlede netto formuesverdien som utgjøres av ulike eiendeler i bakenforliggende selskaper. Dette skyldes at selskapene i en eierkjede kan ha en balanse som delvis består av lån til, fordringer på og eierandeler i andre selskaper i eierkjeden. Det er dermed ikke mulig å identifisere hver enkelt eiendels bidrag til nettoverdien i selskap høyere opp i eierkjeden. Det man derimot kan gjøre er å summere formuesverdien av alle ikke-finansielle eiendeler og bankinnskudd. Når jeg summerer disse verdiene gjennom alle underliggende selskaper, og tilordner verdiene til ultimate, personlige eiere i formuesskatteposisjon basert på eierandeler i hvert ledd, finner jeg at disse eiendelene til sammen utgjør om lag 850 mrd. kroner. Av dette utgjør fast eiendom 61 prosent, bankinnskudd 17 prosent og varebeholdning og driftsmidler 22 prosent (hvorav immaterielle/ikke-avskrivbare driftsmidler kun utgjør 1 prosent).

Hvordan har regelverksendringene påvirket samlet formuesskatt?

Regelendringene i perioden 2005–2022 har ført til store endringer, både i hvem som betaler formuesskatt og hvilke eiendeler det betales formueskatt på. Men på tross av at det er gjennomført store omlegginger av formuesskatten, har samlet proveny (målt i dagens kroneverdi) ligget relativt stabilt på 15–18 mrd. kroner i perioden, som vist i Figur 3. Utviklingen i provenyet avhenger både av skattereglene (bunnfradrag, sats og verdsettelse) og endringer i faktisk formue. For å rendyrke effekten av regelverksendringene, har jeg beregnet formuesskatt med årlige regelverk gitt at formuen holdes uendret. Denne analysen bygger på et modellapparat vi har utviklet (Bjørneby mfl., 2020; Alstadsæter mfl., 2022), som har likheter med mikrosimuleringsmodellen Finansdepartementet bruker for å anslå virkninger av årlige endringer i skattesystemet.[21] Vår modell gjør det imidlertid mulig å beregne effekter av regelverksendringer over flere år og fanger også opp flere detaljer i verdsettelsesreglene i formuesskatten.



Figur 3: *Faktisk formuesskatteproveny (2005–2020) sammenlignet med beregnet proveny gitt 2017-formue med regelverkene for årene 2005–2022. Mrd. 2022-kroner.*

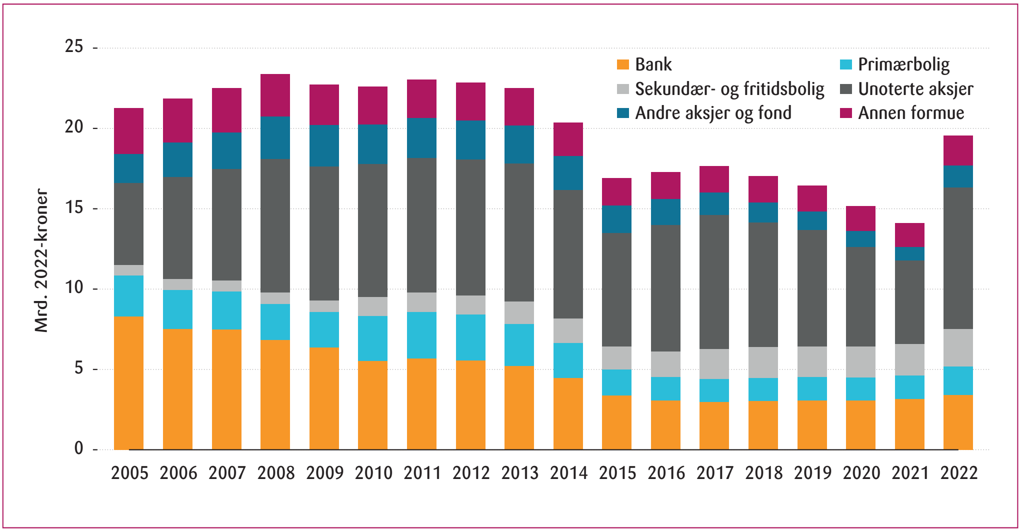
Den røde linjen viser faktisk proveny målt i 2022-kroner (kilde: våre data 2005–2018, Statistisk sentralbyrå tabell 08564 for 2019–2020). Den blå linjen viser beregnet proveny gitt 2017-formue med regelverket for alle år 2005–2022, fremført til 2022.

Figur 3 viser faktisk proveny fra formuesskatten 2005–2020 sammenlignet med beregnet proveny med skattereglene for 2005–2022 gitt 2017-formue. Ved å sammenligne utviklingen i regelverksdrevet endring i proveny (gitt 2017-formue) med utviklingen i faktisk proveny, ser man at det er gitt netto lettelser i formuesskatten, særlig i årene 2014 og 2015 da skattesatsen ble redusert. Disse lettelsene ble imidlertid i stor grad oppveiet av at formuene økte, noe som dempet reduksjonen i faktisk proveny. I perioden 2017–2021 ble det også gitt netto lettelser. Tall fra Statistisk sentralbyrås skattestatistikk viser imidlertid at provenyet økte noe både i 2019 og 2020. I 2022 er det gjennomført en betydelig innstramming i formuesskatten, som oppveier 2/3 av lettelsene som er gitt i årene siden 2013.

Hvilke objekter betales det formuesskatt på?

Endringer i både satsstruktur og verdsettelsesregler har betydning for hvordan formuesskatten fordeler seg på ulike eiendeler. Figur 4 fordeler beregnet formuesskatt med årlige regelverk (gitt 2017-formue, som i Figur 3) på ulike eiendeler. Det vil si at utviklingen i figuren kun drives av

[21] Både formuesdataene og beløpsgrenser for alle år er justert til 2022-kroner. Hensikten med justeringen er å kunne sammenligne hva skatten ville blitt med ulike regelverk fremført til 2022. I vår modell har vi brukt årlige endringer i Folketrygdens grunnbeløp for 2005–2021 som justeringsfaktor (for 2022 har vi lagt til grunn Finansdepartementets anslåtte lønnsvekst på 3 prosent). Finansdepartementet har i de årlige budsjettene delvis brukt anslått lønnsvekst og delvis brukt gjennomsnittlig anslått formuesvekst. Beregnet proveny (og årlige endringer) avviker noe fra Finansdepartementets anslag i de årlige budsjettene, både som følge av ulike justeringsfaktorer og at datagrunnlaget er fra ulike år. Eksempelvis er beregnet provenyøkning som følge av vedtatte regelverksendringer for 2022 om lag 80 prosent av Finansdepartementets anslag: https://www.regjeringen.no/no/statsbudsjett/2022/tilleggsnummer/tilleggsnummer-til-statsbudsjettet-2022-skatter-og-avgifter/tilleggsnummer-til-statsbudsjettet-2022-provenyvirkninger-av-forslaget-til-skatte-og-avgiftsendringer/.



Figur 4: *Formuesskatt fordelt på eiendeler. Beregnet proveny med regelverkene for årene 2005–2022 (gitt 2017-formue). Mrd. 2022-kroner.*

regelverksendringer, ikke av underliggende endringer i formue.

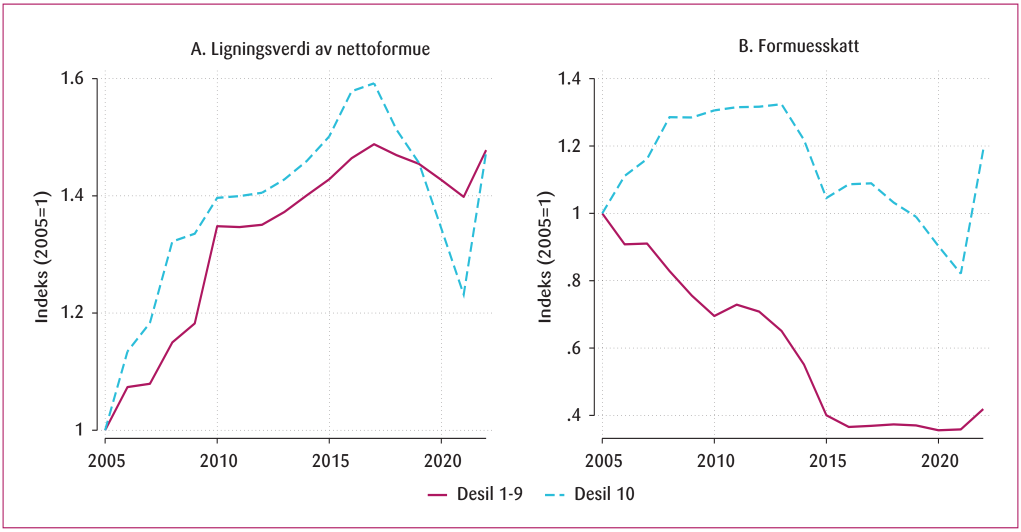
For å kunne anslå hvor mye formuesskatt som betales på ulike eiendeler, må man gjøre noen antagelser. Dette skyldes at formuesskatten betales på den delen av samlet formue fratrukket gjeld som overstiger et bunnfradrag. Ettersom dataene ikke gjør det mulig å skille mellom boliglån og annen gjeld, har jeg i Figur 4 fordelt formuesskatten for hver enkelt husholdning proporsjonalt over eiendelene husholdningen eier (andel av husholdningens skattemessige bruttoformue). Det innebærer at bunnfradraget og gjelden også fordeles proporsjonalt på eiendelene.

De store økningene i bunnfradraget har gjort at andelen av formuesskatten som kan tilordnes primærbolig og bankinnskudd er kraftig redusert (fra over halvparten til om lag ¼). Dette følger av at færre med middels formuer, som typisk har en relativt stor del av formuen i disse eiendelene, betaler formuesskatt. Formuesskatten som kan tilordnes unoterte aksjer økte først, både som følge av at aksjerabatten ble fjernet (2005–2008) og som følge av gradvis økt verdsettelse av næringseiendom (2010–2016), men falt med gjeninnføring og senere økninger av rabatt på aksjer og driftsmidler (2017–2021). Med 2021-regler utgjorde formuesskatten som kan tilordnes unoterte aksjer 37 prosent av samlet proveny (5,2 mrd. kroner). Med 2022-regler er aksjerabatten redusert, samtidig som det er innført en forhøyet formuesskattesats på de høyeste formuene (som i stor grad er plassert i unoterte aksjer, som vist i Figur 2 over). Dette gjør at formuesskatten på unoterte aksjer har økt kraftig, til anslagsvis 8,8 mrd. kroner eller 45 prosent av total formuesskatt i 2022.

Hvordan virker regelverksendringene på ulike grupper i formuesfordelingen?

Som drøftet i de foregående avsnittene, har endringene i formuesskattereglene siden 2005 hatt stor betydning både for hvem som betaler formuesskatt og hvilke eiendeler det betales formuesskatt på. I 2005 betalte om lag 30 prosent av husholdningene formuesskatt. Siden den gang er bunnfradraget mer enn ti-doblet, og med 2022-regler vil anslagsvis 11 prosent av husholdningene betale formuesskatt (anslag basert på 2017-formue fremført til 2022). Gjennomsnittlig formuesskatt for alle formuesskatteytere anslås til 52 000 kroner i 2022. Flertallet av de som betaler formuesskatt, betaler imidlertid langt mindre enn dette. Halvparten betaler under 14 000 kroner. Av samlet formuesskatt betales 2/3 av topp 1 prosent av formuesfordelingen (husholdninger med skattepliktig nettoformue over 12,4 millioner kroner). Disse anslås å betale 390 000 kroner i gjennomsnitt med 2022-regler.

Jeg vil nå se nærmere på hvordan regelendringene har slått ut i ulike deler av formuesfordelingen. For å rendyrke virkninger av endringer i skattereglene, tar jeg igjen utgangs-



Figur 5: *Skattepliktig formue og formuesskatt. Regeldrevet endring relativt til 2005 (gitt 2017-formue) for husholdninger som ville betalt formuesskatt med 2005-regler.*

Figuren viser utviklingen, relativt til 2005-regler, for alle som ville betalt formuesskatt med 2005-regler fordelt etter størrelsen på formuesskatten. Desil 10 prosent ville med 2005-regler i gjennomsnitt betalt 131 000 kroner. Desil 1-9 ville i gjennomsnitt betalt om lag 10 000 kroner i formuesskatt med 2005-regler.

punkt i en gitt formue (2017) og beregner skattepliktig formue og formuesskatt med regelverkene for alle år.

Figur 5 viser regeldrevet utvikling i skattepliktig formue (panel A) og formuesskatt (panel B), relativt til 2005-regler. Figuren omfatter alle som ville betalt formuesskatt med 2005-regler (om lag 950 000 husholdninger) fordelt på to grupper, de 10 prosent med høyest formuesskatt (desil 10) og de resterende 90 prosent blant formuesskatteyterne (desil 1-9).

For desil 10 økte skattepliktig formue med 60 prosent fra 2005 til 2017, som følge økt skattemessig verdsettelse av aksjer (2005–2008) og fast eiendom (2013–2017). Satsreduksjonene i 2014 og 2015 veiet imidlertid opp for innstrammingene i verdsettelsesreglene, slik at formuesskatten med 2015-regler for denne gruppen var tilbake på om lag samme nivå som med 2005-regler. Med gjeninnføring av rabatt på aksjer og driftsmidler fra 2017, og gradvise økninger av denne fra 10 prosent til 45 prosent frem til 2021 ble skattepliktig formue og formuesskatt for denne gruppen kraftig redusert, men dette er delvis reversert av at rabatten ble kuttet til 25 prosent i 2022. Med 2022-regler får denne gruppen også en betydelig innstramming i formuesskatten som følge av økt skattesats.

For desil 1-9 blant formuesskatteyterne ser verdsettelsesrabatten på aksjer ut til å være av mindre betydning. Likevel er denne gruppens skattepliktig nettoformue økt med nærmere 50 prosent som følge av regelverksendringer fra 2005 og frem til i dag. En stor del av økningen skyldes trolig økte ligningsverdier av fast eiendom, herunder de nye ligningsverdiene på bolig fra 2010. Økte bunnfradrag har imidlertid mer enn kompensert for økte ligningsverdier, slik at formuesskatten samlet sett er redusert med 60 prosent for denne gruppen (fra i gjennomsnitt 10 000 kroner med 2005-regler til om lag 4000 kroner med 2022-regler). 70 prosent av denne gruppen betaler ikke formuesskatt med 2022-regler.

HVA VET VI OM EFFEKTENE AV FORMUESSKATT?

De samfunnsøkonomiske kostnadene ved formuesskatt avhenger av hvordan skatten påvirker adferd. Eksisterende empirisk litteratur gir langt fra noe entydig svar på dette. En måte å måle tilpasninger på, er ved å estimere såkalte

Tabell 2: *Empiriske studier på effekter av formuesskatt og estimerte elastisiteter.*

Studie	Land	Metode	Elastisitet (pst.)
Ring (2020)	Norge	Bunching	0,05
Seim (2017)	Sverige	Bunching	0,2
Jakobsen et al. (2020)	Danmark	Bunching	0,3
Brüllhart et al. (2020)	Sveits	Bunching	0,8
Londoño-Velez and Àvila-mahecha (2020)	Colombia	Bunching	2
Alstadsæter et al. (2022)	Norge	Diff-in-diff	5–7
Agrawal et al. (2020)	Spania	Diff-in-diff	6–9
Jakobsen et al. (2019)	Danmark	Diff-in-diff	6–11
Zoutman (2018)	Nederland	Diff-in-diff	12–14
Durán-Cabré et al. (2019)	Spania	Diff-in-diff	15–32
Brüllhart et al. (2020)	Sveits	Diff-in-diff	18–43

elastisiteter, det vil si prosentvis endring i formue ved en reduksjon i formuesskattesatsen på 1 prosentpoeng.[22]

Disse estimerte elastisitetene fanger opp en rekke ulike responsmarginer. Formuesskatten kan føre til alt fra endring i nivå på sparing (Alstadsæter mfl., 2022) , endret portefølje ved at sparesammensetningen vris mot skattefavoriserte eiendeler (Bjørneby mfl., 2020), omplassering av eiendeler gjennom å eie via selskap (Henrekson og Du Rietz, 2014; Durán-Cabré mfl., 2019), oppsplitting av formue på flere personer (Bastani og Waldenström, 2020), feilrapportering (Seim, 2017; Brüllhart mfl., 2020; Londoño-Velez og Àvila-Mahecha, 2020), migrasjon (Agrawal mfl., 2020; Brüllhart mfl., 2020) eller skjuling av formuer utenlands (Alstadsæter mfl., 2019; Londoño-Velez og Àvila-Mahecha, 2020). Noen av disse responsmarginene er delvis substitutter. Dersom man lett kan omgå skatten ved å skjule formuen eller endre porteføljesammensetningen, er det mindre insentiv til å redusere nivået på total sparing.

I hvilken grad skattyter kan redusere formuesskatten uten å redusere faktisk sparing avhenger av unntak og særordninger, hvordan skatteplikten defineres (herunder exit-skatt ved flytting ut av landet) og håndhevelsen av regelverket. Elastisitetene er derfor ikke strukturelle parametere, men avhenger av definisjonen av skattegrunnlaget, og styres til en viss grad av beslutningstakerne, som fremhevet av Slemrod og Kopczuk (2002). En høy elastisitet er dermed ikke nødvendigvis et argument for å at skatten bør reduseres, men kan også tilsi at man bør se på muligheter for å utvide skattegrunnlaget og forbedre håndhevelsen.

Studier som analyserer hvordan rapportert skattepliktig formue påvirkes av formuesskatt, finner elastisiteter som spriker fra nær null til over 40 prosent, som vist i Tabell 2. Den store spredningen skyldes dels at studiene bruker ulike metoder og studerer ulike grupper og tidshorisonter, og dels at estimatene kun gjelder de som faktisk er berørt av skatten. Men det skyldes også at de studerer effekter estimert innenfor helt ulike skatteregimer og andre institusjonelle forhold.

Såkalte bunching-estimater studerer opphopning av personer som rapporterer formue like under innslagspunktet for skatten. Det er vanlig å tolke slike effektestimater som et resultat av skatteomgåelse og -unndragelse heller enn realeffekter på sparing (det er vanskelig tilpasse faktisk formue rett under innslagspunktet). Effektestimatene er vanligvis større i land med stor grad av selv-rapportering av formue, noe som muliggjør under-rapportering.

Studier som baserer identifikasjonen på endringer i skattereglene gjennom en diff-in-diff (forskjell-i-forskjeller) tilnærming, finner generelt større effekter. Men også her spriker resultatene. Dette understreker at effektene avhenger av utforming og håndhevelse av skattereglene (skattegrunn-

[22] Mer presist angir elastisiteten prosentvis endring i formue som følge av en 1 prosents økning i etter-skatt raten ($1-\tau$). Men ettersom formuesskattesatser typisk ligger rundt 1 prosent, er en 1 prosent økning i etter-skatt-raten omtrent det samme som en 1 prosentpoengs reduksjon i skattesatsen.

lag, tredjepartsrapportering, informasjonsutveksling mellom land, utflyttingsregler, variasjon av skatteplikt innad i landet). Elastisiteten er høyest i Spania og Sveits, hvor det er lite eller ingen tredjepartsrapportering.

De fleste studiene på dette området ser på hvordan skattegrunnlaget (rapportert skattepliktig formue) endres som følge av formuesskatt. Dette fanger da opp effekten av alle tilpasninger, herunder omplasseringseffekter som følger av at skattereglene favoriserer enkelte eiendeler. Et unntak er Alstadsæter mfl. (2022) som studerer effekter av den norske formuesskatten på aktiv sparing og finner at sparingen går ned som følge av formuesskatten, men at effekten er svakere ved høyere rabatter i skattemessig verdsettelse (som gjør det lettere å omgå skatten ved å omplassere formuen). Dette er konsistent med at skattepliktig formue er mer elastisk enn reell sparing.

Resultatene fra eksisterende studier av formuesskatten indikerer at effekten på rapportert formue kan være betydelig, men effekten ser i stor grad ut til å skyldes skatteomgåelser og ikke reelle endringer i akkumulering av formue. Selv fra et teoretisk perspektiv er det ikke opplagt at en skatt på formue fører til redusert sparing. Skatten reduserer etter-skatt avkastningen på sparing. Substitusjonseffekten trekker dermed i retning av redusert sparing fordi det blir lønnsomt å konsumere mer (eller jobbe mindre) i dag sammenlignet med å spare til fremtidig konsum. Men skatten gjør det også nødvendig å spare mer (før skatt) for å opprettholde et gitt fremtidig konsum. Inntektseffekten trekker dermed i retning av økt sparing. Ring (2020) studerer endringer i den norske formuesverdsettelsen av bolig, og finner at formuesskatten har en positiv effekt på sparing. Dette forklares med at inntektseffekten dominerer.

De store forskjellene i effektestimater mellom land reflekterer at estimatene springer ut av helt ulike skatteregimer. For eksempel skyldes de høye elastisitetene målt i Spania i Durán-Cabré mfl. (2019) i stor grad omplassering av formue ettersom selskapsformue var unntatt formuesskatt. Elastisitetene i Agrawal mfl. (2020) måler kun effekten av migrasjon innad i landet (hovedsakelig til Madrid, som hadde satt satsen til null). Brüllhart mfl. (2020) har blant de høyeste estimerte elastisitetene på formuesskattegrunnlaget, men forfatterne tilskriver halvparten av den estimerte effekten til at skatteyterne underrapporterer sin formue. Dette er mulig siden Sveits i liten grad har tredjeparts rapporterering til skattemyndighetene. Videre skyldes èn tredjedel av effekten migrasjon innad i landet, det vil si at skatteyter flyttet til andre deler av Sveits med lavere formuesskatt. Når man korrigerer for disse responsmarginene, som kan avhjelpes ved å forbedre utforming av skatten, reduseres effektestimatet til 7, noe som er mer på linje med andre studier.

I en rapport fra den britiske kommisjonen som har vurdert formuesskatt, oppsummeres erfaringene fra andre land. De konkluder med at en godt utformet formuesskatt, med et bredt skattegrunnlag og utstrakt bruk av tredjepartsrapportering, kan oppnå en elastisitet på i størrelsesorden 7–17, avhengig av hvor mye internasjonal migrasjon skatten medfører (Advani og Tarrant, 2021). Dette samsvarer med estimater som er lagt til grunn i et forslag til innføring av formuesskatt i USA (Saez og Zucman, 2019b).

Dette er i samme størrelsesorden som tidligere forskningslitteratur har funnet for elastisiteter av skatt på kapitalinntekt, der konsensusestimater ligger i området 0,1–0,4 (se blant andre Kleven og Schultz, 2014).[23] For å kunne sammenligne elastisiteter av formuesskatt med disse estimatene, må man korrigere for at formue og inntekt er to helt ulike skattegrunnlag. Ved en avkastningsrate på 5 prosent, vil 1 prosent formuesskatt være sammenlignbart med 20 prosent skatt på avkastning. Det vil si at effekten av 1 prosentpoengs endring i formuesskatt må forventes å være 20 ganger høyere enn effekten av 1 prosentpoeng endring i skatt på avkastning. En formuesskatte-elastisitet på 7 er da sammenlignbar med en elastisitet av skatt på kapitalinntekt på 0,35. Det er altså, på bakgrunn av eksisterende forskningslitteratur, ikke grunnlag for å konkludere med at formuesskatten har større uheldige vridningseffekter enn en skatt på kapitalinntekt.

AVSLUTTENDE KOMMENTARER

Formuesskatten har fått en fornyet interesse internasjonalt, og debatteres i flere land som et mulig virkemiddel for å dempe økende ulikhet. Likevel er det per i dag få land som har gått til det skrittet å (gjen)innføre formuesskatt. Et hovedargument i debatten mot formuesskatt er at skatten i praksis lett blir uthulet av unntak og særordninger og at verdien av enkelte formuesobjekter er vanskelig å identifisere. Videre pekes det på utfordringer med å håndheve skatten gitt at eiendeler kan skjules, og at skatten kan føre til utflytting.

[23] Virkningen av skatt på kapitalinntekter vil, i likhet med virkninger av formuesskatt, avhenge av den spesifikke utformingen.

Basert på eksisterende empirisk forskning, er det vanskelig å gi ett svar på hvordan formuesskatt påvirker skattepliktig formue og sparing. Resultatene fra flere studier tyder imidlertid på at formuesskatt har begrenset effekt på sparing, men at skatten kan ha store effekter på porteføljesammensetning, omplassering, flytting innad i land og underrapportering for å spare skatt dersom skattereglene åpner for det. Dersom mulighetene for slike tilpasninger strupes, er det grunn til å anta at effekten på reell sparing vil være høyere enn i et system hvor slike tilpasningsmuligheter er utbredt. Likevel vil en uniform formuesskatt trolig ha bedre effektivitetsvirkninger enn en skatt som forskjellsbehandler ulike eiendeler. Et forsøk på å favorisere «produktiv» kapital svekker fordelingsegenskapene og åpner for tilpasninger, og det er i praksis krevende å trekke et skille mellom «produktiv» og «uproduktiv» kapital.[24] Et bunnfradrag som er tilstrekkelig høyt til å skjerme «normal» sparing over livsløpet vil imidlertid kunne dempe mulige uheldige vridninger i sparebeslutningen.

I en vurdering av formuesskattens rolle er det viktig å skille iboende svakheter ved skatt på formue fra skjevheter som kan forbedres. Erfaringene fra den norske formuesskatten viser at riktig og lik verdsettelse er krevende. Dette gjelder spesielt unoterte aksjeselskaper. Samtidig har man i Norge etabler sjablongsystemer for verdsettelse av fast eiendom som når det først er på plass, krever relativt lave administrative kostnader (sammenlignet med å skulle verdsette den enkelte eiendom årlig). Utstrakt bruk av tredjepartsrapportering av formuesverdier er også viktig for å sikre god håndhevelse og redusere kostnadene ved administrasjon og etterlevelse. Videre kan et høyt bunnfradrag bidra til å dempe eventuelle likviditetsutfordringer og presset for særordninger og unntak. Den betydelige økningen i bunnfradraget i den norske formuesskatten, ser ut til å ha løst mye av likvidtetsutfordringene for personer med verdifull bolig og lav inntekt, noe som fikk mye fokus i den offentlige debatten da bunnfradraget var lavere.

Virkninger av formuesskatt må vurderes opp mot hvordan skattesystemet som en helhet oppnår de målene man har for omfordeling, skatteinngang og effektivitet. I flere land er det et økende press for å reformere kapitalbeskatningen for å sikre fordelingsegenskapene til skattesystemet. OECD (2018) anbefaler i utgangspunktet å styrke omfordelingen ved å innføre progressiv skatt på kapitalinntekt og vurdere å skattlegge urealiserte kapitalgevinster løpende. Men de peker samtidig på at formuesskatten kan forsvares i land som Norge, som ikke har arveavgift og som har en lav og flat skatt på kapitalinntekt. Det er en pågående debatt, både blant økonomer og beslutningstakere, om hvorvidt det er tilstrekkelig å endre beskatningen av kapitalinntekter eller om en løpende skatt på formue kan være et hensiktsmessig supplement til skatt på realiserte kapitalinntekter.

[24] Skatteutvalgets frarådet på denne bakgrunn å innføre en rabatt på «arbeidende kapital», jf. NOU 2014:13 avsnitt 12.7. Da rabatten ble innført, i 2017, ble alle eiendeler i unoterte foretak i praksis gjenstand for rabatt (i tillegg til noterte aksjer, driftsmidler mv.). Finansdepartementet begrunnet dette med at en avgrensning til «eiendeler som på en eller annen måte har virket i næringen» ville være «vanskelig å lovregulere og ville også være vanskelig å praktisere og kontrollere for Skatteetaten», jf. Prop. 1 LS (2016–2017). Finansdepartementet pekte samtidig på at rabatten «vil kunne gi insentiver til å legge private eiendeler i ikke-børsnoterte selskap for å redusere formuesskatten».

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IV Paper 4: Limits to third-party reporting: Evidence from a randomized field experiment in Norway

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Short communication

Limits to third-party reporting: Evidence from a randomized field experiment in Norway ☆

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ABSTRACT

Third-party reporting and employers' tax withholding are powerful compliance mechanisms, as long as the employer and employee do not collude to evade. In cooperation with the Norwegian Tax Administration we designed a randomized field experiment with unannounced on-site audits. Matching audit data to administrative registers, we provide evidence of collusive tax evasion. We find that firms assigned to be audited increased their subsequent wage reporting on behalf of their employees by 18 percent relative to firms assigned to the control group. The effect is more pronounced among small firms with few employees. Our results document limitations of third-party reporting, but also that these limitations can be counteracted by minor on-site audits.

1. Introduction

Third-party reporting is a vital part of enforcement in modern tax systems. However, this is still no guarantee for accurate reporting if parties coordinate in underreporting (Yaniv 1992). Collusive tax evasion can be challenging to detect, and in particular through desk audits, if the employer under-reports the wage payments and the employee cooperates by not correcting these third-party reports. Kleven et al. (2011) document in their influential paper that extensive randomized (desk) audits in Denmark only detected substantial under-reporting of income among the self-employed, who are not subject to third-party reporting. The few existing papers that document collusive tax evasion are mainly from developing countries where third-party reporting is not implemented on wage income (e.g. Kumler et al. 2020 for Mexico; Bergolo and Cruces 2014 for Uruguay; or in the case of VAT e.g. Pomeranz 2015 for Ecuador). Kleven et al. (2016) argue that even though collusive tax evasion can be hard to sustain in modern tax systems with accurate business records and third-party reporting, it may still be possible for employer and employee to collude to evade in small firms.

The main contribution of the current paper is that we document the existence of small firms' underreporting of wage payments and collusive tax evasion in a developed country with fully implemented third-party reporting and withholding taxes. We are to our knowledge to first to document this. In cooperation with the Norwegian Tax Administration, we designed and implemented an experiment with randomized and unannounced on-site audits set up to credibly detect and/or deter unreported labor by firms in the service sector. By merging the audit information to administrative micro data, we were able to show that the audits lead to substantial increase in reported wages and number of employees, implying the presence of pre-audit unreported labor. This means that even the best third-party information reporting system can break down for small firms.

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Recent studies have found that on-site personal visits by a civil servant tend to have a larger effect on compliance than both desk-audits (D'Agosto et al. 2018) and receiving a letter (Telle 2013; Dörrenberg and Schmitz 2017). Boning et al. (2020) rely on a field experiment and find that on-site personal visits by a civil servant have a substantial immediate effect on tax remittance by US firms that seemed to be falling behind on their tax deposits of employment taxes. The US Internal Revenue Service did not match tax returns against third-party reports until long after the filing deadline. In such a setting, unilateral noncompliance by one party without the collusion of the other could be more prevailing (i.e. employers' theft of taxes withheld). This is very different in Norway, where personal tax returns are pre-filled by the tax authority with income reported by third parties (employers, banks, etc.) and successful evasion would require some sort of coordinated misreporting.

In general, the main way for public agencies to detect undeclared work is through unannounced on-site audits that determine the identity of all workers present. However, in a series of inspections by the Danish Customs and Tax in 2004, one third of unregistered employees claimed that it was their first working day in the firm (Kolm and Bo Nielsen, 2008). To avoid this, the Norwegian government introduced a new monitoring rule to combat undeclared work from 2014, requiring firms to maintain a real-time staff register of every person present at the workplace at any time. These registers ought to be available at the site for unannounced on-site audits.

In order to analyze the effects of the new 2014 staff register audits, we cooperated with the Norwegian Tax Administration to design and implement a field experiment where 2,462 firms required to keep a staff register were randomly assigned to an audit group and a non-audit group. The audits were unannounced, on-site, and directly targeted at detecting undeclared work by examining whether all employees present at the time of audit were registered in the staff register. These on-site audits, although relatively non-extensive, are well suited to detect unreported personnel, and thus to have a stronger deterrence effect on wage underreporting than the desk-audits used in many previous studies (e.g. Kleven et al. 2011; DeBacker et al. 2015; Advani et al. 2017; Kotsadam et al. 2021).

We expect wage reporting to increase if the audits had a deterrence effect on collusive tax evasion. If evasion also comprised the use of undeclared workers, we anticipated an increase in the number of reported employees. Furthermore, if some firms failed to report wages, we expected the fraction of firms reporting to increase.

Our results show that firms assigned to be audited on average increased their wage reporting on behalf of their employees by 18 percent and the number of reported employees by 22 percent, relative to firms assigned to the control group. We also find that this type of evasion is more common in smaller firms, in line with the theoretical arguments of Kleven et al. (2016) that, while collusive tax evasion can be hard to sustain in firms with many employees and accurate business records, evasion can be more easily coordinated in small firms. Our findings suggest that on-site audits, even if they are non-extensive and inexpensive, can be a necessary supplement to desk-audits, in order to increase compliance especially for small firms with few employees.

2. Institutional setting

2.1. Third-party reporting and remittance of wages

In Norway, personal income tax return preparation has been fully automated for the vast majority of taxpayers, as a result of extensive third-party reporting.[1] Taxpayers are still legally responsible to examine all the information stated on the pre-filled tax returns and report inaccuracies to the Tax Administration by a given date. The pre-filled returns (upfront matching) provide an efficient tool to screen individuals' tax returns, where any form of unilateral noncompliance by employers or employees is likely to be detected with minimal enforcement resources required.

Employers are required to withhold income tax and social security contributions from employees' paychecks, and to report wages and remit taxes to the Tax Administration. The employer is also required to document this remittance by sending a receipt to the employee, stating the before-tax wage and the amount of the taxes withheld for each wage payment and at year-end.

If an employer fails to actually remit the amount of taxes withheld to the Tax Administration, the wage slip serves as a documentation of remittance on behalf of the employee and the employer is responsible for paying the taxes owed. If the employee cannot provide such a wage slip to document withheld taxes, the employee is liable for the missing tax payments. The employee thus has a strong incentive to check that the wage he receives is what the employer reports to the Tax Administration. Hence, we argue that unilateral noncompliance by one party without the collusion of the other (i.e. employer's theft of taxes withheld), is not likely to be prevailing in our setting. This is different from the situation in many other countries, where tax returns are not automatically matched against third-party reports (or are only matched after the filing) and where discrepancies will only be detected through (desk) audits.

2.2. Incentives for participating in collusive tax evasion

By under-reporting wages, the employee escapes income taxes (progressive schedule starting at 27 percent in 2014) and social security contributions (8.2 percent). The employer foregoes the tax deduction for the undeclared wage costs resulting in overpayment of profit taxes (27 percent) in the case that profits are positive, but benefits from reduced pre-tax wage costs and reduced employer's social security contributions (14.1 percent).

Collusion could also involve tax evasion in other dimensions. If a firm underreports (cash) income and uses that income to pay undeclared wages to employees, both personal income taxes and payroll taxes as well as corporate income tax and VAT are evaded. However, underreporting also comes with a cost in the form of reduced legal rights and social security benefits for the employee, and potential reduced access to credit and markets for the firm. In line with the seminal model by Allingham and Sandmo (1972), the employer and employee are expected to balance their net benefits of participating in such tax evasion towards the perceived probability of being detected and the penalty of being detected.

2.3. Experimental design

In 2014, the Norwegian government introduced a new monitoring rule to combat undeclared work. Firms in certain service sectors with a high degree of private customers and possibility for cash turnover - food service, hairdressers, beauticians, car repair and car care-businesses - are required to maintain a staff register (Thorsager and Melsom, 2017). This register should record any personnel present at the workplace at any time and be available at site for unannounced audits by public agencies.

In cooperation with the Norwegian Tax Administration in Oslo we designed and implemented a field experiment with firms ran-

[1] In 2013, 63 percent of the returns were fully pre-filled and 91 percent of personal income tax returns were filed electronically (OECD 2015).

Table 1
Descriptive statistics by stratum.

Stratum	# firms	Share reporting wages	Mean total wage bill (mill NOK)	Mean number of employees	Share assigned to treatment group	Share in the treatment group audited	Share in the control group audited
1	50	0.56	0.51	6.76	0.48	0.25	0.08
2	92	0.24	0.25	1.82	0.70	0.27	0.21
3	250	0.36	0.56	2.21	0.19	0.15	0.01
4	271	0.82	0.79	4.46	0.71	0.47	0.03
5	53	0.17	0.06	0.55	0.64	0.12	0.00
6	482	0.06	0.02	0.18	0.04	0.15	0.01
7	551	0.74	0.65	6.63	0.55	0.65	0.07
8	52	0.62	0.33	4.48	0.29	0.73	0.03
9	198	0.33	0.11	2.08	0.05	0.67	0.02
10	9	0.67	0.90	12.56	0.44	1.00	0.00
11	39	0.62	0.70	6.03	0.08	0.33	0.00
12	70	0.33	0.29	3.77	0.66	0.50	0.29
Total	**2,117**	**0.45**	**0.41**	**3.45**	**0.36**	**0.48**	**0.03**

Note: Number of firms, fraction of firms reporting positive wages, average reported wage and reported number of employees in 2012, as well as fraction of firms assigned to treatment group and fraction of firms that actually end up being audited in treatment and control group, by strata. Main analytic sample cf. Section 3.2.

domly assigned to on-site audits. The auditors examined the firms' staff registers and compared it to the personnel present at the workplace at the time of audit. The total sample of firms included in the experiment consisted of all active firms in the Oslo region presumed to be required to keep a staff register in January 2014 (according to NACE-codes), in total a base population of 2,462 firms.

When designing such experiments, there is a trade-off between the tax administrations' need for higher audit probability among firms they consider "usual suspects" and highly likely to evade, and the methodological need for randomization in order to ensure identification and credible estimates. A pragmatic solution to this is a "blocked" or "stratified" randomized experiment (Telle, 2013). The Tax Administration thus divided the firms into 12 strata; first by the three main industry categories, and then in sub-categories according to i) size (turnover and number of employees) and (only for restaurants) ii) assumed degree of legitimacy based on previous experience. This enabled a risk-based audit policy, i.e. to impose a higher audit probability to some strata (see Table 1). Thus, we could implement the randomized experiment while the Tax Administration still complied with their requirements to utilize audit resources in a risk-based manner. In each stratum, a varying proportion of firms were randomly assigned to the treatment group for unannounced audits of staff registers. In total 923 firms were drawn to be audited. The remaining 1,539 firms were assigned to the control group and were not supposed to be audited.

The audits were carried out in 2014 and the first half of 2015. Audits were unannounced and the taxpayers were not told that the audits were part of an experiment.

If the auditors discovered irregularities in the staff register, the firm was fined. Also, more than half of the violating firms received a follow-up on-site audit. The fine for failing to maintain the register was about 1,000 € (NOK 8,600) for the first offence and 2,000 € for further offences within 12 months. In addition, a fine of 200 € was imposed for each person not included in the register at the time of audit. Since the vast majority of these firms are small, these are non–negligible sums for the firms in question.

The main deterrence effect of these audits may not necessarily be the risk of being fined for failing to maintain the staff register, but that the Tax Administration would pursue irregularities and uncover undeclared work and turnover through more comprehensive audits. While we did not access information on such actions undertaken by the Tax Administration, our results should be interpreted in light of possible expected follow-up of suspicious firms in line with standard procedure of the Tax Administration. If the audits increased the perceived probability of detection and imposed a deterrence effect, we would expect audited and evading firms to *increase* their wage reporting. But being audited could also induce evading firms to close down their activity and workers to move to other formal or informal firms or to exit the labor market, which would appear as *reduced* reporting in our data leading to a bias toward zero in our estimates.

Taking this to our empirical strategy, we expect wage reporting to increase if the audits had a deterrence effect on collusive tax evasion. If evasion also comprised the use of undeclared workers, we anticipated an increase in the number of reported employees. Furthermore, if some firms failed to report wages, we expected the fraction of firms reporting to increase.

3. Data description

3.1. Outcome of the experiment

We have a list of all 2,462 firms that were part of the experiment, with information on stratum and whether the firm was randomly assigned to the treatment group (923 firms) or the control group (1,539 firms). Furthermore, we have all the reports from the on-site staff register-audits of these firms in 2014 and the first half of 2015, with information about the audits, such as start and end date, the outcome of the audits ("accepted" or "fined", and the size of the fine if applicable) and any annotations made by the auditor. The audits revealed extensive violation with the staff register requirement; nearly a quarter of the audited firms were fined in the first round of audits.[2] However, our analysis does not depend on the outcome of the audits but on how the firms alter their reporting after being audited.

The sectors required to maintain the staff register - food service, hairdressers, beauticians, car repair and car care-businesses - are dominated by small firms that tend to be short-lived, and the recording of bankruptcy, end of business, change of owners or industry often lag by quite some time in the administrative data

[2] In the first audit, 25 percent of the firms received a fine, 28 percent received a warning but no fine and 47 percent were accepted. In the second audit, 30 percent of the firms that received a fine in the first audit were fined also the second time. Among the firms found in compliance in the first audit, 20 percent were fined in the second audit. Since the second audit is not randomly assigned, we cannot infer any causal effect from these findings. We do not know whether these firms did actually comply in the first audit, or if (some of) these firms were in fact in violation also in the first audit without being detected. These findings may also suggest that the auditors follow up with a new audit in firms for which they (correctly) suspect (but cannot satisfactorily document) violation.

we used for our sample definition. Thus, the auditors often experienced they would go to an address and find that the firm no longer existed or that another firm was operating on the premises. Thus, for 39 percent of the firms in the treatment group, we do not have any audit report, indicating that the firm was not audited. For additionally 23 percent of the firms, we have audit reports but the outcome of the audit (‘‘accepted" or ‘‘fined") is lacking, suggesting either that the audit for some reason was cancelled or that the auditor discovered minor irregularities in the staff register, but chose to give a warning and not to levy a fine. In these cases, with missing outcome of the audit, we used annotations made by the auditors to distinguish between assumed audited and not audited firms. In total, we find that only around half of the firms randomized to be audited were actually audited (see Table 1).

Some firms may also have been mislabeled in the registry from which they were drawn due to e.g. incorrect sector classifications.

An additional complication is that firms randomly assigned to the control group were in fact audited. This is a common challenge with real field experiments where tax authorities must balance the benefits of a randomized experiment and the drawback of letting tips of suspected evasions (received after randomization) go unattended. In our main analytic sample (described in Section 3.2), 47 of the 1,354 firms in the control group were audited. This means that the audit frequency of the control group (3 percent) is not negligible, though it is small compared to the 48 percent audit frequency in the treatment group, as seen in Table 1.

3.2. Administrative data

To analyze the effect of audit on firms' tax compliance, we merged the data from the experiment with administrative data from Statistics Norway, relying on unique firm identifiers available in all registers in Norway. We accessed yearly data for 2009–2014 on reported wage payments by all Norwegian employers to all employees on employment level. Also, we add firm characteristics variables from the firm registry, such as organizational form, age of firm, and registry status.

Half of the firms did not report any wage payments in 2014. The choice not to report might be an effect of the audit, even if there is no statistically significant difference in the propensity to report in the treatment and control group. These firms are thus included in the analysis with the outcome variable set to zero. This implies that we measure the total effect of audits, including any potential effects on the extensive margin through firms exiting or starting to report.

The audits were carried out during all of 2014 and the first half of 2015. The annual deadline for employers to report wages for the fiscal year t is the end of January in t + 1, one month after the end of the fiscal year t, but reports can be adjusted until March 1st in t + 1. Audits carried out until March 1st in t + 1 may affect the reporting for year t, because firms can modify their reports immediately. Firms also report wages and remit withholding taxes throughout the year. However, this is reported on the firm level only, and we assume that firms can easily increase their final tax reporting on the employee level at year end if they are deterred after being audited. Thus, reports for the fiscal year 2013 could be affected by the audits carried out in the beginning of 2014 (January-March).

As pre-reform information, we thereforerely on the reports for the fiscal year 2012 to be sure that we have information not affected by the audits. We rely on reports for the fiscal year 2014 to measure effects of the audits on the outcomes, though this may result in some bias toward zero in effect estimates as 7 percent of the audited firms in the treatment group were audited after March 2015.

In our analysis, we include non-reporting firms, but we exclude 42 firms that were deleted from the firm register prior to audits and 65 firms with multiple observations, leaving us with a sample of 2,355 firms.[3] To focus on firms where collusive tax evasion is likely to take place and to reduce the impact from some outlying very large firms, we excluded the 10 percent largest firms (i.e. firms reporting > 26 employees in our baseline year, 2012) from our main analytic sample, which then contains 2,117 firms.[4] The majority of these firms are very small, with an average of 3.4 reported employees in 2012, and almost half of the firms did not report any wage or employees in 2012 (see Appendix Table A.1, and Table 2 in Bjørneby et al. 2018 for details).

4. Empirical strategy

Because firms were randomly assigned, audited and non-audited firms should be statistically indistinguishable pre-treatment (2012), and the effect of audit can then be estimated by comparing outcomes across the two groups post-audit (2014). We start by estimating the average causal effect of the audits by comparing firms' outcomes in the treatment group relative to the control group post-treatment (2014).

By linking the audit-data to data on outcomes reported by the same firms from both before and after the intervention, we also compare *changes* from 2012 to 2014 in the outcomes across the treatment and control group (first-difference). This approach could help to improve the precision of our effect estimates and to correct for any random imbalance (in levels) pre-treatment. This difference-in-differences design does not require the two groups to be equal before treatment, but rests on the somewhat weaker assumption that, absent treatment, the *change* in the outcome variable in the control group is a good estimate for the counterfactual change in the treatment group. Similarly, we also show results from a model where we control for the lagged value of the outcome variable (McKenzie, 2012).

Only about half of the firms assigned to the treatment group were actually audited and a number of firms were audited even though they were assigned to the control group. By including all firms in our treatment and control group, whether or not they were actually audited, we estimate the effect of being assigned an audit, i.e. the intention to treat effect (ITT). While this can be considered the estimate most relevant to capture the effect of the policy intervention, it is an estimate biased toward zero of the effect of actually being audited. Briefly, we will also provide estimates of local average treatment effects (LATE), instrumenting actually being audited with being assigned an audit. This measures a causal effect of being audited using the exogenous variation in audit probability generated from the random assignment as an instrument.

As discussed in the previous section, randomization was conducted within each of the 12 strata with the proportion of treated varying substantially across strata. One common motivation for such a design is that stratification could increase precision if it is based on characteristics that are correlated with the outcome variable (Athey and Imbens 2017). In our experiment, however, the main motivation for stratification was to meet the Tax Administration's need for risk-based audit policy.

This stratification has two important implications for our analysis. The first being that treatment is not random in our overall

[3] The reason for such duplicate firm IDs is that one firm may have several geographically different points of sale, and each such point of sale may then be included with its sub-ID in the population, even if they belong to the same legal entity with identical firm ID.

[4] We also conducted the analysis for the whole sample, for the sample excluding the 5 percent largest firms (i.e. firms reporting >46 employees in our baseline year, 2012) and for subsamples with fewer employees in 2012, which similar results, and results do not appear to be particularly sensitive at this margin. The estimates are reported in Appendix A.1.

Table 2
Estimated effects of being assigned an audit (ITT).

	Treatment group	Control group	Difference	Robust s.e.
Wage (mill. NOK)				
2012	0.40	0.41	−0.01	0.04
2014	0.67	0.58	0.09	0.06
Change 2012–2014	0.27	0.17	0.10*	0.05
2014 with control for lagged value			0.10*	0.05
Number of employees				
2012	3.37	3.38	−0.02	0.26
2014	6.20	5.10	1.10*	0.56
Change 2012–2014	2.83	1.72	1.12*	0.52
2014 with control for lagged value			1.11*	0.52
Fraction of firms reporting positive wages				
2012	0.43	0.43	0.00	0.02
2014	0.51	0.50	−0.01	0.03
Change 2012–2014	0.08	0.07	−0.01	0.03
2014 with control for lagged value			−0.01	0.03
Number of observations				
N observations	763	1,354	2,117	

Note: The columns denoted Treatment group and Control group include reported wage, reported number of employees and fraction of firms reporting positive wages by treatment and control group in 2012 and 2014. The next column (Difference) provides effect estimates from OLS cf. Eq. (1). Each figure from a separate regression on the given dependent variable and weighted with the number of firms in each stratum as defined in Section 5/Appendix A.0. Main analytic sample cf. Section 3.2. Robust standard errors account for heteroscedasticity. * indicates significance at the 5 percent level.

sample, only within each stratum. Second, to the extent that the Tax Administration is actually correct in their beliefs about risk, the strata with high audit-rates will contain a substantially larger proportion of violators than other strata. Consequently, we suspect the treatment effects to also be heterogeneous across strata. To get a consistent estimate of the average treatment effect in the population, we thus need to weight the treatment effect within each stratum by each stratum's share of the population.

In principle, we have 12 separate randomized experiments, one within each stratum. By including a full set of interaction of treatment and strata dummies, i.e. allowing for different treatment effects within each stratum, we can estimate stratum-specific ITT in the following linear model:

$$y_i = \sum\nolimits_{j=1}^{J} \beta_j \times C_{ij} + \sum\nolimits_{j=1}^{J} \tau_j \times Treat_i \times C_{ij} + u_i \quad (1)$$

where y_i denotes our outcome variable (the level of, or in the first difference setting, the change, in wage, number of employees and a dummy for whether the firm reports positive wages), $Treat_i$ is a dummy variable indicating whether firm i was randomly assigned an audit, C_{ij} are strata dummies and u_i is an error term with conditional expectation zero. Then, τ_j is an unbiased and consistent estimator for the ITT in stratum j.

However, we are interested in the ITT for the whole population, not the effects within each stratum (some of the strata are very small, and even if we were interested in the effect within a stratum, our experiment does not have enough power within each stratum).[5] Thus, we calculate the weighted average of the within-stratum average treatment effects, with weights being the share of firms in each stratum.[6]

We also set out to estimate the local average treatment effect (LATE) of actually being audited. Maintaining the idea that we *de facto* have 12 separate randomized experiments, a baseline IV-model could be described by the following two-equation system, instrumenting actual audit with being assigned to treatment group:

$$Audited_i^j = a^j + b^j \times Treat_i + u_i^j \text{one equation for each j = 1,..,J} \quad (2)$$

$$y_i = \sum\nolimits_{j=1}^{J} \beta_j \times C_i + \sum\nolimits_{j=1}^{J} \tau_j \times \widehat{Audited_i^j} + \varepsilon_i \quad (3)$$

where $Treat_i$ is a dummy variable indicating whether firm i was assigned to the treatment group, $Audited_i^j$ indicates whether the firm was actually audited, and C_{ij} are strata dummies. We estimate the parameters τ_j by performing a 2SLS with the vector of equations (2) as the first stage and equation (3) as the second stage. Similar to above this provides one LATE for each stratum, and we get the average over all strata, by weighting by the share of firms in each stratum.

While our reduced form ITT estimates can be given causal interpretation as long as the assignment was random, the IV estimates rely on three additional assumptions. First, our instrument (*Treat*) should only affect the outcome variables (y) through the probability of being audited (*Audited*). This exclusion restriction is likely to hold, as the random assignment is only observed by the firm through the audit. Second, being assigned to the treatment group (control group) should increase (decrease) the probability of being audited for each firm (monotonicity assumption). It seems plausible that audited firms in the control group would also be audited if they were assigned to the treatment group, and similarly that non-audited firms in the treatment group would not be audited if they were assigned to the control group. Third, being randomly assigned an audit (i.e. being in the treatment group) is a good predictor of actually being audited. As previously noted, some of the firms in the control group were in fact audited, and half of the firms in the treatment group were not audited. Moreover, the fraction of firms in the treatment group that were not audited, and the fraction of firms in the control group that were audited, varies considerably across strata. This weakens the correlation between being assigned to treatment group and receiving an audit, particularly in some strata. As a result, we cannot estimate the LATE precisely (Angrist and Pischke 2009, p. 209), and our results may also suffer from weak instrument bias (toward OLS), including too small standard errors. We return to this in Section 5.3.

It is worth noting that these audit effects might be bigger in 2014 than they would have been in later years since the employee register introduced that year. More information, learning, and the presence of a credible audit threat likely will increase compliance over time. This also means that our results may be more informative about non-compliance that probably existed before 2014.

[5] Formal tests confirm that the effects are clearly statistically significantly different across strata in our data.

[6] This can be done by running OLS on Eq. (1) and weight the estimates and calculate the standard errors, or by transforming the parameter vector as described in Appendix A.0.

5. Results

5.1. Graphical evidence

The simplest way to test the effect of being audited on subsequent reporting is to track the reporting by firms in treatment group and control group over time. Fig. 1 plots means of wages, number of employees and the fraction of firms reporting wages in treatment and control groups from 2009 to 2014, where means for each of the 12 strata are weighted with the number of firms in each stratum.

We observe the same level of reporting up until 2012 (pre-treatment). From 2013, firms in the treatment group increased reported wages (Panel A) and number of employees (Panel B) relative to firms in the control group, indicating a positive effect of audits on firms' subsequent tax reporting. However, the fraction of firms reporting any wage (Panel C) does not seem to differ between the treatment and control groups, indicating that the audits did not affect the likelihood that the firms would report any wages. We also note that the randomization seems to have worked well, in that outcomes are nicely balanced in the closest pre-treatment year 2012, and also that trends in the previous years are similar across treated and comparison groups.

5.2. Effects of being assigned an audit (ITT)

Our regression results, presented in Table 2, confirm that being assigned an audit had statistically significant positive effects on both reported wages and number of employees after the audit. The table shows averages for reported wages, reported number of employees and the fraction of firms reporting wages in 2012 (pre-treatment) and 2014 (post-treatment) for the treatment and control groups separately. The third column shows the differences in the outcome variables between the treatment and control group in 2012 and 2014, as well as the differences in the changes from 2012 to 2014.

Prior to treatment, average reporting on the outcome variables in the treatment and control group were statistically indistinguishable. This balance on all of the outcome variables in 2012 is indeed reassuring in that the randomization was implemented as intended.[7] After treatment, firms in the treatment group reported substantially more wage and more employees. Using a first-difference approach, the estimated ITT on firms' wage reporting is NOK 100,000, which amounts to an 18 percent increase. This provides compelling evidence that wages have previously been underreported, and that audits had a deterrence effect that increased compliance. Furthermore, the effect on the reported number of employees is even stronger, with 1.1 employees or a 22 percent increase, which may be taken to suggest that underreporting has taken the form of not declaring workers, rather than not reporting all the wage actually paid (cash) to declared workers.

The audits do not seem to have had any effect on the probability of a firm reporting wages on the extensive margin, suggesting that the evading firms have not been completely informal when it comes to reporting workers (or at least that the deterred evasion does not comprise this form of evasion).

Next, we investigate potential heterogenous effects by firm size. In Fig. 2, we split the sample in mutually exclusive groups by number of employees in 2012. For each group, we show the relative effect estimates (ITT) on reported wage along with the average number of employees pre-treatment. While none of these estimates are statistically significantly different, we see that the relative effect estimate (in percent of the estimated counterfactual) is largest for the firms with very few employees pre-treatment, and gradually decreasing by firm size.[8] These empirical patterns align surprisingly well with the theoretical arguments of Kleven et al. (2016) that, while collusive tax evasion can be hard to sustain in firms with many employees and accurate business records, evasion can be more easily coordinated in small firms.

5.3. Effects of actually being audited (LATE)

As estimates of the effect of being audited, the ITT estimates presented above are too low since half of the firms randomized to be audited did in fact not receive one, and, moreover, since some firms were audited even though they were assigned to the control group. While the ITT estimates measure effects of being assigned an audit, the LATE estimates measure effects of actually being audited. As we would expect, the LATE estimates, shown in Table 3, are larger in magnitude than our ITT estimates.

The IV estimates show that being audited on average increased the audited firms' wage reporting by NOK 420,000, the reported number of employees by 3.6, and the fraction of firms reporting wage by 3 percentage points. However, these estimates are not statistically significant at conventional levels, or are just barely so.

The mostly statistically insignificant estimates may relate to the fact that being assigned an audit is a weak instrument of actually being audited. While the correlation between being assigned an audit and actually being audited is positive in all strata, it differs from 0.05 to 1. Moreover, in almost half of the strata it is not statistically significant at the 5 percent level (t-test), and in only 5 of the 12 strata the F-test statistic exceeds the rule-of-thumb of 10 for adequately strong instruments (Stock et al. 2002). Since we have exactly the same number of instruments as we have endogenous variables (one for each stratum), this implies that we should expect imprecise LATE estimates (Angrist and Pischke 2009, p. 209).[9]

6. Concluding remarks

The main contribution of the current paper is that we document the existence of firms' underreporting of wage payments in a developed country with fully implemented third-party reporting and withholding taxes. We utilize a randomized field experiment with simple on-site audits in combination with administrative data to show that firms increase their wage reporting substantially post-audit, providing evidence of collusive tax evasion between employers and employees.

The audits were relatively simple, with the sole purpose of checking that all employees present at the workplace at the time of the audit were duly registered in the personnel list. If not, the firm was issued a fine. In that sense it was not a traditional tax audit, which is normally more comprehensive and eventually produces a sum of evaded taxes as a direct outcome of the audit. Instead, our estimate represents an indirect measure, post-audit,

[7] Pre-assignment outcome variables are a more comprehensive measure of possible imbalances than other and more specific pre-characteristics of the firms. We did find one pre-characteristic that did differ --- the age of the firms in our treatment group were on average 1.5 years younger than firms in our control group. In Appendix A.2 we show that controlling for age does not affect our main results.

[8] Additional results where we gradually add firms with more employees are provided in Appendix Table A.1.

[9] The weak instrument bias (toward OLS), along with too small estimated standard errors, is less of a concern for just-identified estimators (Angrist and Pischke 2009). The limited information maximum likelihood (LIML) estimator can be less biased (and without similarly too small standard errors) when instruments are weak (op. cit.). Our estimates are virtually unchanged, including standard errors, when we apply LIML instead of 2SLS.

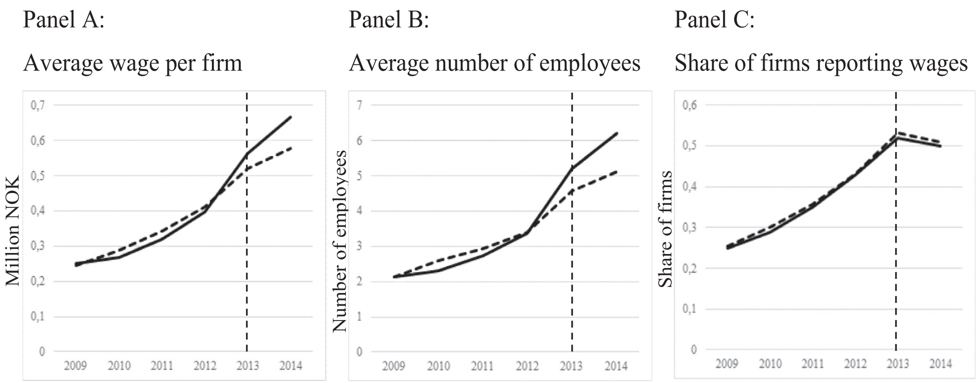

Fig. 1. Average wage, number of employees and fraction of firms reporting wages in treatment group (solid line) and control group (dashed line) 2009–2014. Main sample.

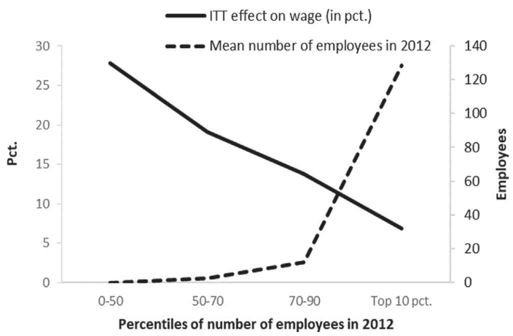

Fig. 2. The effect estimate is larger for firms with few employees. Note: Means for each of the 12 strata weighted together with the number of firms in each stratum.

of previously underreported wage payments from the firm. We find that these firms underreport a substantial fraction of their wage bills, but it is, however, unclear how large this wage underreporting is in aggregate, since the audited firms were rather small, in specific service sectors in one city (the capital) in Norway.

Although our results are specific to our setting, they show clearly that even in Norway's state-of-the-art system of information reporting, third-party reporting is not necessarily self-enforcing in settings where the employer and employee can collude to evade. This suggests that other countries can experience problems of such collusive evasion, and that efforts in excess of merely relying on third-party reporting and paper trails should be taken to prevent it. Our results also point at a policy solution to the problem: a legal framework requiring real-time lists of all employees combined with inexpensive on-site audits. This is important to consider for tax administrations when allocating their enforcement resources – they need to identify in which situations the third-party can be trusted and in which situations they are likely to collusively underreport. Further research is required to unravel the mechanisms in place and to estimate the size of these under-reported wage payments in a more general setting.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Table 3
Estimated effects of actually being audited (LATE).

Change 2012–2014	Compliers in treatment group	Compliers in control group	IV	Robust s.e.
Wage (mill. NOK)				
2012	0.46	0.43	0.03	0.36
2014	0.97	0.52	0.45	0.51
Change 2012–2014	0.51	0.09	0.42	0.26
Diff 2014 with control for lagged value			0.42	0.26
Number of employees				
2012	3.55	3.23	0.32	1.96
2014	7.78	3.90	3.88	2.59
Change 2012–2014	4.23	0.67	3.56	2.23
Diff 2014 with control for lagged value			3.67	2.16
Fraction of firms reporting positive wages				
2012	0.49	0.49	0.00	0.18
2014	0.82	0.79	0.03	0.26
Change 2012–2014	0.33	0.31	0.03	0.16
Diff 2014 with control for lagged value			0.03	0.22
N observations			2,117	

Note: The columns denoted Compliers in treatment group and Compliers in control group include estimated reported wage, reported number of employees and fraction of firms reporting positive wages for the compliers in the treatment and control group in 2012 and 2014. The next column (IV) provides effect estimates from 2SLS with audited dummy interacted with strata dummies, and audited being instrumented by being assigned an audit for each stratum separately; see Eqs. (2) and (3) in Section 5. Each figure comes from a separate regression on the given dependent variable and weighted with the number of firms in each stratum as defined in Section 5/Appendix A.0. Main analytic sample cf. Section 3.2. Robust standard errors account for heteroscedasticity. * indicates significance at the 5 percent level.

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