Tax Noncompliance Penalties: Evidence and Optimality*

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Abstract

We study tax noncompliance penalties as an enforcement instrument, focusing on their effects on compliance behavior and tax administration. Using novel administrative data from Norway that link penalty assignments to appeals and tax records, we analyze a reform that reduced the statutory penalty rate from 30% to 20%. Comparing cohorts of noncompliant taxpayers, we estimate an elasticity of reported income with respect to the penalty rate of 0.3 to 0.45. We also document substantial administrative effects. Higher penalties increase appeal activity: doubling the penalty raises the likelihood of appeal by 60%, leading to longer processing times and lower expected collections. This shows that penalties are not costless, but involve important administrative trade-offs. Further, we develop a framework for optimal penalty design and show that, to a first-order approximation, the actuarial penalty rate should equal the optimal linear income tax rate. Because detection probability is low, calibrating the model to Norway yields an optimal penalty rate of about 200%.

JEL codes: H26, H83, H21, H22, H23, H24
Key words: Tax penalties, tax administration, tax evasion and avoidance

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1 Introduction

Little is known about the consequences of penalties for tax, regulatory, and criminal noncompliance, as well as for administrative and enforcement outcomes. Penalties are intended both to deter noncompliance ex ante and to generate revenue ex post when violations are detected. Yet their effects depend not only on their statutory level, but also on how they interact with administrative processes such as case selection, appeals, and enforcement capacity. A substantial literature has studied tax noncompliance, from modeling its behavioral foundations (Allingham and Sandmo (1972); Yitzhaki (1974); Becker (1968)) to providing empirical estimates of its size, composition, and distribution (Guyton et al. (2023); Kleven et al. (2011)). Recent work has focused on optimal tax administration (Keen and Slemrod (2017)) and quantifying the benefits and costs of different measures to reduce tax noncompliance—namely audits (Boning et al. (2024)).

However, much less attention has been devoted to the penalty rate itself as a tax enforcement instrument. Theoretically, higher penalties generate more revenue both through behaviorally decreasing income underreporting and mechanically through increasing collections on infra-marginally detected underreported income. At the same time, unlike audits that require considerable operational resources, there is no nominal cost associated with changing the penalty rate. This combination makes penalty rates particularly attractive for tax administrations. Yet, there is a striking lack of evidence on the *real* costs and benefits of changing the tax noncompliance penalty rate and on regulatory noncompliance penalties more broadly. How should policymakers and economists think about optimal penalties in the context of tax and other domains of noncompliance?

We address this gap by studying the administration of, and a reform to, the statutory tax noncompliance penalty rate in Norway. Using novel administrative data that link penalty assignments to appeal outcomes, tax returns, and demographic characteristics, we provide, to our knowledge, the first evidence on both taxpayer responses to intensive-margin changes in the penalty rate and the real administrative costs associated with higher penalties. This integrated dataset allows us to quantify administrative costs along several dimensions, including appeal behavior, collection probabilities, and time to resolution. Our analysis focuses on the ex post effects of the penalty rate following detection and sanction through personal income tax audits.[1] We show that penalty rates have sizable impact not only on reported income, but also on appeal behavior and administrative outcomes, and we use these insights to characterize the trade-offs underlying optimal penalty design.

We begin by presenting a behavioral modeling environment of taxpayer responses to penalties, which illustrates the tradeoffs generated by higher penalty rates. While higher penalties have the potential to reduce ex-ante tax evasion, they may also be accompanied by unintended adverse effects. First, a higher penalty burden raises the incentive for taxpayers to contest the penalty through formal appeal and to expend more resources pursuing those appeals. Second, taxpayers may substitute towards riskier evasion vehicles that are harder to detect for the tax authorities.

Our empirical analysis begins with establishing novel descriptive facts about penalty assignment and administration. In spite of the theoretically positive fiscal strengths of the penalty rate, penalties themselves account for only 0.02% of income tax revenues in a given year (although this descriptive equilibrium object masks the deterrence effects of penalties), where 0.25% of taxpayers receive a penalty every year. Tax compliance penalties assigned typically amount to around 2% of taxpayer's gross income. Compared with tax-compliant taxpayers, penalized individuals earn more, are more likely to declare self-employment income, are more likely to make transfers abroad and to tax havens, and are more likely to own a business; these differences persist after conditioning on audit (although we document non-linearities and outliers in terms of income). Lastly, we find that while penalties are administered in a broadly progressive manner, the effective penalty rate (penalty divided by gross income) is broadly regressive within the population of penalized individuals, although this fact may be attributable to a negative relationship between income rank and underreported income share.

Our central empirical exercise studies a quasi-experiment in the form of a decrease in the Norwegian standard income tax noncompliance penalty rate from 30% of underreported base to 20% of underreported base that occurred in 2016 (a 33% relative decrease in the

[1] We focus primarily on individual income taxpayers, but future work will extend the analysis to businesses and examine ex ante deterrence effects.

statutory penalty rate). As these audits occur in a non-random manner, we estimate the ex-post impact of the penalty rate by implementing a difference-in-differences event study design that relies on a parallel trends assumption for taxpayers sanctioned under the new ordinary penalty rate regime as for the pre-reform ordinary regime. Importantly, the validity of this design also relies on a comparability assumption. We document that the composition of penalized taxpayers indeed changes around this reform, so we validate our estimates by implementing models that attempt to alleviate concerns pertaining to changing composition. Namely, we estimate a matching design and a correlated random effects model that control for pre-audit levels and changes in income. We also incorporate audit data to estimate a model that directly controls for the noncompliance risk score assigned by the tax authorities to tax returns.

We estimate a positive impact of the penalty rate on ex-post declared income and taxes paid. Specifically, we find that the group sanctioned under the 20% penalty rate regime reports between 5-10% income less per year than do taxpayers sanctioned under the prior 30% rate regime, corresponding with a commensurate difference in income taxes paid. Using the reform as an instrument for a change in the penalty rate, we estimate an elasticity of taxable income with respect to the ex-post penalty rate of 0.45 among tax noncompliant individuals. To address the threat that estimated penalty rate effects simply reflect compositional shifts in the sanctioned population around the policy change rather than true treatment effects, we estimate the model under the alternative specifications mentioned above that aim to control for changes in the composition of penalized taxpayers, and obtain similar results. We also document some evidence that taxpayers under the less-harsh post-reform 20% rate regime experience fewer "quality-of-life" disruptions, such as loss of business. In addition, the richness of our data allows us to provide evidence of spillover effects onto household peers and business partners of penalized taxpayers.

We then turn to studying the impact of the penalty rate and size on real administrative costs in the form of formal appeals/complaints as well as collection and processing times. Appeals are costly from the tax administrator's perspective because they involve legal system expenses and delay collection. Consistent with this intuition, we find that larger penalties are associated with longer collection times and lower collection probabilities. We use a similar difference-in-differences design approach around the penalty rate reform to estimate elasticities of appeal probability of around 0.6. We substantiate the robustness of this result by providing similar estimates from leave-one-out estimator designs that leverage the variation in average penalty size among other penalized individuals in the same year and penalty category, as well as in the same municipality in an additional design. Lastly, we show the robustness of our results with respect to the inclusion of individual fixed effects for the smaller sample of penalized individuals that receive penalties on multiple occasions.

We conclude by contextualizing our findings within a model of optimal tax administration to rationalize the penalty schedule in Norway. We model penalties within a broader class of instruments we conceptualize as "dual-revenue-generating" tools—instruments that theoretically increase tax collections both through behavioral effects that reduce underreporting and mechanically through increased collections on infra-marginally detected underreporting. Following an extension of Keen and Slemrod (2017), we show that the social budgeter should set the statutory penalty rate such that the actuarial penalty rate—the in-expectation net payment rate per unit of underreported income—equals the optimal linear income tax rate. Additionally, where canonical optimal tax models show that the presence of income tax evasion drives down the optimal statutory income tax rates (e.g. Sandmo (1981)), accounting for dual revenue instruments such as penalties mitigates this result. We find that in most cases the income tax rate and penalty rate should be set as strategic complements. However, the strategic complementarity/substitutability of the penalty rate and other non-penalty enforcement efforts (e.g. audits) is largely non-trivial, depending on the second-cross derivatives of revenues, administrative costs, and agent income underreporting with respect to enforcement efforts and the penalty rate. Calibrating this model to the Norwegian tax administrative environment, we estimate an optimal statutory penalty rate of 200%. This is significantly higher than the prevailing empirical rate.

We proceed as follows. Section 2 places our results in the broader academic literature. Section 3 develops a conceptual model that illustrates the microeconomic foundations of how taxpayers respond to penalties. Section 4 presents the institutional background, the data environment, and descriptive statistics. Section 5 and Section 6 provide quasi-experimental evidence on penalty assignment and its impacts on income reporting and appeal behavior, respectively. We discuss the implications for optimal penalty administration in Section 7. Section 8 concludes.

2 Placing in the broader literature

We view this paper as contributing to the literatures on tax administration and tax noncompliance, crime and regulatory noncompliance, and inequality. Conceptually, Allingham and Sandmo (1972) and Keen and Slemrod (2017) are the most closely related. The former paper establishes the behavioral foundations of taxpayer's tax evasion decisions, giving the clear prescription that policymakers can affect tax evasion by altering the perceived detection probability or the fine associated with detection and punishment. Many works have focused on the role of audits and tax amnesties in determining income underreporting behavior whose mechanisms operate primarily through detection channels, although no works have empirically established a causal link between individual perception of detection probability and tax evasion (Bergolo et al. (2023), Advani et al. (2023), Alstadsæter et al. (2022), and Slemrod (2019)). In the context of Norway, Bott et al. (2020) uses manipulated moral and detection letters in an experimental design to show that moral suasion significantly increases self-reported foreign income. Similarly, the contributions by Bjørneby et al. (2021), Hebous et al. (2023), and Løyland et al. (2024) point to positive compliance effects of audits as tax enforcement tools. At the same time, little empirical attention has been allocated to the role of penalties. Reflecting this gap, Slemrod (2019) refers to the penalty rate as "the neglected sibling among the two central policy parameters of the Allingham-Sandmo deterrence model" (p. 942). Brounstein (2023) finds that Ecuadorian taxpayers respond strongly to changes in the pecuniary cost of sending money to tax havens. Based on this finding, it is conceivable that taxpayer income underreporting may respond more strongly to pecuniary measures.

Keen and Slemrod (2017) provides a foundational model of optimal tax administration and thinking about how tax administrations should design enforcement instruments, characterizing the complementarity and substitutability between enforcement expenses (most practically taken as auditing) and the income tax rate. The authors provide the framework for a general extension to other instruments, but we consider as our central object of interest precise parameterizations of penalties and their interaction with other instruments—namely enforcement expense and the tax rate. A large literature studies the fiscal impacts of audits (Boning et al. (2024)). However, one key conceptual distinction of pecuniary penalties within the broader class of tax administration instruments lies in the fact that the penalty rate by definition is nominally zero cost: that is, there is no direct cost associated with changing the penalty rates (unless, potentially, one considers political economy costs); at the same time, there is no evidence on the *real* costs (or revenue impacts) of pecuniary penalties.

Becker (1968) establishes the modern foundations of penalties and crime, and expresses expected punishment as the product of punishment probability and -severity (see also Ehrlich (1973)). The overwhelming majority of attention on penalties in the context of criminology focuses on prison/jail sentencing and other non-pecuniary penalties as well as on property and violent crime, as opposed to on financial and economic crime. Norris and Rose (2024) is one of the few studies in this space to focus on strictly pecuniary penalties, in the form of "legal financial obligations" fees that courts levy on guilty defendants in the US. The authors study assignment of these fines primarily in the context of traffic violations and misdemeanor offenses, finding that payment rates do not respond to the size of the fine. There is also a large literature on the criminal deterrence effects of policing (operating on the punishment/detection probability side), largely finding a negative impact on crime (e.g. Levitt (1997)). In terms of ex-post effects of punishment on crime, Rose and Shem-Tov (2021) finds a negative impact of longer prison sentences on criminal recidivism; Huttunen et al. (2026) find a large negative impact of extensive margin prison sentence assignment on financial crime recidivism. While other foundational models rationalize exorbitantly high penalties to deter crime (Dhami and al-Nowaihi (2006) and Sandmo (1981)), there exists little consensus on the impact of more extreme punishment—such as the death penalty—on criminal activity (Donohue and Wolfers (2009)). Dhami and al-Nowaihi (2006) explores this question from a behavioral perspective, asking why the penalty should not be infinite (per the paper, assignment to the death penalty with positive probability of measure zero). The authors adopt a theoretical behavioral perspective based on individual internalization of probability; that according to prospect theory, individuals cannot properly internalize corner-solution expected utility values. We argue that an interior solution for the optimal penalty rate exists based on more practical revenue-raising and welfarist grounds. Kopczuk (2006) similarly argues that greater penalties require criminal prosecution (which is costly) and that these also imply higher costs of false positives, considering that cheating and honest mistakes may be difficult to distinguish.

However, empirical work that examines the role of punishment and detection in deterring economic and financial crime is scarce. Although in principle such offenses are not conceptually distinct from other forms of criminal activity, they pose substantial empirical challenges. In particular, their relatively low frequency and the fundamentally different processes that govern detection, investigation, and litigation make them difficult to study using standard empirical approaches in economics. Rather, most of the research attention on economic and financial crime has come from governmental and NGO spaces (Nazzari and Favarin (2025) and Financial Action Task Force (FATF) (2023)). We view ourselves as contributing novel evidence on the tax compliance and fiscal revenue and cost impacts of different pecuniary penalties in the realm of tax noncompliance, which we regard as more relevant for studying financial crime.

We also view our work as speaking closely to the literature on post-tax income (and wealth) inequality to the extent that tax noncompliance and policy aiming to regulate tax noncompliance have clear first order implications for inequality. In the US, existing work has demonstrated the difference between pre- and post-tax inequality (Auten and Splinter (2024); Piketty et al. (2017); Piketty and Saez (2003))) as well as wealth inequality (Smith et al. (2022); Saez and Zucman (2016)). Penalty assignment has first-order implications for affecting the distribution of post-tax income and for affecting income underreporting activities that may bias our estimates of pre-tax inequality. In particular, larger penalties may induce decreases in income underreporting that are observed disproportionately at the top of the income distribution (Guyton et al. (2023); Alstadsæter et al. (2019); Alstadsæter et al. (2018)). Penalties also have mechanical impacts on the distribution of post-tax income. Alternatively, we demonstrate that higher penalties may also induce taxpayer substitution from high-detection-risk underreporting vehicles to lower-detection-risk vehicles, which has ambiguous effects on the overall amount of income underreporting. We further provide novel descriptive evidence on the allocation of penalties and effective penalty rates along the income- and wealth distribution.

3 Conceptual model of income tax evasion, penalties, and administrative appeal

This section introduces a conceptual framework that illustrates the different ways in which taxpayers may respond to changes in the penalty rate. Higher penalties may increase revenue via decreases in underreporting and increased collections on infra-marginally detected underreporting. At the same time, they can also generate counterveiling behavioral responses, including a greater incentive to engage in administrative appeal and substitution toward harder-to-detect underreporting vehicles. We develop the static behavioral foundations of taxpayers' income underreporting and penalty appeal decisions in four steps. First, we establish an environment building on Allingham and Sandmo (1972). We then explore the relationship between noncompliance penalties and underreporting. In the third step, the tax authority detects underpayment, and the taxpayer decides whether and how much to expend on appeals/litigation. Finally, we explore the taxpayer's joint optimization procedure of simultaneously deciding underreporting and ex-ante appeal decision and effort/expense.

3.1 Model setup

In a static setting, a taxpayer i with true income y is deciding how to underreport her income. She has two vehicles to use, one of which is less susceptible to discovery via audit. She allocates z_1 and z_2 to each vehicle at costs $\xi_1(z_1)$ and $\xi_2(z_2)$, so that she ultimately underreports her income by $z_1 + z_2$, reporting $y - z_1 - z_2 := x$ in income to the tax authorities for personal income tax purposes.

The first underreporting vehicle is riskier than the second underreporting vehicle in terms of discovery in case of an audit, but is less expensive to use. Namely, $\xi_2'(e) > \xi_1'(e)$ and $\xi_2''(e) > \xi_1''(e)$ $\forall$ $e > 0$ and $\xi_2(0) = \xi_1(0) \equiv 0$. Also assume that the use of the vehicles has no effect on the cost of the other (i.e. $\frac{\partial^2 \xi_i}{\partial z_1 \partial z_2} = 0$ $\forall$ $i = 1, 2$). She will be audited with fixed probability ρ_1. Upon audit, all of her underreporting in the first vehicle will be discovered, whereas the underreporting in the second vehicle will be discovered with probability $0 < \rho_2 < 1$ conditional upon audit.

After underreporting and before audit realization, she pays $T(x)$ in taxes, and has disposable income amount $y - T(x) - \xi_1(z_1) - \xi_2(z_2)$. The tax authorities observe this taxpayer i reporting income x and paying taxes $T(x)$. However, the authorities conduct an audit and detect an amount of unreported income z.[2] The penalty and appeal environment can be described as follows:

- Taxpayer i initially reported x in income and paid $T(x)$ in income taxes.
- The tax administration alleges that by underreporting her income by z, they suffered tax loss of $T(x+z) - T(x)$.
- The tax authorities require that i pays back the sum of the tax loss discovered in the audit $T(x+z) - T(x) = l(z;x)$ plus an additional fine $\phi(l(z;x); z+x) := \phi(z,x)$ where $\phi(0;x) = 0$ and $\frac{\partial \phi}{\partial z} > 0 \; \forall \; x, z$.[3]
- The taxpayer can pay a fixed cost F to appeal the allegation plus a variable cost α to reduce the probability that the tax court is successful in its proceedings.
- The tax authorities can pay resources β to increase its probability of success in court.
- The probability the tax administration is successful in appeal comes with probability $r(\alpha, \beta; \theta, z, x)$ for center parameters θ, z, and x. We assume no settlement option.
- Upon the tax authority's successful defense, the taxpayer pays her discovered unpaid taxes and the fine. The tax authority's and the taxpayer's legal expenses are borne by each respective party.[4]
- Upon successful appeal of the taxpayer, she does not make additional payments beyond her initial tax payment of $T(x)$. The tax authority's and the taxpayer's legal expenses are borne by each respective party.

[2] The audit (taken as fixed) may not have uncovered the entirety of taxpayer i's true underreporting, where taxpayer i's true income was $x + z + q$ (where q may equal 0 or z_2 for simplicity). In particular, z takes value z_1 if the audit is unsuccessful in discovering the second vehicle; z takes value $z_1 + z_2 = \mathcal{Z}$ if the audit is wholly successful.

[3] We assume that ϕ does not depend on the manner of underreporting, but rather the overall amount of detected underreporting.

[4] One can also study a legal environment where the losing party pays the appeal fees of the successful party. However, while this practice often occurs in civil court settings, legal fees are not typically reimbursed in the context of tax court.

3.2 Individual optimal underreporting responses

A taxpayer has utility linear in pre-audit disposable income and optimizes her expected utility. Absent audits, she allocates her underreporting portfolio such that $T'(x) = \xi_1'(z_1) = \xi_2'(z_2)$.

In the presence of audits, ignoring endogenous appeal effort, the taxpayer optimizes[5]

$$\underset{z_1,z_2}{argmax}(1-\rho_1)\Big(y - T(x) - \xi_1(z_1) - \xi_2(z_2)\Big) + \\ \rho_1(1-\rho_2)\Big(y - T(x+z_1) - \phi(z_1;x) - \xi_1(z_1) - \xi_2(z_2)\Big) + \\ \rho_1\rho_2\Big(y - T(y) - \phi(z_1+z_2;x) - \xi_1(z_1) - \xi_2(z_2)\Big)$$

Such that $y - T(y) - \xi_1(z_1) - \xi_2(z_2) - \phi(z_1+z_2) \geq 0$.[6]

The taxpayer optimizing z_1 and z_2 here yields first-order conditions:

$$(1-\rho_1)T'(y-z_1-z_2) = \xi_1'(z_1) + \rho_1(1-\rho_2)\frac{\partial\phi(z_1)}{\partial z_1} + \rho_1\rho_2\frac{\partial\phi(\mathcal{Z})}{\partial z_1}$$
$$(1-\rho_1)T'(y-z_1-z_2) + \rho_1(1-\rho_2)T'(x+z_1) = \xi_2'(z_2) + \rho_1\rho_2\frac{\partial\phi(\mathcal{Z})}{\partial z_2}.$$

This system implicitly defines a solution to the optimal underreporting vehicle allocation decision (z_1^*, z_2^*).

A corner solution with zero underreporting corresponds with cost and penalty schedules with steepness far in excess of the tax schedule (or with detection of sufficiently high probability), but can also be rationalized with high fixed costs associated with sheltering. A corner

[5]For simplicity, we presently assume that the taxpayer does not consider the possibility of appeal in her optimal underreporting decision. We explicitly study joint optimization after establishing underreporting and appeal results separately.

[6]We view this budget constraint as the most empirically relevant for the taxpayer's sheltering decision. As opposed to alternate formulations of the budget constraint, this constraint expresses that the taxpayer's income sheltering is limited by consideration of the "worst-case scenario" in which full discovery of sheltered assets requires her to pay full taxes as well as the resulting fine after already having paid for sheltering costs. It also represents a more conservative budget scenario, as it is clearly true that $T(y)+\phi(z_1+z_2) > T(y-z_1-z_2)$. This constraint also comes with the possibly undesirable interpretation that an individual never underreports income to the point that, in case of a successful audit, she would have to engage in borrowing or consumption out of savings to pay back the remainder of taxes and penalties. An alternate formulation, for instance that $y - T(y-z_1-z_2) - \xi_1(z_1) - \xi_2(z_2) \geq 0$, could carry the interpretation that the cost of sheltering is the binding constraint.

solution of maximal sheltering corresponds with cases with a steep tax schedule relative to the steepness of the penalty and sheltering cost schedules (or with detection of sufficiently low probability) (where $z_1 + z_2 = y - \xi_1(z_1^*) - \xi_2(z_2^*)$) with exactly one of either z_1^* or $z_2^* = 0$ except in the knife-edge case where $U'(z_1^*) = U'(z_2^*) > 0$.[7]

We turn to studying the comparative statics of this system at an initial optimal underreporting allocation (z_1^*, z_2^*). For simplicity, consider a linear income tax rate τ and a penalty rate ϕ. The first-order conditions for an interior underreporting allocation define the following system:

$$f(z_1^*, z_2^*, \tau, \phi, \rho_1, \rho_2) = \begin{bmatrix} (1-\rho_1)\tau - \xi_1'(z_1) - \rho_1\phi \\ (1-\rho_1)\tau + \rho_1(1-\rho_2)\tau - \xi_2'(z_2) - \rho_1\rho_2\phi \end{bmatrix} = \vec{0} \in \mathbb{R}^2.$$

We can express partial derivatives of (z_1^*, z_2^*) with respect to ϕ.[8]

$$\begin{bmatrix} \frac{\partial z_1^*}{\partial \phi} \\ \frac{\partial z_2^*}{\partial \phi} \end{bmatrix} = - \begin{bmatrix} \frac{\rho_1}{\xi_1''} \\ \frac{\rho_1\rho_2}{\xi_2''} \end{bmatrix}, \tag{1}$$

with their sum reflecting the change in total underreporting in this framework. This simple setup reveals an important asymmetry in the response of each sheltering type to the

[7] z_1^* and z_2^* represent a utility-maximizing allocation if they satisfy the second-order conditions:

$$\begin{aligned} &\xi_1''(z_1)\xi_2''(z_2) + (1-\rho_1)\xi_1''(z_1)T''(y-z_1-z_2) + \rho_1(1-\rho_2)\xi_1''(z_1)T''(y-z_2) + \rho_1\rho_2\xi_2''(z_2)\phi''(\mathcal{Z}) \\ &\quad + (1-\rho_1)\xi_2''(z_2)T''(y-z_1-z_2) + \rho_1(1-\rho_2)^2T''(y-z_1-z_2)T''(y-z_2) \\ &\quad + \rho_1(1-\rho_2)\phi_{z_1}''(z_1) + \rho_1(1-\rho_1)(1-\rho_2)T''(y-z_1-z_2)\phi_{z_1}''(z_1) \\ &\quad + \rho_1^2(1-\rho_2)^2T''(y-z_1-z_2)\phi_{z_1}''(z_1) + \rho_1(1-\rho_2)\rho_2\phi_{z_1}''(z_1)\phi''(\mathcal{Z}) \\ &\quad + \rho_1\rho_2\xi_2''(z_2)\phi''(\mathcal{Z}) + \rho_1^2(1-\rho_2)\rho_2T''(y-z_1)T''(y-z_2)\phi_{z_1}''(z_1) \geq 0 \end{aligned}$$

and

$$-\xi_1''(z_1) + (1-\rho_1)T''(y-z_1-z_2) - \rho_1(1-\rho_2)\phi_{z_1}''(z_1) - \rho_1\rho_2\phi''(\mathcal{Z}) < 0.$$

This result follows from the simplifying assumption that $\frac{\partial\phi(\mathcal{Z})}{\partial z_1} = \frac{\partial\phi(\mathcal{Z})}{\partial z_2}$ and $\frac{\partial^2\phi(\mathcal{Z})}{\partial z_1\partial z_2} = \frac{\partial^2\phi(\mathcal{Z})}{\partial z_1^2} = \frac{\partial^2\phi(\mathcal{Z})}{\partial z_2^2}$, i.e. that upon full discovery, the fine is only a function of overall underreporting and does not consider the allocation of unsheltered funds between vehicles. If the tax and penalty schedules are linear or globally convex the second-order conditions are trivially met. Basic shape regularity conditions in this setting otherwise include requirements that the tax and penalty schedules not be *too* concave, potentially providing a lower bound of the second derivative.

[8] In the more general case with non-zero cross-cost effects between underreporting vehicles, this derivative generalizes to $\frac{\rho_1}{\xi_{11}\xi_{22}-\xi_{12}^2} \cdot \begin{bmatrix} -\xi_{22} + \rho_2\xi_{12} \\ \xi_{12} - \rho_2\xi_{11} \end{bmatrix}$, where the decrease in underreporting using the risky vehicle may induce a net substitution into underreporting via the harder-to-detect, more expensive vehicle.

penalty. That is, for $\rho_2 < 1$ the penalty induces an attenuated decrease in the usage of a safer vehicle relative to the decrease in the use of the less-safe sheltering vehicle. In the extreme case where the second vehicle is never discovered after an audit, intuitively, the penalty rate has no effect on the optimal sheltering amount with the safer vehicle. Additionally, because the sheltering cost schedules are additively separable between vehicles and here the penalty schedule sees zero curvature, there is no cross-substitution effect where a decrease in the usage of the first vehicle usage increases usage of the second safer vehicle.[9]

3.3 Appeal decision

Some time after paying taxes on the honestly-reported portion of the income, an audit is realized and the tax authorities discover underreported income amount $z \in \{z_1, z_1 + z_2\}$, and are requiring the individual to pay the difference in taxes plus a fine on the underreported amount $T(x + z) - T(x) + \phi(z; x)$.[10]

The taxpayer can either concede this amount or engage in administrative appeal at cost $F + \alpha$, reflecting fixed cost F and variable cost α (e.g. effort or lawyer fees), with probability of failure $r(\alpha, \beta; \theta, z)$.[11] The taxpayer ultimately bases her appeal decision and effort (extensive and intensive margins) to maximize payoff:

$$\begin{cases} \textbf{No appeal:} \ -\Big(T(x+z) - T(x) + \phi(z;x)\Big) \ w.p. \ 1 \\ \textbf{Appeal success:} \ -(F+\alpha), \ w.p. \ 1 - r(\alpha^*, \beta; \theta, z) \\ \textbf{Appeal failure:} \ -\Big(T(x+z) - T(x) + \phi(z;x) + F + \alpha\Big), \ w.p. \ r(\alpha^*, \beta; \theta, z) \end{cases}$$

[9] Generally, high curvature of the penalty schedule attenuates the responsiveness of underreporting, simply because a high curvature allows a small decrease in underreporting to affect a large decrease in penalty. However, this relationship ultimately depends non-trivially on the relative magnitudes of the curvatures of the sheltering cost schedules, the curvature of the penalty schedule, and the discovery probabilities. The opposite intuition holds if the penalty schedule is concave.

[10] We assume no false discovery on part of the audit.

[11] This central specification features legal expenses as not reimbursed by the losing party. Section D.1.1 considers the alternative where the winning party's legal expenses are covered by the losing party.

3.3.1 Intensive margin appeal decision

Conditional on appeal, the taxpayer chooses appeal effort to maximize

$$-F - \alpha - r(\alpha)\Lambda$$

i.e. minimize

$$\alpha + r(\alpha)\Lambda,$$

for the "stakes" of the negative outcomes (from the taxpayer's perspective) $\Lambda := T(x^*+z^*) - T(x^*) + \phi(z^*; x^*)$ and shorthand $r(\alpha) := r(\alpha, \beta; \theta, z)$ for the probability that the taxpayer loses (i.e. that the tax authority wins). Importantly, the taxpayer takes all other arguments in her appeal failure probability, including tax authorities' appeal efforts, as exogenous to her appeal effort. This setup yields a first-order condition and simple elasticity rule for optimal appeal effort:

$$\varepsilon_{r,\alpha^*} = -\frac{\alpha^*}{r(\alpha^*)\Lambda}. \tag{2}$$

At the optimal level of intensive-margin appeal effort, the elasticity of taxpayer appeal success probability equals the ratio of optimal taxpayer appeal expense and the in-expectation payment on the stakes. Alternatively, the semi-elasticity of taxpayer success (or failure) probability in optimality is simply the ratio of intensive-margin expense and the stakes. As a more direct expression of optimal appeal expense,

$$\alpha^* = -\varepsilon_{\tilde{r},\alpha^*}\Lambda^* = -r(\alpha^*)\varepsilon_{r,\alpha^*}\Lambda^*, \tag{3}$$

with taxpayer optimal appeal expense increasing in the elasticity of taxpayer loss probability with respect to taxpayer appeal expenses (where $\frac{\partial r(\alpha)}{\partial \alpha} < 0$ for an interior solution) as well as in alleged tax deficiency, penalty, and tax authority appeal expense. This result here implies that the taxpayer's optimal appeal expense is a linear transformation of the penalty schedule.[12]

Applying the implicit function theorem to the first-order condition allows us to gauge the response of optimal appeal effort as the penalty varies:

$$\frac{\partial \alpha^*}{\partial \phi} = \frac{-\frac{\partial r(\alpha)}{\partial \alpha} z}{\Lambda \frac{\partial^2 r(\alpha)}{\partial \alpha^2}} > 0,$$

given the condition $\frac{\partial^2 r(\alpha)}{\partial \alpha^2} > 0$ (i.e the taxpayer appeal success probability is concavely increasing in appeal effort/expense). This comparative static generates a crucial piece of intuition: that optimal appeal effort/expenses conditional on appeal is increasing in the penalty rate. The government's optimal appeal effort decision follows analogously.

3.3.2 Extensive margin appeal decision

As an expected utility maximizer, the taxpayer decides to litigate if

$$T(x^* + z^*) - T(x^*) + \phi(z^*; x^*) > F + \alpha + r(\alpha^*, \beta; \theta, z^*, x^*)\Big(T(x^* + z^*) - T(x^*) + \phi(z^*; x^*)\Big).$$

$$\implies \big(1 - r(\alpha^*, \beta; \theta, z^*, x^*)\big)\Lambda^* > F + \alpha^*.$$

The intuition underlying the taxpayer's extensive margin decision of whether to litigate is straightforward. All else equal, a greater amount of tax loss and underreporting penalty increase the incentive to litigate. Appeal is disincentivized by greater fixed costs of appeal, greater expected total appeal expenses, and lower probability of appeal success.

3.4 Joint optimization

Section D.1.2 fully develops the joint optimization problem where the taxpayer makes a simultaneous decision over allocation (z, L, α) for underreporting $z \in \mathbb{R}_+$, extensive margin appeal decision $L \in \{0, 1\}$, and intensive margin appeal effort/expense $\alpha \in \mathbb{R}_+$. For brevitiy,

[12]The second-order condition for this interior solution to produce an optimum is:

$$\frac{\partial^2 r(\alpha)}{\partial \alpha^2}|_{\alpha^*} > 0,$$

i.e. the taxpayer sees diminishing returns to her legal expenses in terms of the probability of appeal success.

the central insight yielded by considering the joint decision is that the possibility of litigating serves to lower the probability that the taxpayer will have to pay back any underpayment or penalty, so that the taxpayer compensates by engaging in greater underreporting relative to a no-appeal baseline.

4 Institutional background and data description

Our analysis builds on unusually rich Norwegian administrative data that contains detailed income, wealth, tax and personal characteristics data for the whole population over many years, combined with unique data on audits, tax penalties, appeals, and outcomes. To our knowledge, this is the first time researchers have been accessing this extensive combination of register data and tax enforcement data. However, we are also confronted with institutional and data frictions that may be easily overlooked.

A tax penalty case is not a single observable event. It is the outcome of a longer administrative sequence that may involve selection into control, case handling, revision of the tax base, assignment of a penalty, possible appeal, and eventual collection or reversal. These stages are often recorded in different systems and for different operational purposes, if systematically registered at all. The empirical challenge is then not simply to identify who received a penalty, but to reconstruct, as far as possible, where in the administrative chain that observation comes from and what it leaves unobserved. We had a long and constructive dialogue with the Norwegian Tax Administration to do exactly this, and the process then also revealed substantial improvement points in the internal data and case worker structure that we discuss in more detail in Alstadsæter et al. (2026).

These frictions shape both the scope of our analysis and the choices we make in constructing the dataset. They help explain why parts of the penalty process can be studied sharply, while others can only be approached indirectly or with additional assumptions.

4.1 Tax reporting and penalty administration in Norway

The empirical testing ground for this paper is Norway, which has operated under a dual income tax system since 1992. This system combines a proportional tax rate on capital income with progressive taxation of labor income. The taxation of personal income involves a flat rate on ordinary income (net income after deductions) of 22% and a bracket (step) tax on gross income (primarily labor and pension income) before deductions. Since ordinary income and personal income tax apply together, in addition to social security contributions, the effective marginal tax rate can be substantially higher than 22%. As of 2025, the top marginal tax rate on labor income can reach around 47.4%. Moreover, all taxpayers with a net wealth exceeding NOK 1,760,000 are liable to pay wealth tax to the municipality and state with a combined maximum rate of up to 1.1%.[13]

Every year in March/April, individuals who receive a salary, pension or disability benefit are issued a pre-filled tax return with a summary of their income, deductions, wealth and debt for the previous fiscal year. Individuals are responsible to check the accuracy of the provided information and make amendments to this preliminary statement. The deadline for changing and submitting the tax return is April 30. Following the submission, the Norwegian tax authorities process the returns and send out a final tax assessment notice before December 1. While in the presence of third-party reporting tax noncompliance is expected to be more concentrated along margins where information is harder to verify, such as self-employment or foreign-reported income, Bjørneby et al. (2021) demonstrate the limits of Norway's third-party reporting system and find evidence of collusive tax evasion involving employers and employees. Underreporting or misreporting may trigger penalties and interest on unpaid taxes. To enforce compliance, the Norwegian tax authorities regularly conduct random audits and risk-based audits.

The administration of income tax penalties in Norway is regulated in Section 14 of the Tax Administration Act. Under this provision, taxpayers may be charged with penalties ("*tilleggsskatt*") in cases where they did not provide correct or complete information in their tax returns. Such penalties are only imposed if inaccurate information results in a tax advantage for the taxpayer. A tax advantage typically arises if the taxpayer has paid too little tax, for example if she did not report all of her income or wealth, or entered a deduction or debt to which she is not entitled. Notification of penalties is provided in writing. Today, the ordinary penalty rate amounts to 20% of underreported base. In more

[13]The exchange rate to USD is approximately NOK 10 per USD as of April 2026.

serious cases, the Norwegian tax authorities may levy an increased penalty of up to 40%. Hence, the highest applicable penalty can be as high as 60% of underpaid tax (to be paid back in full along with interest). The elevated penalty rate is applied in cases where the taxpayer provides intentionally or with gross negligence incorrect or incomplete information to the tax authorities. We hereafter refer to the distinction between these two different penalty types as corresponding to the "penalty regime". Tax penalties will not be levied if the incorrect information is due to excusable conditions, such as illness, old age, inexperience or other related reasons. However, there are strict requirements that taxpayers familiarize themselves with the relevant regulations.

Prior to 2016, the ordinary tax penalty rate was levied at 30% of tax that has been or could have been evaded. In 2016, Norway implemented a major reform of its tax penalty system by reducing the standard rate from 30% to 20%. This change occurred in the course of the passage of the Tax Administration Act and was introduced through proposition 38 L (2015-2016) to the Norwegian Parliament (Finansdepartementet (2015)). The Ministry of Finance submitted this proposal on December 11, 2015[14]. The reform aimed to enhance legal certainty, proportionality, and fairness in tax enforcement. Specifically, it sought to simplify the penalty system, increase its predictability, and encourage voluntary compliance. According to the Ministry, the introduction of a more moderate and standardized penalty rate was expected to improve perceptions of legitimacy and fairness in the tax system, thereby promoting long-term compliance behavior. With the introduction of the Tax Administration Act the Norwegian government also introduced a compulsory fine for taxpayers who do not submit their tax returns within the deadline. This compulsory fine is deducted from the tax penalty. In our data environment, we are able to disentangle late filing fees from tax noncompliance penalties. The Tax Administration Act was passed in 2016 and the new penalty rate came into effect on January 1, 2017, applying for the prior year's tax returns to be filed in 2017. Moreover, the new regulations contain a transitional rule, which states that when a first-instance on penalty assignment is applied after January 1, 2017, the legal framework that results in the lowest penalty rate shall be applied. In our data, we observe

[14] https://www.regjeringen.no/no/dokumentarkiv/regjeringen-solberg/aktuelt-regjeringen-solberg/fin/pressemeldinger/2015/forenkler-reglene-for-skatteforvaltning-og-styrker-rettssikkerheten/id2466803/

exactly the income year the issued penalty relates to.

Taxpayers have the right to appeal the tax authorities' decision within six weeks of receiving the notification of penalty assignment. Appeals must be submitted in written format, either electronically or on paper to the tax office, and contain a justification. The appeal is first processed by the tax office, and if the tax office upholds its decision, the case will be forwarded to the Directorate of Taxes or the Tax Appeals Board. If taxpayers disagree with the decision of the tax office or Tax Appeals Board, they can take legal action within six months of the decision being made. As long as a case concerning penalty assignment has not been legally decided, taxpayers are entitled to deferral of claims for payment of the penalty. Part of the reform involved an administrative overhaul of the appeals process, rendering it easier for penalized taxpayers to engage in formal appeal/complaint.

4.2 Data

To study the impact of tax noncompliance penalties on individual behavior, we combine rich register data from Statistics Norway and the Norwegian tax authorities on the universe of income and wealth taxpayers in Norway from 2007 to 2021, including novel data on penalty amount recipiency. Our data environment combines commonly employed administrative tax data (with the benefit of also including data on wealth underlying the administration of the Norwegian wealth tax) with demographic characteristics and additional data indicating whether a taxpayer was assigned a penalty in a given year, whether the penalty was assigned in an ordinary or elevated penalty rate regime, and the penalty amounts assigned under each regime. This newly accessed penalty dataset spans the period 2011-2021, which we complement with audit information that includes audit selection and noncompliance risk scores.

From the shareholder registry, we are able to infer business ownership. We use this along with the population registry to investigate potential spillover effects onto business partners and household members. The currency registry further allows us to trace individuals' international bank transfers, including those to tax haven jurisdictions. We winsorize all monetary variables on the 0.5% and 99.5% levels within year and express all values in terms of NOK 2015.

We construct an individual-year panel of taxpayers and impose perfect balance from 2007 to 2021. We observe around 4.5 million tax returns each year, or around 80% of the 2025 national population.

Individuals in our environment receive penalties for a variety of tax noncompliance infractions. While we do not observe the reason for penalty assignment aside from late payment status, we understand these penalties to reflect instances of underreporting and misreporting. Importantly, these penalties are assigned contemporaneously in response to operational audits: e.g. for a penalty observed in 2015, the penalty was implemented in response to an operational audit of a 2015-2016 tax declaration prior to the 2017 calendar year. We observe whether an individual faced an ordinary or an elevated penalty as well as the associated amount. Tax noncompliant individuals are assigned to the high-rate penalty regime if they deliberately engage in tax fraud or act with gross negligence.

Prior to 2016, the ordinary penalty rate was levied at 30% of underreported base; the elevated penalty was levied at a discretionary amount between 0% and 60%. However, both penalty regimes feature at least some discretion in their application: for instance, we observe around 10% of penalty recipients with "zero-amount" penalties. Additionally, our measures of income and wealth are expressed uncorrected for detected underreporting. When studying the distributional characteristics of tax noncompliance penalties, before constructing within-year income rankings we correct reported income for misreporting by adding to all taxpayers' reported income their penalties divided by the respective penalty rates and marginal tax rates.

There are a number of data limitations that restrict our approach to causal inference. Specifically, we do not observe the precise statutory penalty rate applied (in the case of the presence of caseworker/judge discretion) nor the amount of underreported base or underpaid tax. We also do not observe key metadata around the auditing, penalty assignment, or appeals processes such as caseworker or judge assignment, or other variables that may indicate the presence of decision maker discretion. We also note that random audits are conducted in a highly limited manner in Norway.

The relevant administrative systems from which our tax penalty data are constructed by the Tax Administration were historically designed primarily for case processing and storage, not for longitudinal research on the full life cycle of sanctions. This distinction matters because it implies that some variables are missing not by accident, but because they were never recorded in a form suited for statistical analysis in the first place.

4.3 Descriptive evidence on income tax penalties and appeals

We now turn to producing descriptive statistics on penalties and the statutory distributional incidence of penalties. Little is known—even descriptively—about who receives penalties and how penalties impact overall tax liability and net collections. A distinctive feature of our analysis is the ability to incorporate information on appeals of penalties. This allows us to examine not only the initial imposition of penalties across taxpayers but also to shed light on the characteristics of those who appeal, appeal outcomes, and the interconnection between penalty severity and the appeals process.

The assignment of income tax penalties. Table 1 establishes descriptive statistics on penalty assignment for 2015.[15] Panel (a) provides an overview of penalty characteristics. For tax year 2015 we observe 12,257 penalty assignments in our data, of which 1,349 correspond to elevated penalties. The average (median) penalty amounts to NOK 8,770 (NOK 2,000), with a small mass of penalties at zero wherein taxpayers are only required to pay back underpaid taxes plus interest. Around 4% of penalties administered are appealed by taxpayers (we explore descriptive statistics around appeals and appeals outcomes in Section 6).[16] We can also observe that penalties are quite infrequently administered and make up a small share of revenues: 0.25% of taxpayers receive a penalty in a given year, forming 0.02% of Norway's gross annual income tax revenues.

[15]Table A.1 provides analogous descriptive statistics for pooled observations from 2011 to 2021.
[16]Table A.3 provides summary statistics for penalties disaggregated by ordinary/elevated rate status.

Table 1: Descriptive statistics of tax noncompliance penalties
Panel (a): Characteristics of penalties (2015)

	Mean	SD	p10	p50	p90	N
Penalty amount	8,770	21,830	0	2,000	23,000	12,257
Penalty amount (log)	8.93	1.18	7.6	8.85	10.4	6,911
Positive penalty amount (binary)	.56	.5	0	1	1	12,257
Penalty share of income	.03	.08	0	.0038	.08	11,051
Penalty appeal	.04	.19	0	0	0	12,257
Any penalty count	12,303					
Elevated penalty count	1,349					
Penalized population share	.0027					
Penalized share of income tax receipts	.0048					
Penalty share of income tax receipts	.0002					

Panel (b): Penalty recipient and non-recipient characteristics

Dependent variable	Non-penalized	Penalized	Difference	Difference (with audit controls)
Total corrected income	454,077	746,495	292,418	199,899
	[885,256]	[1,793,016]	(16,972)	(16,987)
Log total corrected income	12.7	12.8	.17	.05
	[1.25]	[1.83]	(.02)	(.02)
Any self-employment income	.07	.35	.28	.21
	[.26]	[.48]	(.0043)	(.0043)
Any capital income	.88	.88	-.0016	-.0124
	[.33]	[.33]	(.003)	(.0029)
Log gross wealth	13.1	12.6	-.501	-.674
	[2.61]	[3.36]	(.03)	(.03)
Log net taxable wealth	12.2	12.8	.58	.23
	[2.51]	[2.77]	(.04)	(.04)
Has a majority business ownership stake	.03	.24	.21	.19
	[.18]	[.43]	(.0039)	(.0039)
Any transfer abroad	.18	.38	.2	.14
	[.39]	[.48]	(.0044)	(.0044)
Any transfer with tax havens	.02	.04	.03	.02
	[.13]	[.21]	(.0019)	(.0019)
Age	47.8	44.2	-3.53	-3.14
	[18.8]	[12.3]	(.12)	(.12)
Immigrant	.15	.34	.18	.16
	[.36]	[.47]	(.0045)	(.0045)
Female	.5	.19	-.31	-.264
	[.5]	[.39]	(.0037)	(.0037)
Married	.43	.42	-.0114	-.02
	[.5]	[.49]	(.0047)	(.0047)
University	.33	.29	-.0427	-.0696
	[.47]	[.45]	(.0044)	(.0044)
Unique individuals	4,532,780	12,257		

Note: This table displays descriptive statistics of 2015 tax noncompliance penalties in Panel (a) and the attributes of penalty (non-)recipients in Panel (b). In Panel (a), some taxpayers receive both ordinary and elevated penalties. The “Non-penalized” and “Penalized” columns in Panel (b) present means and standard deviations (in hard parentheses) of taxpayer characteristics stratified by penalty recipiency status for a given individual-year (where a given taxpayer can appear in both columns in separate years). The “Difference” column presents the coefficients of a series of univariate regressions of each row variable on an indicator of penalty assignment; The coefficients in Column (4) result from regressions that also control for audit flag status. Soft parentheses contain heteroskedasticity-robust standard errors.

Panel (b) focuses on the attributes of taxpayers, stratifying by whether they received any tax noncompliance penalty. The table suggests notable differences between penalty recipients in terms of gender, age, education, and immigrant status. Implicated taxpayers are on average more likely to be male, younger, less educated, and more likely to have an immigration background. Moreover, significant differences emerge for economic status. Specifically, penalty recipients have higher incomes, income tax liabilities, exhibit a significantly higher probability of claiming self-employment income (26pp relative to a non-penalized baseline of 7%) or capital income (2pp relative to a non-penalized baseline of 87%), and are more likely to engage in money transfers abroad and with tax haven jurisdictions in a given year. Penalty recipients are also eight times as likely to own a business. Many of these differences persist strongly after controlling for audit status.

Table A.4 explores heterogeneity in penalty size among penalized taxpayers along "intensive" and "extensive" margins. The relationship between penalties and taxpayer/return characteristics vary substantially between these two margins. On the intensive margin, penalty size increases in income, as well as in the presence of self-employment income, taxable wealth, making international bank transfers, with most of these relationships persisting after controlling for log income and gross wealth.[17] Older and male taxpayers face somewhat higher penalties, while immigrants receive lower amounts. Marital status and education appear to be negatively correlated with the severity of penalties. Many of these relationships change sign when examining the extensive margin. Positive penalty recipients compared to zero-amount penalty recipients are less wealthy and exhibit less economic activity in the form of business ownership and transfers abroad. Additionally, there appears a non-linearity in the relationship between extensive-margin penalty receipt and income, where in logarithms, non-zero penalty recipients have about 80% more income than zero-penalty recipients, where the relationship is statistically insignificant (and of opposite signs) in levels income.

Figure 1 elaborates on these descriptive results, showing the frequency of penalty assignment along income. Panel (a) depicts a level of progressivity for the assignment of tax penalties on the extensive margin.[18] In a given year, nearly 1.5% of the top 1% of earners

[17]Figure A.6 displays the distribution of log penalty amount along conditional on penalty.

[18]Figure A.1 shows that this relationship persists after restricting to the flagged population (Panel (b)).

receive a penalty. Around 3% of earners in the top 0.01% receive a penalty. These insights are not driven by the fact that high-income earners are audited more frequently, persisting after restricting the sample to audited taxpayers only (Panel (b)).

Panel (b) plots the evolution in effective tax and penalty rates among Norwegian taxpayers based on income rank. We calculate the effective penalty rate as the penalty amount divided by gross income plus imputed income correction, excluding negative values and, in the figure, values in excess of one.[19] Among both penalized and non-penalized taxpayers, effective income tax rates rise with income, reflecting the de facto progressivity of the Norwegian tax system. However, we observe a mild decline in the effective income tax rate among top earners, corroborating evidence in other high-income countries (Piketty et al. (2017)). We also observe a steeper effective income tax rate gradient at lower earnings levels for penalized individuals than non-penalized individuals. In spite of the observed progressivity of the income tax, we observe a net regressivity in penalty administration, with effective penalty rates decreasing from around 6% at the bottom of the income distribution to near zero within the population of penalized taxpayers. Of course, this fact may be attributed to underreporting amounts also decreasing as a share of income among penalized individuals along income rank.[20][21]

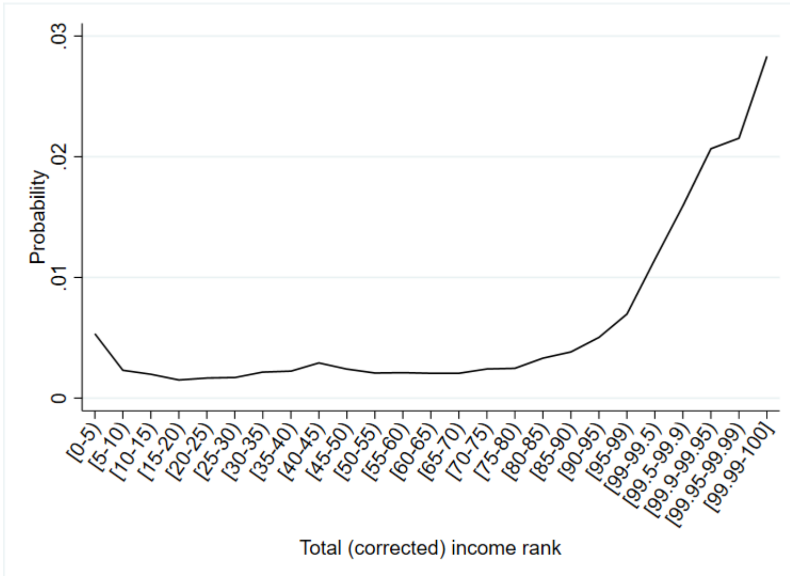
Appeals. Our unique data environment also allows us to provide a detailed picture of the characteristics of appeals, and their relationship with the degree of severity of tax penalties, and the outcomes of those appeals. Our main data insight into the appeals process consists of a data set of all penalty appeals in the past ten years. Table C.1 gives descriptive statistics on outcomes for the 2.5% of penalties that saw administrative complaint/appeal between 2011 and 2021. Among appealed penalties, around three quarters have some closing, the overwhelming majority of which (overall 64% of appeals) result in some positive collection by the tax authorities. Appeals are made on rulings pertaining to tax years several years in the past, likely due to the combined length of time for audit plus the time delay that is

[19]The effective tax rate figures in the exhibit do not correct for underpaid taxes, which we do not directly observe.

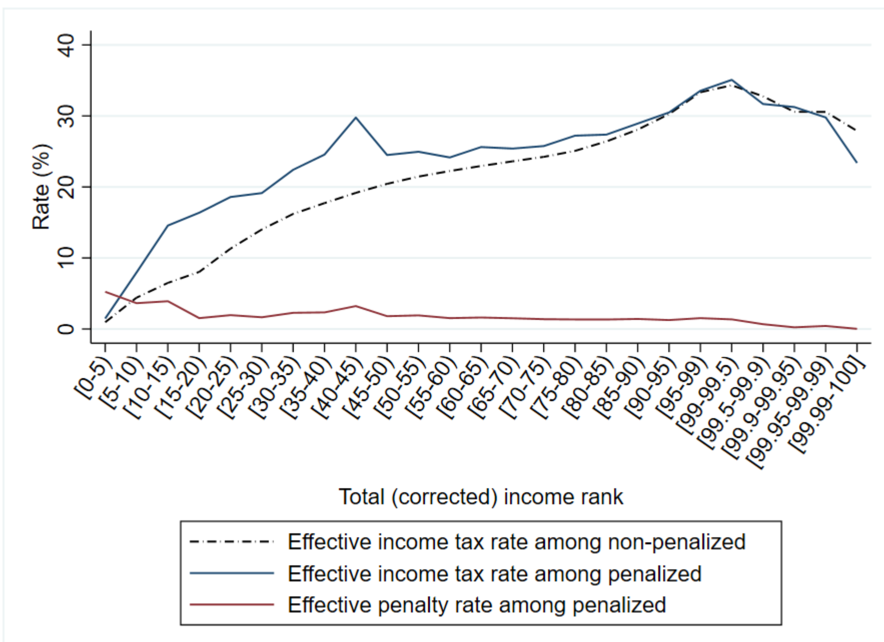
[20]Figure A.2 replicates this figure, ranking taxpayers by gross wealth rank, resulting in similar findings.

[21]Figure A.3 - Figure A.9 show additional descriptive findings pertaining to the distributional characteristics of penalties, including effective rates, aggregate penalties, imputed income corrections, and penalties per capita by income rank, as well as the number of penalties per taxpayer in our time frame.

Figure 1: Distributional characteristics of penalty receipt and effective penalty rates (2015)



Panel (a): Penalty assignment v. income rank



Panel (b): Effective income tax rates and penalty rates by income rank

Note: These figures plot the distributional characteristics of penalty recipiency by income rank for taxpayer-years in 2015. Income ranks are calculated following correction for detected underreporting. Effective penalty rates refers to penalty amount divided by positive total implied corrected income, with observations with effective penalty rates in excess of one excluded.

standard for tax filing.

Conditional on a case closing, 78% of appeals result in no penalty removal, with 10% and 12% resulting in partial and full removal respectively. We can observe that conditional on *some* penalty removal, around 64% of the penalty is removed. Lastly, the table reports that on average, the appeals process takes 2.5 months; however, many appeals are observed as resolved instantly, so that conditional on non-zero processing time, appeals can take on average more than half a year. This said, we observe wide variation in resolution time.

Table 2 reports the average outcomes associated with appeals and Table C.1 reports balance on taxpayer covariates for taxpayers that appeal versus do not appeal penalties received during the respectively implicated tax years. Appellants have significantly higher income and wealth, are more likely to claim capital income and transfer money to tax haven jurisdictions, are older, and are more likely to have completed university. Figure 2 Panel (a) shows how appeal probability increases approximately monotonically in penalty size, even after controlling for taxpayer income and wealth, as well as year fixed effects. Two interpretations of these results may be that 1) higher earning taxpayers view their penalties as more worthy of appeal or that 2) higher earning taxpayers have greater resources to engage in administrative appeal. Figure 2 Panel (b) illustrates the distributional pattern of appeals across the income spectrum, conditional on having received a penalty. The incidence of appeals follows an asymmetric U-shaped pattern: compared to middle-income earners, individuals situated at the lower end of the income distribution exhibit a higher likelihood of appealing. We further observe a pronounced right-tail of the distribution, showcasing that the likelihood to appeal rises sharply at the top of the income distribution. This pattern suggests that both resource availability and perceived returns to contesting may shape the decision to appeal, with affluent taxpayers potentially leveraging greater financial and informational resources, while lower-income individuals may appeal as a defensive strategy when faced with additional burden.

These descriptive statistics provide a baseline perspective on why a tax authority would view appeals as costly and desirable to avoid, from a purely revenue-maximization perspective: appeals decrease the probability of collection, increase the time-to-collect (which would require greater time discounting), and increase direct expenses to resolve these appeals (e.g.

Table 2: Summary statistics on appeals outcomes

Variable	Mean	SD	N
All appeals			
Has a closing	.735	.441	3,562
Collected	.636	.481	3,562
Years from initial tax year, at opening	3.47	2.10	3,562
Conditional on closing (and positive initial penalty)			
No penalty removal	.778	.416	1,610
Penalty is only partially removed	.096	.295	1,610
Penalty is fully removed	.122	.328	1,610
Share removed (conditional on some removal)	.639	.449	357
Process time (years)	.203	.663	2,618
Process time (years), conditional non-zero	.530	.989	1,001

Note: This table displays descriptive statistics on appeals outcomes, separately for all appeals between 2011 and 2021 and for those appeals for which a closing date is observed.

litigation, allocating tax law or collection specialist time). Moreover, Table C.2 uses OLS regressions to show that these cost indicators correlate positively with penalty size and effective rate. For instance, doubling the penalty size increases the time-to-resolution by 17% and decreases the probability that the case is observed to have a closing by 14pp.[22] Table C.4 demonstrates that the positive relationship between penalty size and appeal probability persists even after directly controlling for the noncompliance risk score assigned to each tax return. These empirical results corroborate some of the insights from the conceptual model in the previous section.

[22]Figure 6 shows these relationships graphically.

5 The impact of the penalty rate on ex-post income reporting behavior

5.1 Quasi-experiment: The 2016 reform to the statutory penalty rate

To estimate the causal impacts of changing the penalty rate, we exploit a legislative reform to the statutory penalty rate. Prior to 2016, individuals penalized under the standard ordinary penalty rate regime faced a statutory 30% fee on the underreported base, to be paid back alongside the underpaid tax obligation in full and interest. On December 11, 2015, the Norwegian government announced to lower the statutory penalty under this regime to 20%, constituting a 33.3% relative decrease in the statutory penalty rate.

We leverage the timing of penalty recipiency before and after this reform to estimate the differential evolution in reporting behavior for around penalty recipiency for ordinary penalty rate recipients. We restrict ourselves to examining ordinary penalty recipients and we define taxpayers' penalty events as the year of their earliest observed penalty between 2011 and 2021. We implement this definition in order to avoid mechanically biasing our estimates by conditioning on ex-post compliance or noncompliance behavior, although Figure A.5 shows that a vast majority of penalized taxpayers are observed only ever receiving a single penalty.[23] Additionally, the year we observe as the penalty year represents the year in which the taxpayer received a penalty, typically pertaining to an instance of observed tax noncompliance in the tax year immediately prior, although the tax authorities can inspect tax returns for up to three prior fiscal years.

Our approach to estimating the effect of the change in penalty rate on taxpayer's ex-post income reporting behavior involves comparing the evolution in taxpayer outcomes around penalty recipiency for taxpayers penalized in the 30% statutory rate pre-reform regime against those penalized under the 20% statutory rate post-reform regime in a "difference-in-differences event study" fashion. In particular, treatment (penalty receipt) can be viewed as

[23] We do not duplicate individuals receiving multiple penalties to study their subsequent penalty events. We also drop individuals penalized starting 2019 from our comparison, as we observe additional changes in penalty severity beginning with this year.

staggered for both groups, but with the post-2016 cohorts exposed to a lower penalty rate than the pre-reform cohorts.

Equation (4) shows the reduced form of our main estimating equation. The first sum itself only measures the effect associated with event time (i.e. detection), which we typically view as invalid for inference purposes, as penalty receipt occurs in a non-random manner. The second summed term isolates the differential effect of the post-reform penalty recipients that face the 10pp lower statutory penalty rate. We use observations within five years of penalty recipiency. Intuitively, this design aims to net-out the non-random effects of extensive-margin penalty recipiency, attributing any difference in post-penalty outcomes between pre- and post-reform cohorts to the effect of the change in the penalty rate.

$$y_{it} = \alpha_i + \delta_t + \underbrace{\sum_{k=-5}^{5} \gamma_k EventTime_{k(it)}}_{\text{Extensive-margin penalty effect}} + \underbrace{\sum_{k=-5}^{5} \beta_k Event\ Time_{k(it)} \cdot 1\{Penalty\ Year_i \geq 2016\}}_{\text{Intensity effect}} + \varepsilon_{it}. \tag{4}$$

Our main outcomes of interest for taxpayer i in year t, y_{it}, are reported (not corrected) total income and income tax payments. We additionally estimate this equation for a broader set of variables, including disaggregations of income, wealth, deductions, and certain "quality-of-life" events, such as change of residence and divestment of business ownership. α_i and δ_t in Equation 4 denote taxpayer- and regime status $\times$ year fixed effects, respectively. We cluster standard errors at the taxpayer level.

5.1.1 Internal Validity

We are interested in estimating an effect β of a change in the penalty rate on taxpayer ex-post dependent variable y. The validity of our estimation strategy requires a set of assumptions. First, a parallel trends assumption requires that $\beta_j \in \{\hat{\beta}_k\}_{k<0} = 0$: that prior to penalty recipiency, outcomes for both groups of penalized taxpayers evolve identically. We can validate this assumption empirically. When this assumption holds, $\{\hat{\beta}_k\}$ identify a treatment effect on the post-reform treated cohorts, relative to the pre-reform penalty cohorts.

Under a homogeneous treatment effects assumption and a parallel trends assumption, this treatment effect represents an average treatment effect of the penalty rate decrease for the population of penalized taxpayers. However, under a heterogeneous potential outcomes framework, we may risk misattributing observed post-penalty differences to the effect of the penalty rate change instead of to an effect of changing composition of penalized cohorts. Indeed, Table 3 illustrates some important differences in observable characteristics between pre- and post-reform penalized taxpayers. For instance, penalized taxpayers in the post-reform regime earn approximately 20% more than those in pre-reform regime. Figure A.3 also shows a marked decrease in penalty assignment beginning in 2016. Although this decrease may partly mechanically reflect the time it takes ex-post to conduct audits and assign penalties, these differences may prompt concern about compositional changes in penalized individuals and heterogeneous treatment effects. It is difficult to empirically determine heterogeneous potential outcomes between these two groups. To address such concerns, Section 5.3 expands on the robustness of our results by estimating models using different models that attempt to control for these differences via matching approaches and correlated random effects models that attempt to directly control for differences in observable pre-penalty characteristics, as well as via testing spillover effects onto business and family peers that would rule out versions of heterogeneous treatment effect models.

Additionally, the use of unit and time fixed effects in difference-in-differences design with staggered treatment ("event studies") similar to as in Equation (4) introduces bias from heterogeneous treatment effects (Borusyak et al. (2024) and Callaway and Sant'Anna (2021)). For this reason, we typically estimate this equation either using an approach from Callaway and Sant'Anna (2021) that aggregates treatment effects from "cleaner" subgroups comparisons of treated and non-yet-treated units, or in the case of instrumental variables regression, estimate forms of this equation while omitting year fixed effects.

Because we study the differential response of penalized individuals, this design can only estimate agents' ex-post responses to penalties, as opposed to the deterrence response induced by the penalty rate. While in a perfect-information environment, ex-post and deterrence effects coincide (as there are no "learning" or "experience" effects), empirical evidence suggests widespread misperception in penalty rates that lead to information updating occurring after salient tax events (such as penalty receipt). Relatedly, central to the validity

Table 3: Pre-penalty descriptive statistics of ordinary-rate penalty recipients
Cohorts pre- and post- statutory penalty-rate decrease

Dependent variable	Pre-reform	Post-reform	Difference
Log corrected income	12.8	13	.24
	[1.09]	1.11	(.01)
Log penalty size	8.87	8.66	-.205
	[1.16]	1.08	(.01)
Effective penalty rate	.04	.03	-.0077
	[.04]	.04	(.0004)
Has self-employment income	.26	.2	-.0604
	[.39]	.35	(.0039)
Has capital income	.89	.91	.03
	[.29]	.26	(.0029)
Log gross wealth	12.1	13.1	1.06
	[2.98]	2.95	(.03)
Any transfer abroad	.28	.34	.06
	[.36]	.38	(.0041)
Any transfer with tax haven	.03	.04	.02
	[.12]	.14	(.0015)
Has majority business ownership stake	.16	.31	.16
	[.34]	.43	(.0045)
Age	40.8	42.4	1.57
	[12.6]	12.8	(.14)
Immigrant	.33	.3	-.0285
	[.47]	.46	(.01)
Female	.21	.21	.0006
	[.41]	.41	(.0045)
Married	.4	.44	.04
	[.46]	.47	(.01)
University	.25	.32	.07
	[.43]	.47	(.01)
	[16,773,402]	44,967,440	(441,266)
Unique taxpayers	42,916	10,862	

Note: This table displays four-to-one year pre-event average descriptive statistics of Norwegian ordinary-rate personal income tax noncompliance penalty recipients based on whether they received their penalty under the pre-reform 30% statutory rate or the post-reform 20% statutory rate. Hard brackets indicate standard deviations; soft brackets indicate heteroskedasticity-robust standard errors. All monetary values are expressed in real NOK 2015.

of this design is a behavioral assumption that taxpayer's knowledge of the penalty rate is informed most saliently by their own receipt of the penalty rate. I.e. we implicitly treat a taxpayer penalized under the more stringent 30% pre-reform regime as still "exposed" to the

30% regime, even after the statutory rate changed to 20%. We view this assumption as likely valid. For instance, while (Bergolo et al. (2023) show that firms have on aggregate unbiased beliefs over penalty rates, the authors also provide evidence for both 1) widespread variation in (incorrect) beliefs across firms and 2) lack of updating incorrect beliefs on penalties in response to salient enforcement signals, such as audit threat letters. Alternatively, interpreted in a less restrictive manner, our approach simply estimates the difference in ex-post behavior of taxpayers penalized post- v. pre-reform.

5.2 Results

Figure 3 Panels (a) and (b) show the intuition and baseline empirical validation for our approach. The figures use a two way fixed effect (TWFE) estimator from Callaway and Sant'Anna (2021) to plot the effects of penalty receipt for both pre- and post-reform penalized for log *reported* income and taxes paid. We observe parallel trends in outcomes between groups prior to penalty receipt, subsequent to which our approach attributes differential response to the change in penalty rate. In both cases, we observe a difference emerge of around 10%. Namely, compared to the change for taxpayers penalized under the 30% regime, taxpayers penalized under the 20% regime declare around 10% less income and pay around 10% less income tax post-penalty.

The results suggest a 8-12% decrease in reported income and taxes paid among the group subject to the lower 20% statutory penalty rate. Contextualizing this response within the relative change in the statutory rate of $-1/3$, we estimate a behavioral elasticity of reported income and taxes paid ex-post to the statutory penalty rate of between 0.15 and 0.3.

Table 4 displays estimates for our designs. Columns (1) and (5) give reduced form estimates of the impact of the reform; the remaining columns use the reform as an instrument for estimating different parameterizations of penalty severity.[24][25] Figure 4 shows the event

[24]Table B.1 reports the first-stage estimates in which the penalty rate reform serves as instrument for taxpayers' effective penalty rates. Consistent with the institutional design of the reform, we document a significant decrease in the effective penalty burden of affected individuals. The strong predictive power is supported by F statistics that by far exceed conventional thresholds for instrument relevance across all specifications for penalty size. The magnitude of the relationship is particularly pronounced during the 2016-2018 period, when the reform was most salient.

[25]In order to mitigate the threat of bias from using two way fixed effect estimators in staggered treatment settings with heterogeneous treatment effects, the table uses estimates from models that only include

study estimates corresponding with the impact of log penalty size and effective rate on ex-post income declaration and income tax payment. A 10% increase in penalty size induces a 4.2% increase in declared income and 4.6% increase in taxes paid, thereby corresponding with slightly greater elasticities than implied by the reduced form results of 0.42 and 0.46 respectively. Columns (3) and (7) give the respective impacts of a 1pp increase in *effective* penalty rate: a 1pp increase in effective penalty rate increases income declared and taxes paid by 4.9% and 6.2% respectively. We find similar increases parameterizing the change in penalty as log effective penalty rate. Lastly, parameterizing the penalty in the form of log effective penalty rate gives a useful interpretation as the *relative* change in the penalty rate, wherein we find that a 10% increase in the penalty rate (locally a 2-3pp increase) induces an increase in reported income and taxes paid of around 2% per year.[26]

Table 4: IV event study difference-in-differences:
Impact of penalty rate on declared income and taxes paid (penalty rate reform IV)

	Log income				Log taxes paid			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform × Post-event	-0.067				-0.081			
	(0.012)				(0.0092)			
Log penalty × Post-event		0.42				0.46		
		(0.069)				(0.056)		
Effective penalty rate × Post-event			0.11				0.14	
			(0.022)				(0.018)	
Log effective penalty rate × Post-event				0.20				0.22
				(0.031)				(0.023)
Fixed effects	ID	ID	ID	ID	ID	ID	ID	ID
Specification	Reduced form	IV: reform	IV: reform	IV: reform	Reduced form	IV: reform	IV: reform	IV: reform
Observations	423,293	384,956	418,842	380,905	396,080	360,167	392,034	356,454
Number of clusters	50,508	45,916	49,420	44,948	50,011	45,483	49,024	44,601
F-statistic	129	131	118	151	728	510	589	685

Note: This table displays results from a series of instrumental variable difference-in-differences events study regressions corresponding with the reduced form $y_{it} = \alpha_i + \gamma PostPenaltyEvent_{it} + \beta PostReform_i \cdot PostPenaltyEvent_{it} + \varepsilon_{it}$. In each instrumental variables regression, the penalty rate reform serves as an instrument for the change in the respective parameterization of the penalty rate for taxpayers penalized starting in 2016. Effective penalty rate is defined as penalty divided by total corrected income in the year of penalty. All specifications include taxpayer fixed effects. Regression constants and Post-event coefficients omitted for legibility. Standard errors clustered on the taxpayer level are included in parentheses.

Section 5.2 breaks down the overall response by income type, distinguishing between deductions, salary income, self-employment income, and capital income. We observe an increase in deductions claimed (also seen in Figure B.2 Panel (b)). In addition, we find

individual fixed effects.

[26]Figure B.4 and Section B estimate these designs using stratified two way fixed effect designs that also include penalty regime status × year fixed effects. While these designs may be more susceptible to bias via heterogeneous treatment effects, they yield nearly identical results.

decreases in reported income attributable to sources that are possibly more susceptible to misreporting such as self-employment (Kleven et al. (2011)). We also document a decrease in wage income, although we cannot attribute this response to an increase in wage collusion or a decrease in real labor supply (Bjørneby et al. (2021)). We observe nearly zero change in capital income or loss declaration patterns.

Table 5: Heterogeneous responses of income types and deductions (reduced form)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log deductions	Log wage income	Has income from self-employment	Log income from self-employment	Has positive capital income	Log capital income	Has negative capital income	Log capital loss
Post-reform × Post-event	0.064	-0.066	-0.014	-0.074	-0.026	0.0041	0.010	0.16
	(0.011)	(0.011)	(0.0030)	(0.030)	(0.0024)	(0.024)	(0.0014)	(0.13)
Constant	11.5	12.5	0.22	12.1	0.87	6.57	0.028	10.5
	(0.0038)	(0.0026)	(0.00074)	(0.0054)	(0.00055)	(0.0050)	(0.00029)	(0.030)
Observations	332,413	334,627	464,005	96,686	464,005	402,598	464,005	7,516
Number of clusters	51,034	45,654	53,686	19,196	53,686	50,063	53,686	2,765
F-statistic	109	19	29	13	119	58	38	1.2
Adjusted R-squared	.57	.53	.52	.55	.49	.72	.13	.56

Note: This table displays results from a series of difference-in-differences event study regressions corresponding with the reduced form $y_{it} = \alpha_i + \sum_{k=-5}^{5} \gamma_k EventTime_{k(it)} + \sum_{k=-5}^{5} \beta_k \theta_i EventTime_{k(it)} + \varepsilon_{it}$, determining the impact of the penalty rate reform on different income types and deductions by comparing pre- and post reform penalty recipients before and after receiving their penalties. All specifications include taxpayer fixed effects. Standard errors clustered on the taxpayer level are included in parentheses.

5.3 Further robustness tests and extensions

A central concern regarding the robustness of our results from the quasi-experimental design pertains to heterogeneous treatment effects and differential sample composition between those penalized before and after the statutory penalty rate decrease from 30% to 20% in 2016. Indeed, we observe that those penalized following the change report significantly more earnings and wealth than those penalized before the reform. Heterogeneous treatment effects may correlate systematically with these observable characteristics. For instance, higher earners may demonstrate greater sensitivity to the ex-post penalty rate, thus biasing estimates greater in magnitude.

Matching and Correlated Random Effects. In one manner of addressing this concern, we attempt to control for these differences by estimating a series of correlated random effects designs and implementing a matching procedure and so as to align characteristics of penalty recipients subject to the different penalty rates.

First we estimate analogues of Equation (4) omitting taxpayer fixed effects while directly controlling for within-taxpayer time-invariant characteristics that could account for differential sample composition between penalty regimes. Table B.4 summarizes the results for a series of such designs, controlling for permutations of the assigned penalty size, pre-event declared income, and pre-event declared wealth. Reassuringly, we obtain nearly identical results across specifications in line with our preferred estimates of a relative decline in both declared income and total taxes paid by about 7.5% and 11% respectively. In particular, controlling directly for penalty size (and penalty size in combination with income and wealth) allows us to directly compare like individuals between penalty regimes, at least based on the severity of their penalties. Figure B.5 shows the event study estimates. Both of these outcomes demonstrate stability over time, although the effect size for total income mildly attenuates after 4 years.

We expand on our correlated random effects model analysis by controlling for audit risk score. Every year, the tax authorities assign tax returns a risk score based on their perceived likelihood that a declaration features some tax noncompliance, which determines the audit selection process. We parameterize audit score in two ways: first as the log score, and second, categorically based on within-year score rank (percentiles of the within-year distribution and within the top 1% of this distribution. This second parameterization, using within-year ranks, also guards against bias from shifts in the risk score assignment process over time.Table B.5 and Table B.6 display the output from these models. In these tables, the odd-numbered columns control for log risk score, and the even-numbered columns control for within-year rank. We observe a nearly identical 6-7% relative decline in income declared and 10% decline in income taxes paid by post-reform cohorts of penalized individuals. Controlling directly for risk score further addresses concerns that our results reflect changes in the composition of penalized taxpayers combined with heterogeneous treatment effects.

Lastly, our Mahalanobis matching procedure maps each individual penalized starting 2016 with five individuals penalized between 2011 and 2015; individuals are matched based on the reported income and wealth for four years prior to penalty and the matches are constructed with replacement among the pre-reform sample. Table B.3 displays the balance of the matching procedure. Compared to Table 3, Table B.3 performs well in aligning observable characteristics of the two groups, namely income (as expected), but also penalty size and effective penalty rate. The two groups still exhibit some precisely estimated, yet substantially attenuated differences. For instance, post-reform taxpayers declared around 10% more wealth in pre-penalty years. In addition, the matched samples do not see significant differences in age, gender, or marital status. Overall, we view this design as alleviating some concern over the differential selection between the samples.

Figure B.6 displays the results of estimating our central specification on the matched sample. We document largely similar results, albeit more attenuated. For this matched sample, both income declared and taxes paid decrease by around 5% among the post-reform group relative to the matched pre-reform group beginning one year post penalty. However, this difference completely attenuates four years after the event. Table B.7 summarizes the findings for this design.

From this exercise, we conclude that our estimated response estimate is not attributable to differences in potential outcomes between the two penalty cohorts as predicted systematically based on differences in penalty severity, income, or wealth, Holding these characteristics fixed, we find results in line with those of our main specification.

Spillover effects. A recent literature has evolved studying spillover effects of tax enforcement that can arise from information sharing and joint tax preparer networks (Battaglini et al. (2019), Boning et al. (2020)). The richness of the Norwegian data allow us to analyze peer effects in the context of tax noncompliance penalties along two dimensions. First, we examine the effects on non-implicated individuals that share the same household identifier as the penalty recipients. Second, we leverage information from the shareholder registry to analyze spillovers to business partners that hold at least 10% ownership shares in the same company. An advantage of analyzing spillover effects lies in the potential that peers may not suffer from non-comparability that we observed for post- vs. pre-reform penalized taxpayers (spouses, adult children, housemates, and parents). Table B.8 displays the results from the quasi-experimental design for household peers (Panel (a)) and business partners (Panel (b)) of post-reform penalty recipients relative to those of pre-reform penalty recipients. We document a relative decline in declared income and income tax payments among both household- and business peers of taxpayers largely identical to our main estimates. We interpret these results as both validating the fiscal spillovers of tax enforcement measures and substantiating the validity of our main approach because we view as less likely that the possibility the heterogeneous potential outcomes of pre- and post-regime penalty would extend to their peers.

"Welfarist" outcomes. Lastly, we turn to studying a set of "quality-of-life"-related outcomes. A welfarist position against higher penalties could argue that penalties induce negative effects on well-being or utility, such as disruptions to financial stability, solvency-related change in residence, or the loss of business assets. If larger penalties induce negative effects on individuals' quality-of-life outcomes not directly related to payment of the penalty, we should observe a relative improvement in these measures for the group of taxpayers penalized under the 20% statutory penalty rate regime over those facing the 30% penalty rate. Figure B.3 plots the differential evolution of these outcomes for the two groups. We find no compelling differential effect of the decrease in the penalty rate on the occurrence of bankruptcy. However, we do find a relative decrease in the probability of loss of majority ownership in a business, as well as a relative increase in the probability of changing residence across municipality, which may be interpreted as a negative impact of greater penalties if penalties inhibit voluntary moves. This result showcases that targeted enforcement strategies, while designed to improve compliance, may have the potential to generate unintended welfare costs that extend beyond tax collection matters.

6 The impact of the penalty rate on appeal behavior

Having established estimates of the impact of the penalty rate on direct tax collections, we now turn to estimating some of the real administrative costs of penalties. Insofar as appealed penalties require additional time and labor resources to resolve and penalty size correlates positively with penalty appeal probability, we view appeals as potentially constituting a central administrative cost associated with the penalty rate. To this end, here we focus primarily on estimating the causal impact of changes in tax noncompliance penalties on a taxpayer's likelihood of appealing an assigned penalty.

We develop two main avenues for estimating the causal impact of the penalty rate on appeal probability: 1) a difference-in-differences design that exploits the same 2016 reform, and 2) a strategy that exploits idiosyncratic variation in the penalty rate within year and across ordinary and elevated regimes. Both of these designs see important limitations, for which reason we view them as complementary in ascertaining this causal relationship.

6.1 Estimating the appeals response via difference-in-differences

While our difference-in-differences event study design exploiting the 2016 decrease in the statutory penalty rate as a quasi-experiment is well-suited for analyzing ex-post income reporting and tax payment, this framework faces conceptual and empirical limitations in the appeals setting. First, penalty appeal is case-specific and does not evolve dynamically as do other annual outcomes: either a case is appealed or it is not. This first feature is somewhat innocuous. Ideally, given that the ordinary penalty rate saw a decrease and the elevated penalty rate saw no change, we would estimate a difference-in-differences design that compares the appeal probability for penalties under the two regimes around the statutory rate change, treating the penalty cases as a pooled cross section (as opposed to as a taxpayer-year panel).

However, another central challenge lies in a likely exclusion violation of the penalty rate reform on appeals, in which taxpayer responses may be affected through channels other than the reform itself. Specifically, the 2016 change in the penalty regime may indeed have influenced appeals not only by lowering the penalty rate, but was also accompanied by a reform to the appeals process that made it easier for penalized taxpayers to engage in administrative complaint. Estimating a difference-in-differences design that exploits the 2016 reform risks misattributing appeals effects to the change in penalty rate versus to the administrative-appeals restructuring. Indeed Figure C.1 plots the time series of appeal rates by penalty rate regime, illustrating both 1) a broad increase in appeals over time, namely beginning in 2016 (the year of the concomitant complaint process reform), and 2) a divergence in the likelihood of appeals from 2016 between the decreased ordinary penalty rate regime and the unchanged elevated penalty rate regime. This second point gives intuition behind the difference-in-differences strategy: the appeal rate decreased for appeals under the ordinary rate regime relative to those under the elevated penalty regime after the penalty rate under the ordinary regime decreased from 30% to 20%. This result heuristically corroborates the conceptual mechanism developed in the conceptual model in Section 3.

More formally, for a penalty case j, we can estimate equations of the form

$$\begin{aligned}1\{Appeal_j\} = \beta_0 + \beta_1 1\{OrdinaryRegime_j\} + \beta_2 1\{Year_j \geq 2016\} \\ + \beta_{dd} 1\{OrdinaryRegime_j\} 1\{Year_j \geq 2016\} + \Gamma X_j + \varepsilon_j. \quad (5)\end{aligned}$$

Identifying a reduced form causal effect β_{dd} of the penalty rate reform on appeal probability relies on a series of assumptions. The first assumption is that absent the penalty rate reform to the ordinary penalty regime, the appeal probability would evolve in parallel for both groups. We can validate this assumption by demonstrating parallel trends. Second, there is an exclusion restriction that the reform is unaccompanied by other factors that differentially affect either of the penalty regimes. We acknowledge the presence of an important threat to this assumption in the form of a separate reform that rendered it easier to open administrative appeals/complaints of penalties, which we observe in the time series beginning in 2016. However, under an assumption that this reform affected the ease of complaints equally for both penalty regimes, this change poses no exclusion restriction violation. This possibility is plausible given our observation that penalties under both regimes saw an increase in appeals in 2016. This said, in our data environment it is not feasible to explore this hypothesis in a more direct manner.

Figure 5 shows the results of estimating Equation (5) using the universe of positive penalty recipients from 2011 to 2020. We observe parallel trends between both groups prior to the reform. Beginning in 2016, the appeal probability for the ordinary regime relative to that for the elevated regime begins to decrease. By five years post-reform, the relative appeal probability decreases by nearly 8pp relative to the 2014 difference. Interestingly, this effect size increases in magnitude over time in a statistically significant manner,[27], which

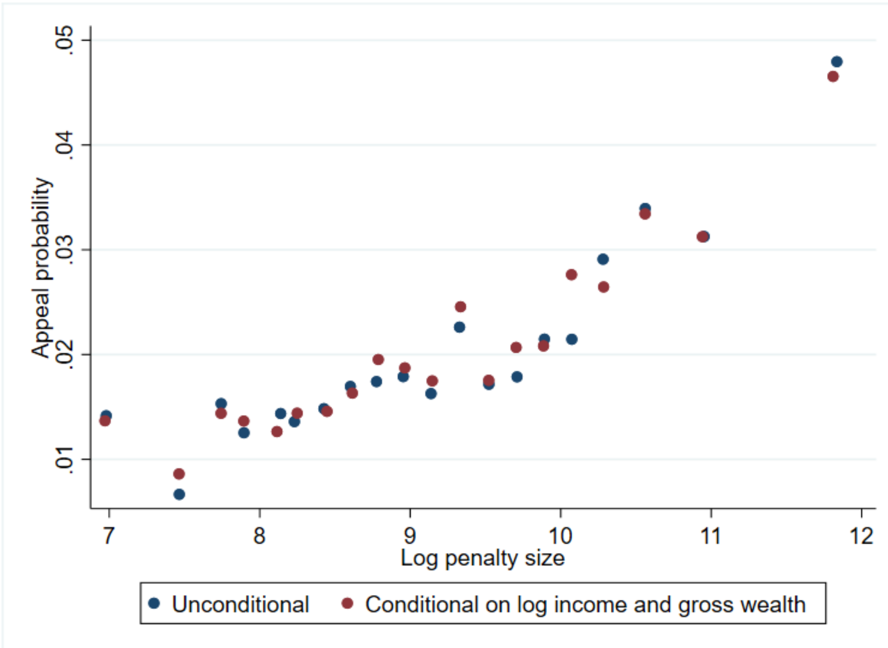
[27] One can estimate the dynamic component of this effect using an analogous triple differences model that tests the interaction between ordinary penalty regime, post-period status, and continuous time. This estimation procedure results in a triple difference coefficient of -0.015 with a bootstrapped standard error of 0.004.

may be attributable to the presence of learning effects or knowledge spillovers regarding the appeals process.

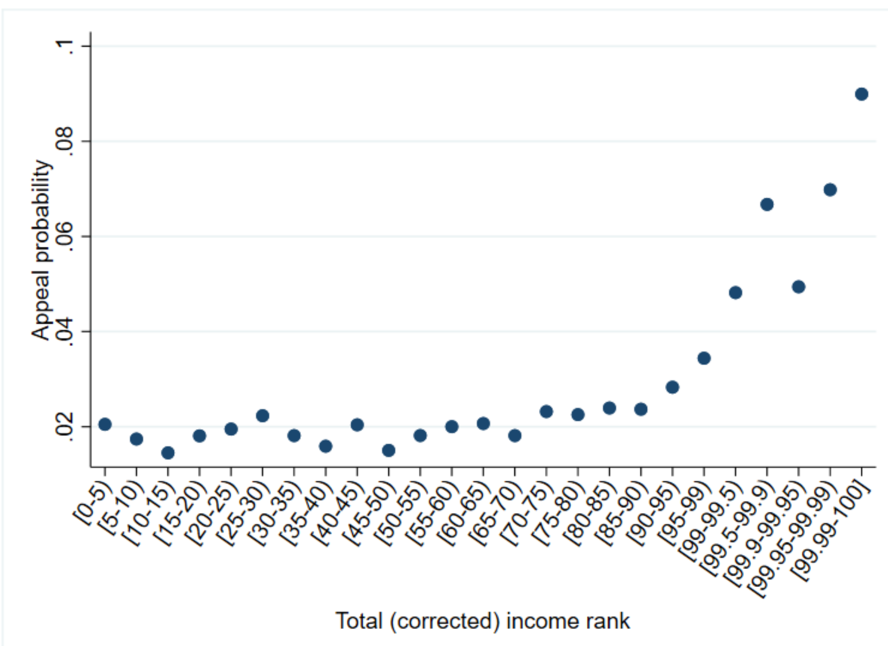
Table 6 Panel (a) presents the reduced form estimates of the impact of the penalty rate reform on appeal probabilities in the two groups. Column (1) gives estimates from an unsaturated regression, reporting a difference-in-differences coefficient of -2.5pp. Columns (2)-(4) vary the inclusion of year fixed effects and as well as log income and wealth amounts, producing very similar estimates. Columns (5)-(8) reproduce the first four columns, however also including log penalty amount as a control variable, aiming to attribute any differential impact explicitly to the penalty *rate*, holding the penalty *amount* constant. Remarkably, the estimated effect sizes remain largely unmoved, if not mildly larger in magnitude than their respective analogue in Columns (5)-(8). We can contextualize this decrease proportionally as an elasticity by dividing the 2.5pp change by the counterfactual appeal rate, estimated as the sum of the Constant and Post coefficients in Column (1), then normalized by the relative change in penalty rate: $\hat{\varepsilon}_{Appeal,\theta} \approx \frac{-.025/(.063+.014)}{-33\%} = 0.97$, statistically indistinguishable from one. Interestingly, the estimated coefficients on Log Penalty in Columns (5)-(8) imply an average elasticity of 0.43 (calculated from an approximate doubling of the penalty size as $\frac{.6\%/1.4\%}{100\%}$). Table C.4 replicates these results, controlling for assigned risk score. We observe nearly identical results, alleviating concerns that compositional changes drive the estimated appeal effects.

Panel (b) reports analogous formal IV estimates using the reform as an instrument, giving estimates smaller in magnitude of a 1.4pp increase in the appeal probability given a doubling of the penalty rate, or in relative terms, an elasticity of around 0.6. Lastly, in the presence of heterogeneous potential outcomes—namely where those in the elevated penalty rate regime are more rate-responsive in terms of their appeal behavior to the change in ease of administrative appeal—estimates under this approach would constitute an upper bound, although it is not necessarily clear whether and which direction the threat to the exclusion violation may introduce bias to this estimate.

Figure 2: Relationship between penalty appeal, penalty size, and income/wealth demographic



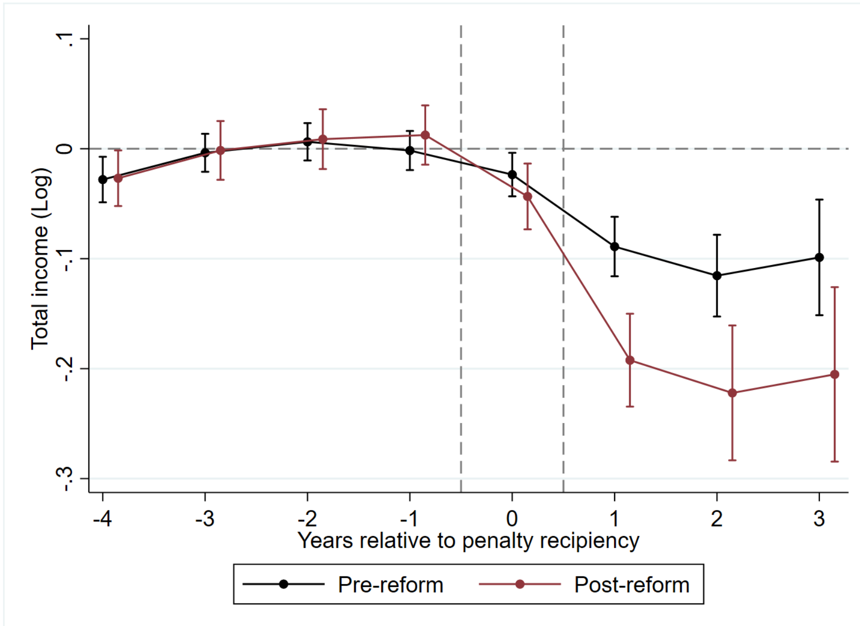
(a) Appeal versus penalty size



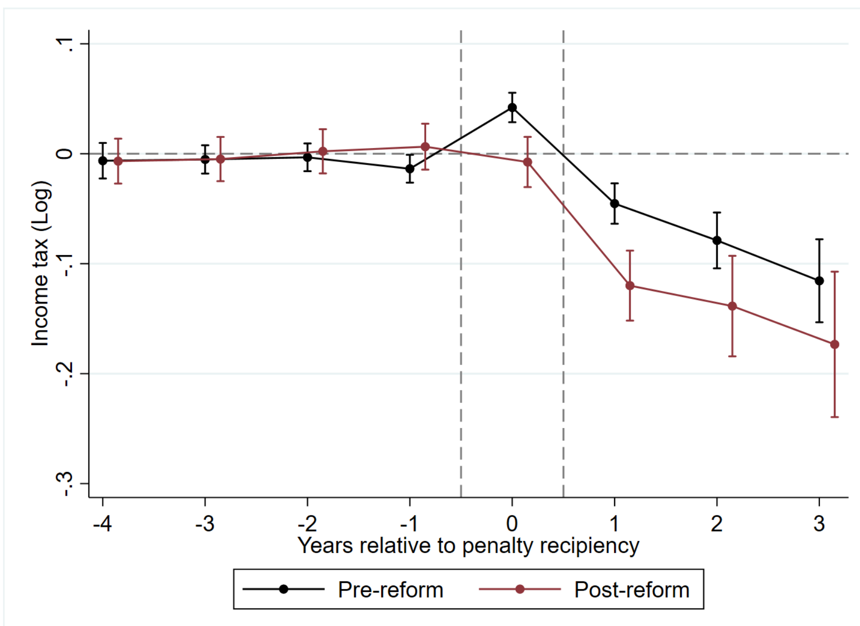
(b) Appeal probability by income rank

Note: These figures display the binned scatter plots of whether or not a penalty is appealed on characteristics pertaining to the penalty and penalty recipient. Both series in Panel (a) include year fixed effects. Corrected income ranks in Panel (b) are constructed on the annual-level. Both panels by definition condition on receipt of penalty.

Figure 3: Event studies around penalty recipiency (reduced form)
Stratified by penalty cohort



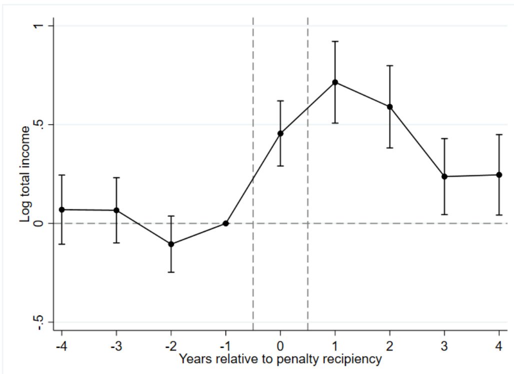
(a) Log income



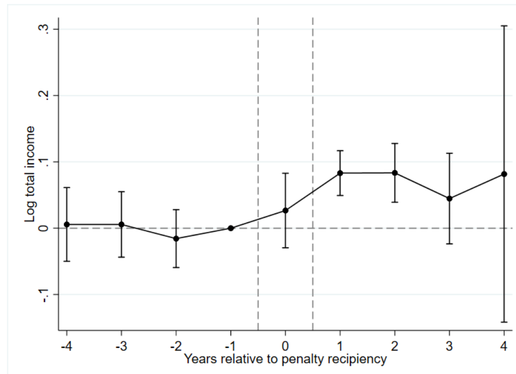
(b) Log taxes paid

Note: These figures display the dynamic effects of the decrease in the statutory penalty rate on the logarithm of total declared income and taxes paid. The figures plot the treatment effect coefficients $\{\hat{\beta}_k\}_{k\geq 0}$ and 95% confidence bands associated with event studies $y_{it} = \alpha_i + \delta_t + \sum_{k=-5}^{5} \gamma_k EventTime_{k(it)} + \varepsilon_{it}$ stratified by post- and pre- reform penalty cohort using corrections for two-way fixed effect bias via the procedure from Callaway and Sant'Anna (2021). Standard errors are clustered at the taxpayer level.

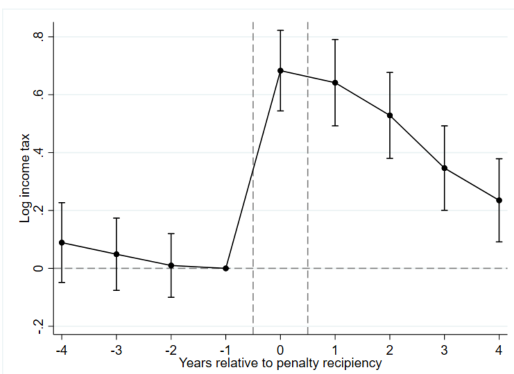
Figure 4: Event study IV difference-in-differences:
Impact of penalties on declared income and taxes paid



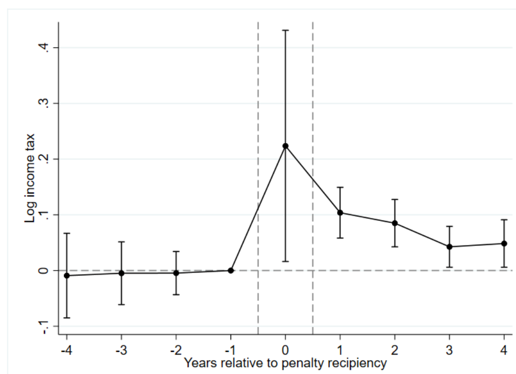
(a) Log income v.
Log penalty size



(b) Log income v.
effective penalty rate



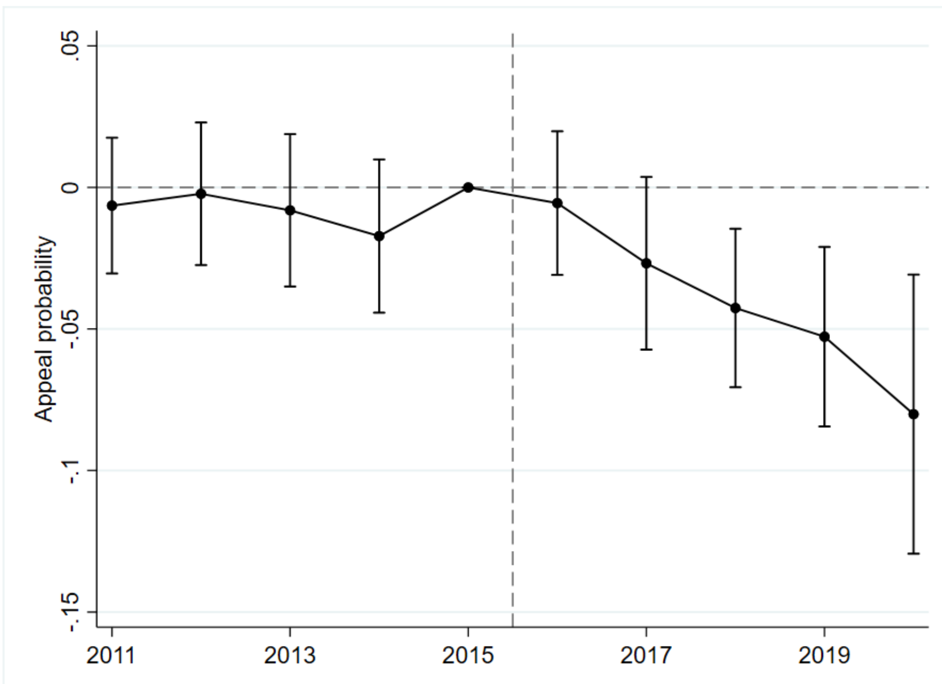
(c) Log taxes paid v.
Log penalty size



(d) Log taxes paid v.
effective penalty rate

Note: These figures plot the treatment effect coefficients $\{\hat{\beta}_k\}$ and 95% confidence bands associated with the instrumental variables difference-in-differences event study $y_{it} = \alpha_i + \sum_{k=-5}^{5} \gamma_k EventTime_{k(it)} + \sum_{k=-5}^{5} \beta_k \theta_i EventTime_{k(it)} + \varepsilon_{it}$, omitting the estimation bins. The underlying approach leverages an instrumental variable design, using a binary indicator for penalty receipt during post reform years 2016-2018 as an instrument for the respective penalty variable. The estimating equation includes taxpayer fixed effects. Standard errors are clustered on the taxpayer level.

Figure 5: Impact of the penalty rate reform on taxpayer-appeal decision
Difference-in-differences using the 2016 penalty rate reform



Note: Figure displays difference-in-differences estimates comparing the evolution in penalty appeal probability for penalties under the ordinary and elevated penalty regimes, where in 2016, the ordinary penalty rate decreased from 30% to 20%. The estimating equation also includes year fixed effects. Confidence intervals are computed using bootstrapped standard errors.

Table 6: Impact of the penalty rate reform on taxpayer-appeal decision
Panel (a): Reduced form difference-in-differences estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ordinary regime × Post	-0.025	-0.026	-0.023	-0.024	-0.027	-0.027	-0.024	-0.024
	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.010)
Ordinary regime	-0.0088	-0.0080	-0.011	-0.011	0.00038	0.00038	-0.0020	-0.0019
	(0.0029)	(0.0031)	(0.0021)	(0.0024)	(0.0047)	(0.0048)	(0.0037)	(0.0037)
Post	0.063	0.062			0.064	0.063		
	(0.0082)	(0.0078)			(0.0081)	(0.0077)		
Log penalty					0.0058	0.0058	0.0059	0.0061
					(0.0021)	(0.0022)	(0.0021)	(0.0022)
Constant	0.014	-0.072	0.036	-0.043	-0.047	-0.082	-0.025	-0.054
	(0.0025)	(0.029)	(0.0030)	(0.029)	(0.022)	(0.032)	(0.021)	(0.032)
Fixed effects	None	None	Year	Year	None	None	Year	Year
Penalty controls	No	No	No	No	Yes	Yes	Yes	Yes
Log income and wealth controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	76,192	69,710	76,192	69,710	76,192	69,710	76,192	69,710
Adjusted R2	.0232	.0264	.0252	.0285	.0256	.0282	.0276	.0305

Panel (b): IV estimates

	(1)	(2)	(3)	(4)
Log penalty	0.017	0.014	0.015	0.016
	(0.0048)	(0.0049)	(0.0043)	(0.0047)
Constant	-0.14	-0.10		
	(0.040)	(0.026)		
Fixed effects	None	None	Year	Year
Log income and wealth controls	No	Yes	No	Yes
Observations	76,192	69,710	76,192	69,710

Note: These tables display results from a series of regressions of a binary indicator of appealing a penalty as dependent variable on interactions of penalty regime assignment and a post-2016 year indicator. Panel (a) gives a series reduced form estimates assessing the relative change in appeal probability between the two regimes around the 2016 reform. Panel (b) displays results from a series of instrumental variables regressions of a binary indicator for appealing a penalty as dependent variable on log penalty amount, instrumented as interactions of penalty regime assignment and a post-2016 year indicator. All regressions in Panel (a) restrict to a similar sample of positive penalty recipients as in Panel (b). Standard errors clustered on the year-level are included in parentheses.

6.2 Estimating the appeals response via within-year variation in penalties

As an alternative approach to identifying the causal effect of the penalty rate on appeals behavior, we adopt an instrumental variables strategy that assigns individuals the average penalty of other individuals penalized in the same regime and year. The intuition for this approach is that individuals' penalties may be well-approximated by average penalties within a given year $\times$ penalty regime cell, but unrelated to the other case circumstances that may drive appeal behavior. Unlike a judge-IV, however, this design does not aim to exploit a measure of idiosyncratic judge or auditor leniency, although we explore a similar strategy in Section 6.3. We instrument for the size of a penalty faced by a taxpayer using a leave-one-out average penalty rate constructed from other taxpayer's penalty assignments within the same year and penalty regime. We observe substantial variation in penalty size over years, and the leave-one-out estimator captures cross-sectional variation in penalty severity that is plausibly exogenous to penalty-recipient potential outcomes, mitigating concerns about endogenous penalty assignment. Moreover, by leveraging within-year comparisons of penalty amounts, we can avoid relying on cross-year variation that may threaten identification if the appeals process changed contemporaneously with penalty rates.

More formally, for a case of taxpayer i in year t, we estimate the following reduced form equation and corresponding instrumented designs to isolate the causal impact of changes in assigned penalties on appeals behavior:

$$Appeal_{it} = \alpha_t + \beta \times PenaltySize_{it} + \varepsilon_{it}. \tag{6}$$

$Appeal_{it}$ is a binary indicator equal to one if taxpayer i appeals a penalty in year t and zero otherwise. $PenaltySize_{it}$ is a continuous measure of penalty exposure that allows us to directly leverage changes in penalty severity and is measured in as either the natural logarithm of the total penalty amount assigned to a taxpayer or the effective penalty rate (total penalty amount divided by total income).[28] Equation (6) further features year fixed effects, indicated by α_t. ε_{it} is the error term. We cluster standard errors at the year-level.

Our main specification includes year fixed effects, but not penalty regime fixed effects in order to leverage the variation in penalty size between ordinary and elevated-rate penalties.[29]

[28]There are two logical ways to construct leave-one-out mean penalty instruments for these parameterizations. In our baseline approach, for a penalty θ_{it}, we construct the instrument parameterization as respective parameterizations of the penalty amount, e.g. $\ln(\bar{\theta}_{-it})$, where $-it$ is the set of individuals other than i that share the same year $\times$ penalty regime cell. An alternate approach would construct the parameterized instruments as the leave-one-out mean of the parameterized penalty, e.g. $\overline{\ln(\theta)}_{-it}$.

[29]Year $\times$ regime cell sizes are coarsely defined so as to be quite large, containing on average 2000 individuals, and as a consequence the leave-one-out-mean themselves demonstrate little variation within these cells.

The validity of this instrumental variables approach here requires that within regime-year leave-one-out average penalties (i) predict individuals' penalties and (ii) are unrelated to other factors that affect taxpayers' propensity to appeal. This second condition may not hold in the presence of factors that generate both appeals and larger penalties common within group.

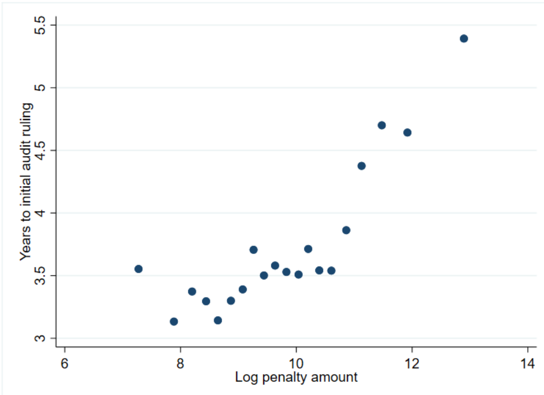
Additionally, because observation of further downstream post-appeal outcomes (e.g. processing time, whether or not there is closing, penalty exoneration, etc.) by definition conditions on having appealed a penalty, we cannot credibly estimate this causal design for these outcomes. Instead, we present ordinary least squares results demonstrating correlations between penalty characteristics and post-appeal outcomes in Figure 6 and Table C.2.

Table C.5 Panels (a) and (b) show that our instruments are highly predictive of individuals' respective penalty burdens and show strong first stages across all specifications. We document large F-statistics and R-Squared values of around 0.1-0.15, suggesting that these instruments indeed contribute meaningful variation in predicting penalty rates within years.

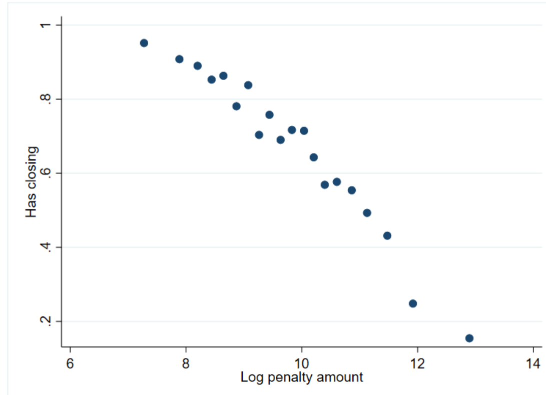
Table 7 reports the main results from our approach. Columns (1)-(3) give estimates of the impact of log penalty amount. Column (1) gives an OLS estimate of .007: a doubling in the penalty size correlates with a 0.7pp increase in appeal probability. The IV estimates in Columns (2) and (3) assign higher values of the causal impact of penalty size of around 1.2pp-1.8pp for a doubling of penalty size. We conjecture that the downward bias of OLS relative to IV estimates may result from strategic targeting of larger penalties by the tax authorities toward cases that may be less likely to appeal. In relative terms, this effect size associated with a doubling of the penalty would correspond with a proportion increase in appeal likelihood of about 60%, or an elasticity of 0.6. This estimate is stable after controlling for annual income and wealth declarations. We observe a pattern arise when analyzing the impact of changes in the effective penalty rate on appeal. Columns (4) estimates that after controlling for corrected income and declared wealth, a 1pp increase in the effective penalty rate induces a 0.65pp increase in the probability of appeal.[30]

[30] Table C.6 displays analogous results using our alternate set of leave-one-out mean parameterization instruments, yielding similar conclusions.

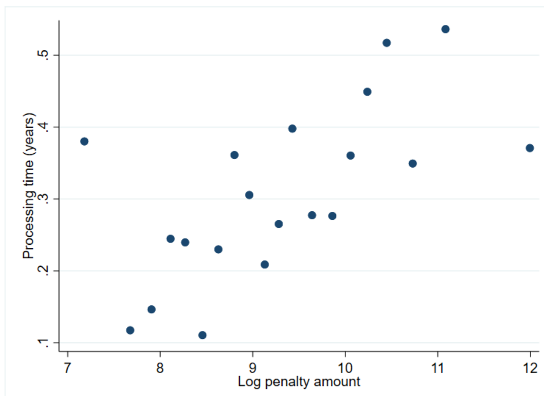
Figure 6: Relationship between post-appeal outcomes and penalty amount



(a) Time to initial ruling



(b) Appeal has closing



(c) Time to appeal ruling

Note: These are binned scatter plots of various appeal-related outcomes on log penalty size. All figures include year fixed effects and control for log income and wealth. The variable "Years to initial audit ruling" (Panel (a)) is in principle applicable to all penalties, but is only observed for appealed penalties.

6.3 Robustness

We substantiate the robustness of this result using three additional strategies complementary approaches. First, Table C.7 expands on our results by exploring the variation in penalty rates within penalty type. Columns (1)-(2) and (3)-(4) stratify by ordinary and elevated penalties respectively (even-numbered columns control for income and wealth). Using our leave-one-out instruments, we estimate a positive impact of penalty size on appeal probability in both cases. While we observe magnitudes of between 2 and 3.5 times greater for the elevated penalty regime than the ordinary regime, elevated penalties feature a greater baseline appeal rate of 5.4% so that proportionally, we these columns result in similar, pos-

Table 7: OLS and IV results: impact of penalty size on taxpayer-appeal decision

	(1)	(2)	(3)	(4)
Log penalty	0.0071	0.017	0.018	
	(0.0023)	(0.0052)	(0.0055)	
Effective penalty rate				0.0065
				(0.0015)
Log corrected income			-0.0089	0.0013
			(0.0036)	(0.0019)
Log gross wealth			0.0031	0.0044
			(0.00095)	(0.00076)
Constant	-0.044			
	(0.021)			
Year fixed effects	Yes	Yes	Yes	Yes
Specification	OLS	IV	IV	IV
Observations	76,192	76,192	69,710	79,209
F-statistic	9.55	11	4.15	14.4

Note: This table displays results from a series of instrumental variable regressions with a binary indicator of appealing a penalty as dependent variable on different parameterizations of penalty severity, using the leave-out mean penalty rate as an instrumental variable (constructed as parameterizations of the leave-one-out mean penalty size). Standard errors clustered on the year-level are included in parentheses.

sibly slightly smaller, elasticities of appeal with respect to penalty size of 0.37 and 0.5. Columns (5) and (6) pool all penalty types, including regime-year fixed effects, yielding similar conclusions.

Second, we develop an analogous set of estimates that exploit variation within year $\times$ penalty regime $\times$ municipality (Norwegian *kommune*) cells. During this time, Norway had 475 distinct municipalities with on average 10,000 people per municipality. In our case, each of these cells contains on average 16 penalized taxpayers with a median of four. In this exercise, we therefore test the impact on appeal probability of penalty size, as predicted by others in the same year $\times$ penalty regime $\times$ municipality cell. This strategy relies on a leniency mechanism that within a local context, when the effective penalty among peers is high, the authorities would moderate the individual penalty downward, and vice versa. Table C.8 confirms this intuition, showing a strong and negative predictiveness of peer penalty sizes on one's own penalty size. The table gives estimates from versions of this instrumental variables specification, varying the included covariates and minimum cell sizes.

Among our main sample, we estimate appeal probability elasticities of between 0.25 and 0.5.

Lastly, we present evidence on the impact on appeals of greater penalties—specifically leveraging the within-taxpayer variation in penalty size for taxpayers that are penalized multiple times. Only around 20% of our sample of penalized individuals are observed as penalized multiple times between 2011 and 2021. Restricting our sample to these individuals by including taxpayer fixed effects aims to net out individual idiosyncrasies in propensity to appeal, instead leveraging specifically differences in penalty size within individuals over time. Columns (1)-(3) present OLS estimates; Columns (4)-(6) give causal IV estimates using our year $\times$ penalty regime leave-one-out mean strategy from Section 6.2. We find compelling that even within an individual, after controlling for corrected income and wealth as well as "penalty order" (the ordinal sequence of penalty receipt), greater penalties *within* individual induce greater appeal probabilities. Our estimates are identical in sign but around one-third the magnitude of the estimates in Table 7. This difference suggests that there exists large variation in appeal propensity across individuals, while there is more limited, albeit statistically significant, scope for within-individual responsiveness to the penalty rate.

These results align with the predictions from our taxpayer behavioral model in Section 3: while stricter enforcement through higher penalties exerts a positive effect on tax compliance, they also heighten taxpayers' incentives to challenge their imposed penalties and engage in a potentially lengthy appeals process. Given that appeals require substantial resources—including staff time and legal assessments—the additional amount generated by higher penalties may considerably strain tax authorities' capacities. This then does not only result in increased administrative expenditures, but may also delay the resolution of other cases, thereby decreasing overall system efficiency. Our analysis is the first to shed light on both the benefits and costs associated with administering tax noncompliance penalties, and provides a basis for the design of an efficient penalty schedule. In the final section, we study theoretically how the optimal penalty rate should be set so as to balance the tradeoff between compliance enhancement and administrative feasibility.

7 Optimal tax administration with dual-revenue instruments

In the preceding sections we developed a behavioral framework that highlights the key margins through which tax penalties affect taxpayer behavior, the use of different evasion technologies, and the decision to appeal an issued tax penalty. We then brought this framework to the data, documenting that higher penalty rates increase reported income and tax payments, but that they also generate increased administrative costs and that frictions in the appeals system may reduce the effective collection of assessed liabilities. Penalties are neither a costless nor a purely deterrent instrument, but rather operate through multiple channels that jointly determine their fiscal and welfare consequences. Here we ask how tax authority should optimally set the penalty rate.

The goal of this section is to provide an understanding of and intuition for how the tax administration should think about the optimal design of the penalty rate, followed by a calibration to the Norwegian environment that builds on insights from the analysis above. We develop conceptual results by extending the model from Keen and Slemrod (2017) to explicitly consider the dual-revenue generating properties of the penalty rate in conjunction with other tax administration policy instruments. The most important results consist of the expression for the optimal penalty rate, intuition for its mechanics, and the subsequent calibration. For brevity, we develop only the central results here; other peripheral results as well as those that follow as more straightforward (yet insightful) extensions of Keen and Slemrod (2017) are included in Section D.2.

We begin by considering a representative individual with quasi-linear preferences expressed implicitly in expectation.

$$W = x - \psi(l) + v(g), \tag{7}$$

for expected consumption x, labor supply l, and common good g. $\psi(\cdot)$ and $v(\cdot)$ are both increasing and strictly convex and concave, respectively. Public spending g is financed by a linear tax on income and is taken as exogenous by the taxpayer. The taxpayer is able to shelter and underreport an amount e off of her true tax base wl at some cost $c(e, \alpha)$ based on the amount underreported and sheltering costs. We impose c_e, c_{ee}, and $c_{e\alpha} > 0$ and the taxpayer chooses strictly positive concealment. Additionally, with some probability, the taxpayer will be caught by the tax authorities and required to pay back the taxes due on the discovered underreporting. This expected repayment depends on policy parameters that affect the probability of discovery, the expected amount of discovery, the tax rate t, and the penalty schedule $r := r(e, t, \Xi)$ for a vector of enforcement instruments Ξ. In our case, α refers to tax administration instruments that only generate revenues through behavioral improvements in compliance, e.g. increasing perception of monitoring (in the absence of audit itself) or engaging in informational campaigns about compliance and audit/threat.

$$x = wl - t(wl - e) - c(e, \alpha) - r(e, t, \Xi) \tag{8}$$

This setup expands on the model environment in Keen and Slemrod (2017). The instruments contained in vector Ξ enact a dual revenue effect: by affecting underreporting behavior and by collecting on successful detection of infra-marginal underreporting.

In Keen and Slemrod (2017), the function $c(e, \alpha)$ refers to the costs of compliance or concealment. Part of the strength of their model environment lies in the quite general interpretation of how these costs are internalized. In their setting, the reader interprets this as some average internalized cost associated with underreporting that presumably internalizes detection risk. However, consider in our setting that $c(e, \alpha)$ explicitly captures the pecuniary costs of underreporting, which the social planner can influence (e.g. increasing information reporting increases the costs that individuals need to pay to misreport in more sophisticated manners). Furthermore, individuals consume out of post-detection income in expectation. Assume that detection is associated with perfect specificity (no false positives), and finally that, unlike as in Norris and Rose (2024), penalty recipients do not default on their penalties.[31]

[31] We explicitly consider an extension of our model with a non-payment risk (that increases as a function of the penalty rate) below. Nonetheless, there are several reasons for abstracting from non-payment risk. For one, a legacy literature has focused on macroeconomic default risk on debt (e.g. Arellano (2008)) as well as on legal financial obligations in non-tax settings (e.g. Norris and Rose (2024)), demonstrating the Laffer mechanics of debt repayment. Additionally, penalties are typically relevant for very high-income taxpayers whom we assume exhibit little liquidity constraints and default risk. Of course, while default risk

This setup yields first-order conditions for interior optima for labor supply and underreporting: $(1-t)w = \psi'(l)$ and $t = c_e + r_e$, with $e_\alpha = -c_{e\alpha}/(c_{ee}+r_{ee}) < 0$ and $e_t = (1-r_{et})/(c_{ee}+r_{ee})$. For a simple parameterization of collections on underreporting $r = \rho\eta(t+\theta)e$ (for penalty rate θ, audit rate ρ and expected discovery share η), $r_{et} = \rho\eta$, i.e. the share of underreporting that will be detected in expectation, which we can understand as less than one so that $e_t > 0$. Lastly, we have $e_\xi = -\frac{r_{e\xi}}{c_{ee}+r_{ee}} < 0$ for an arbitrary dual-revenue instrument $\xi \in \Xi$.

The government collects revenues via the linear tax and penalties levied and expends funds on public goods and the enforcement costs:

$$g + a(\alpha, \Xi) = t(wl - e) + r(e, t, \Xi). \tag{9}$$

Combining this equation with Equation (7) and Equation (8) yields an alternative expression for social welfare:

$$W(t, \Xi) = wl - tz - c(e, \alpha) - r(e, t, \Xi) - \psi(l) + v\Big(tz + r(e, t, \Xi) - a(\alpha, \Xi)\Big), \tag{10}$$

for taxable income $z(t, \alpha, \Xi) := wl(t, w) - e(t, \alpha, \Xi)$. The government's cost $a(\alpha, \Xi)$ is assumed to be weakly monotonically increasing and non-negative in its arguments. Additionally, note that in this specification, enforcement efforts do not have first-order effects on earnings. We omit potential real impacts of underreporting behavior on labor income for simplicity. In general, allowing for endogenous responses of labor supply to underreporting activity generates real earnings responses to enforcement measures, which change the perceived tax rate and mitigate the negative impact of the tax rate and labor supply, by attenuating the statutory tax rate toward a weakly lower "perceived" tax rate.

mechanically becomes a non-trivial possibility for exorbitantly high penalties, we can interpret our model so that part of this non-payment risk is also internalized through administrative costs a. Moreover, as we discuss below, non-payment risk as well as aggregate collections on underpayment are quite low in Norway, rendering this distinction less relevant for our empirical setting.

7.1 Optimal tax administration

We fully develop and discuss first-order conditions for the tax rate and the tax enforcement instrument in Section D.2.1. The key insights obtained by incorporating penalties are the following: 1) For the tax rate, while a standard inverse elasticity rule shows that the greater the base elasticity, the lower the optimal tax rate, this effect is undermined by the presence of collections on underreported income; the optimal income tax sees additional upward pressure through the addition of collections on marginal underreporting that is successfully detected. 2) For the optimal level of enforcement (that does not directly generate revenue), the presence of collected income on detected underreporting dampens the benefits associated with improving compliance. Indeed, in the hypothetical scenario that $r_e > t$ (perhaps where underreporting agents are risk loving or exhibit behavioral internalities that prevent them from internalizing that underreporting delivers losses in expectation), the optimal α is actually negative in order to encourage underreporting (delivering more revenue to the social planner in expectation than via the income tax).

Dual revenue instrument first-order condition. Let us characterize the first-order condition for an arbitrary instrument $\xi \in \Xi$ with dual revenue effects:

$$W_\xi = -r_\xi + v' \cdot (r_\xi + z_\xi(t - r_e) - a_\xi) = 0. \tag{11}$$

The term $-r_\xi$ gives the welfare loss from the decrease in private consumption due to the change in the penalty instrument ξ. The second term reflects the net revenue change multiplied by the value of the public good. The revenue change is composed of three parts: 1) the increase in revenues on infra-marginal collections; 2) the change in collections due to the behavioral responses of agents that substitute away from underreporting (thereby paying income tax as opposed to the actuarial marginal rate on underreported income); and 3) administrative/other costs of increasing the level of the instrument. Rearranging this condition gives:

$$\varepsilon_{e,\xi} = \frac{r}{e(t - r_e)} \cdot \frac{\epsilon_{r\xi}(v'(1 - \frac{a_\xi}{r_\xi}) - 1)}{v'}. \tag{12}$$

Note that $\frac{r}{e(t-r_e)}$ measures the ratio of collections on underreporting to the overall tax gap after accounting for collections on successfully detected underreporting. This expression prescribes that the penalty instrument ξ should be calibrated such that the elasticity of underreporting with respect to ξ should equal the ratio of the welfare effect impact of the mechanical change in collections to the tax gap.

Closer inspection of this result reveals a perhaps counter-intuitive finding: Presuming $\varepsilon_{e,\xi} \leq 0$, the welfare-weighted mechanical net change in collections and the tax gap should be opposite sign. Given that $v' > 1$, we can rationalize opposite signs of these objects in two manners. In the more likely case, we have $t > r_e$. Given that $\epsilon_{r\xi} > 0$, it must be the case that $v'(1 - \frac{a_\xi}{r_\xi}) - 1) < 0$, i.e. that $\frac{a_\xi}{r_\xi} > \frac{v'-1}{v'}$: the mechanical marginal-cost-revenue ratio must exceed the relative valuation of the public good and private consumption.

A second possibility is that $r_e > t$: the actuarial penalty rate (not only the penalty rate) collected on underreported income *exceeds* the linear income tax rate. This case would come with the surprising implication that the social budgeter would prefer individuals to underreport their income rather than honestly pay income tax. In this case, a risk-neutral agent would of course not choose to underreport; i.e. $e = 0$. This situation would arise if the tax administration's marginal costs of increasing the penalty instrument ξ are very low, which may indeed be the case for zero-nominal cost instruments with low behavioral salience.

We cannot simply calibrate ξ^* in closed form, as ξ^* likely appears in the partial derivatives of r. However, one expression of $\xi^* > 0$ (for $\varepsilon_{e,\xi} \neq 0$, $r_e < t$ and $\frac{a_\xi}{r_\xi} \neq \frac{v'-1}{v'}$) gives

$$\xi^* = \varepsilon_{z,\xi} \cdot \frac{z(t - r_e)}{r} \cdot \left(\frac{\epsilon_{r\xi}(1 - v'(1 - \frac{a_\xi}{r_\xi}))}{v'} \right)^{-1}. \tag{13}$$

Dual-revenue-generating instruments see an opposite elasticity rule to that of the optimal income tax, increasing in proportion with the sensitivity of agents. A greater (positive) elasticity of reported income to the instrument $\varepsilon_{z,\xi}$ implies a greater effectiveness in inducing agent substitution from underreporting to honest declaration (and paying income tax). Intuitively, greater costs a_ξ imply a lower ξ^* such that similarly to as with the above intuition, we can rationalize a very large ξ so that that $r_e > t$ if marginal costs are very low. However, with risk-neutral agents with full knowledge of the penalty environment and a monotonically increasing costs in ξ, the maximum optimal ξ^* would be set such that $r_e(\xi^*) \equiv t$.

Before proceeding, to illustrate additional properties of the optimal level of ξ, we highlight two special cases:

Case 1. $\varepsilon_{z,\xi} = 0$: When taxpayers are unresponsive to the penalty rate, the first-order condition reduces to a perhaps more simple optimality condition that the marginal-cost-revenue ratio equals the relative valuation of the public good and private consumption:

$$\frac{a_\xi}{r_\xi} = \frac{v' - 1}{v'}. \tag{14}$$

Case 2. $a_\xi = 0$: We highlight this benchmark as of interest because changing the penalty rate, as opposed to audit rate or intensity, indeed incurs zero *nominal* cost (Becker (1968)). Of course, Section 6 provides novel estimates of the real administrative costs associated with the penalty rate, but this case provides a useful benchmark. In this case, the first-order condition is:

$$\frac{v' - 1}{v'} \cdot r_\xi = e_\xi (t - r_e) \tag{15}$$

Because $\varepsilon_{e,\xi} < 0$, this condition implies that the actuarial collection rate on underreported income *exceeds* the linear income tax rate so that $r_e > t$. In this case, agents would only continue to engage in underreporting if they exhibit behavioral internalities or behave as risk-loving.

Noting that for an agent at her envelope, $e_\alpha = e_\xi \cdot c_{e\alpha}/r_{e\xi}$,[32] so that substituting this equivalence into the first order condition in α and simplifying yields the relationship at the optimal levels of α and ξ:

$$\frac{r_\xi / v' + a_\xi - r_\xi}{c_\alpha / v' + a_\alpha} = \frac{r_{e\xi}}{c_{e\alpha}}. \tag{16}$$

The numerator on the left-hand side is a first-order approximation of the marginal welfare cost of increasing ξ, and the denominator represents the marginal welfare cost of increasing α. At the optimum, this marginal welfare cost ratio is equal to the ratio of the curvatures

[32] In terms of elasticities, $\varepsilon_{e,\alpha} = \frac{c_{e\alpha}\alpha}{r_{e\xi}\xi} \cdot \varepsilon_{e,\xi}$

of each respective instrument's cost function from the taxpayer's perspective.

At an optimal level of ξ, from Equation (11) it results that

$$\frac{r_e(\xi^*)}{t} = 1 + \Big(\frac{v' - 1}{v'} - \frac{a_\xi}{r_\xi}\Big) \cdot \frac{1}{\varepsilon_{et}} \cdot \frac{r_\xi}{e} \cdot m, \tag{17}$$

for $m = \frac{1-r_{et}}{r_{e\xi}}$.

This expression most clearly indicates the optimal relationship between r_e and t. Because $\varepsilon_{et}, r_\xi, e, m > 0$, it is necessarily the case that $r_e(\xi^*) > t$ if $\frac{a_{\xi^*}}{r_{\xi^*}} < \frac{v'-1}{v'}$, which is the case when v' is very large or the marginal cost of increasing instrument ξ is very low (relative to marginal collections). In practical terms, the social planner would set $r_e(\xi^*) \equiv t$.

7.2 Optimal compliance gap and recoup gap

We develop results on the optimal compliance gap and the optimal recouped tax gap in Section D.2.2. One important implication of incorporating collections on underreporting is that the optimal tax gap increases as the actuarial penalty rate on underreported income approaches the linear income tax rate: the tax administration tolerates more underreporting if in expectation more underreported revenues are re-captured.

7.3 Complementarity and substitutability among instruments

To what extent do these different instruments operate as strategic complements or substitutes? This question is relevant if one or multiple instruments cannot be feasibly adjusted (possibly due to political economy constraints).

This section poses seven questions pertaining to the strategic interactions of the tax rate, administrative enforcement effort, and penalizing instruments:

1. Is it better to raise funds with ξ, α, or t?
2. If ξ is constrained, (when) should the tax administration raise t? I.e. what is the slope of the best response function $t(\xi)$?
3. If ξ is constrained, (when) should the tax administration raise α? I.e. what is the slope of the best response function $\alpha(\xi)$?
4. If t is constrained, (when) should the tax administration raise ξ?
5. If α is constrained, (when) should the tax administration raise ξ?

6. How should the tax authorities prioritize different dual revenue raising instruments?

In answering these questions, we assume constant v' for simplicity. Questions (4) and (5) are, of course, symmetric with questions (2) and (3), respectively. In the following, we focus on questions (2)-(5). Section D.2.3 addresses questions (1) and (6). In brief, the most important insights from these questions are that in a setting with multiple dual revenue raising instruments, when the tax administration budget is constrained below the social optimum, all pairs of instruments, say $\xi_i,\ \xi_j \in \Xi$, should have equilibrated ratios of marginal net funds raised equal to the ratios of privately-borne marginal costs.

7.3.1 If ξ is constrained, (when) should the tax administration raise t?

We apply the implicit function theorem to the first order condition in the tax rate (Equation (25)), the first-order condition for the choice of t, resulting in the derivative of the marginal welfare impact of t with respect to the penalty instrument ξ. If this object is positive, then the penalty instrument and the tax rate are strategic complements. If the object is negative, then the instruments are strategic substitutes.

The sign of the slope of the best response function $t(\xi)$ is given by the sign of:

$$W_{t\xi} = \underbrace{(v'-1)\cdot(r_{t\xi}+z_{\xi})}_{>0} + \underbrace{v'\cdot r_{e\xi}\cdot e_t}_{>0} + v'\cdot(t-r_e)\cdot z_{t\xi}. \tag{18}$$

Therefore, it is a sufficient, but not necessary condition for the statutory tax rate and the penalty instrument to be strategic complements (i.e. that $dt/d\xi > 0$) that $(t-r_e)$ and $z_{t\xi}$ be of the same sign.

The first two terms of Equation (18) are positive and support the idea that t and ξ are strategic complements. An increase in the penalty instrument increases revenue both by discouraging underreporting and by increasing collections on infra-marginal underreporting. However, if $t < r_e$, then the tax administration expects greater collections on marginal underreporting than on marginal honest reporting. This force undermines the complementarity of the instrument if the case holds and $z_{t\xi} > 0$. We typically think that $z_{t\xi} \geq 0$, i.e. the taxable income response to the tax rate attenuates for greater levels of penalty. Empirically, we cannot find any cases where $t < r_e$ so that heuristically, we think of t and ξ unambiguously as strategic complements.

7.3.2 If ξ is constrained, (when) should the tax administration raise α?

We apply the implicit function theorem to the first-order condition in α (Equation (27)), the first-order condition for the choice of α. The slope of the best response function is given by the sign of

$$W_{\alpha\xi} = -(a_{\alpha\xi} - r_{e\xi}e_{\alpha}) + z_{\alpha\xi}(t - r_e) \tag{19}$$

Unlike the relationship between ξ and t, the first two terms here unambiguously suggest the strategic substitutability of ξ and α. First, the tax administrative cost function α is likely weakly convexly increasing so that $a_{\alpha\xi} \geq 0$. If administrative costs are additively separable between ξ and α, then this term is equal to zero. The second term reflects a decrease in collections on underreporting through the negative effect of the enforcement instrument on underreporting. The last term is ambiguous, based on whether the signs of $(t - r_e)$ and $z_{\alpha\xi}$ agree.

It is a sufficient, but not necessary, condition for the enforcement instrument and the penalty instrument to be strategic substitutes (i.e. that $d\alpha/d\xi < 0$) that $t < r_e$. Because $z_{\alpha\xi} \geq 0$, an increase in the penalty instrument reinforces the enforcement instrument such that if $t < r_e$, the substitution response from underreporting to honest declaration results in a decrease in collections. This being said, given that typically $t > r_e$ it is possible that the instruments indeed serve as complements. Empirically, this is difficult to determine, as the sign depends on second cross-derivatives of the administrations cost, revenue collections, and income declaration function. If we assume the agent's income declaration response to enforcement efforts is constant in the penalty rate, this term simplifies to zero, and we can conclude that enforcement efforts and the penalty rate operate as substitutes.

7.4 Parametric application and empirical calibration to Norway

To more concretely illustrate our results, we give an explicit parametric form to the penalty instruments. That is, we specify an audit rate ρ, an expected discovery share η, and a linear penalty schedule that requires the taxpayer to pay a penalty rate θ plus the underpaid taxes t on successfully detected underreported income. That is, $r(e,t,\Xi) := r(e,t,\rho,\eta,\theta) = \rho\eta(t+\theta)e$. For notational ease, let $\Delta := t - \rho\eta(t+\theta)$ refer to the difference between the statutory tax rate and the in-expectation collection rate (inclusive of back-tax) on underreported income, $D := \rho\eta e$ refer to the aggregate amount of discovered underreporting, and $d := \rho\eta$ represent t the discovery rate of underreported income.

7.4.1 Optimal penalty rate θ

Inserting the above parameterizations into the first-order condition for the penalty rate in Equation (11) and rearranging gives:

$$\theta^* = \Delta \cdot \frac{z}{D} \cdot \varepsilon_{z,\theta} \cdot \frac{v'}{1 - v'(1 - \frac{a_\theta}{D})}. \tag{20}$$

This expression is not purely closed-form, as the penalty rate θ appears in the term Δ. Further rearranging this expression yields:

$$\theta^* = \frac{t(\frac{1}{d} - 1)}{1 + \frac{1}{\varepsilon_{e,\theta}} \cdot \frac{v' \cdot (1 - \frac{a_\theta}{D}) - 1}{v'}}, \tag{21}$$

Section D.2.6 highlights several special cases of the optimal penalty rate under this parametric form.

7.4.2 Numerical calibration

Lastly, we apply this framework to calibrating the optimal penalty rate and the relationship between the penalty rate and other instruments. To start, we re-express Equation (21) in terms of our estimated objects and publicly-available aggregate statistics.

$$\theta^* = \frac{t \cdot z \cdot \varepsilon_{z,\theta} \cdot (1-d) - b \cdot \varepsilon_{b,\theta}}{d \cdot z \cdot \varepsilon_{z,\theta} - D \cdot (\frac{v'-1}{v'})}. \tag{22}$$

We use $\varepsilon_{z,\theta}$ instead of $\varepsilon_{e,\theta}$, as we directly estimate the former parameter, whereas the latter suffers from severe censoring issues. We also decompose $a \cdot \varepsilon_{a,\theta}$ into $b \cdot \varepsilon_{b,\theta}$ where b represents the costs of the complaint/appeals procedure (within aggregate administrative expenditures). We use the following values for estimated and environmental parameters.

- $\varepsilon_{z,\theta} = .45$
- $\varepsilon_{b,\theta} = .6$
- Aggregate administrative expenditures on appeals: $b = 73.8$ million NOK (Norwegian Ministry of Finance (2023b))
- Aggregate declared income: $z = 2.4$ trillion NOK (internal calculation)
- Top marginal tax rate: $t = .474$
- Aggregate discovered underreporting: $D = 74.2$ billion NOK (Norwegian Ministry of Finance (2023a))
- Marginal social value of spending: $v' = 1.2$
- Underreporting discovery rate: $d = .2$ (Alstadsæter et al. (2019))

From these values, we can calibrate an optimal penalty rate. First, it is worth remarking that relative to aggregate collections, the expenditures on the appeals board are extremely low: appeals expenditures amount to about five orders of magnitude less than overall collections, and aggregate discovered underreporting represents only around 3% of aggregate declared personal income.[33] This is to say that empirically, in calibrating the optimal rate, our expression further simplifies to $\theta^* \approx t \cdot \frac{1-d}{d}$, a simple function of the tax rate and the discovery rate. This discovery rate d (observed evasion divided by total evasion) is estimated with substantially less certainty. Alstadsæter et al. (2019) finds that audits only detect around 20-30% of evasion including undeclared offshore income among the wealthiest individuals in Scandinavia.

For these parameters, we calibrate an optimal penalty rate of 200%.[34] While this figure is an order of magnitude larger than the current statutory ordinary penalty rate in Norway, a penalty rate of this size is not uncommon in other countries. For instance, Belgium, Brazil,

[33]The low level of expenditure on tax enforcement and complaints anecdotally corroborates other works that estimate a very high marginal value of public funds associated with tax enforcement expenses (e.g. Boning et al. (2024)).

[34]Figure D.1 plots the optimal penalty rate a separate functions of the discovery rate and of the marginal value of social spending, keeping all other parameters fixed as specified.

India, and Singapore all feature statutory penalty rates in excess of 100% (up to 400% in Singapore). While this calibration does not take into consideration political economy costs or non-payment risk[35], it demonstrates that the optimal penalty rate is likely far greater than the prevailing statutory rate.

8 Conclusion

This paper is the first to study the impact of income tax noncompliance penalty size/rate on taxpayer behavior. The magnitudes and directions of taxpayer responses to the penalty rate as well as conceptual findings have central implications for tax administration policy as well as our understanding of policy aiming to regulate crime and regulatory noncompliance.

A central lesson from our analysis is that the effectiveness of tax enforcement tools cannot be evaluated independently of the data systems through which they operate. While modern tax administrations generate large volumes of data, these systems are typically designed for case processing and legal documentation rather than for tracking the full lifecycle of enforcement actions. As a result, key elements of the compliance process—such as the link between initial detection, penalty assignment, appeal decisions, and final collection—are often only partially observable or difficult to reconstruct in a consistent way. These limitations directly constrain the ability of policymakers to assess the performance of enforcement instruments and to design evidence-based reforms. The fragmentation of information across systems and over time can make it challenging to distinguish behavioral responses from administrative frictions when interpreting observed outcomes.

Our findings point to the value of targeted investments in data infrastructure. A first priority is to enable consistent tracking of cases across the full administrative chain, linking audit selection, case handling, penalty decisions, appeals, and final resolution through stable

[35] We can extend this calibration by considering non-payment risk, augmenting the collections function as $r := \rho\eta(t+\theta)e \cdot (1-q(\theta))$, where $q(\theta)$ represents either a probability of non-payment or an expected non-payment share, increasing in the penalty rate. This extension alters the optimal penalty rate formula analogue to Equation (22) as $\theta^* = \frac{t \cdot z \cdot \varepsilon_{z,\theta} - (r \cdot q \cdot \varepsilon_{q,\theta} - b \cdot \varepsilon_{b,\theta})/(1-q)}{d \cdot z \cdot \varepsilon_{z,\theta} - D \cdot (\frac{v'-1}{v'})}$. However, because both aggregate collections on underreported income r and non-payment probability q are comparably low in Norway, the inclusion of this non-payment risk only negligibly alters the calibrated optimal penalty rate. We can observe though that a greater level of non-payment risk augments the conceptual forces that drive down the optimal penalty rate.

identifiers. A second priority is to improve the granularity and standardization of recorded information, including the type of audit or control activity, the legal and factual basis for adjustments, and the classification of outcomes in appeals. A third priority is to ensure that key economic variables such as the underlying amount of under-reported income or tax are recorded in a way that allows for meaningful analysis of effective tax and penalty rates. Policy changes such as reforms to penalty rates, audit strategies, or appeal procedures create natural opportunities for learning, but only if data systems are structured to capture the relevant outcomes.

We develop a simple conceptual model that illustrates the compliance- and cost-administrative impacts of higher penalties. We further develop the conceptual foundations of the tradeoffs and optimality considerations inherent to setting the penalty rate. Using a framework extension of Allingham and Sandmo (1972), we establish the conceptual grounds for adverse responses to higher penalty rates, demonstrating that higher penalty rates, while likely decreasing overall underreporting, also potentially induce increased appeals on part of taxpayers, which may be costly from an administrative perspective. Additionally, higher penalties may also induce substitution to underreporting vehicles with lower probability of detection (although under only a change in penalty rate (i.e. ceteris paribus), this cross-vehicle substitution does not outweigh the own-vehicle decrease in underreporting).

Our empirical work focuses on two components of this tradeoff: the ex-post income reporting response and administrative complaints response to the penalty rate following the receipt of a tax noncompliance penalty. Our main empirical design leverages quasi-experimental variation in the penalty rate in the form of a Norwegian legislative reform that lowered the standard statutory penalty rate from 30% of underreported base to 20%. To isolate the effect of this response, we implement a difference-in-differences event study design that compares the evolution in outcomes of cohorts penalized before and after this reform around receipt of their penalties. We find that the 10pp penalty rate decrease induced a 10% relative decline in gross income declared and overall tax payments in the years following receipt of penalty. Given the relative penalty rate decrease, this response corresponds with an elasticity of tax collections and taxable income with respect to the ex-post penalty rate of upwards of between 0.3 and 0.45. We also provide some evidence that the decrease in the penalty rate led to a relative improvement in quality-of-life outcomes among penalized individuals, including a lower probability of relatively losing ownership stakes in a business. We further leverage the richness of our data to show that the compliance effects of penalties can extend beyond directly implicated taxpayers, generating spillovers to both business peers and household members.

On the administrative cost side, we show that higher penalties induce a higher probability of administrative appeal/complaint, which we show is associated with longer processing times, greater labor costs, and lower collection probability for the tax administration, thereby reducing the expected present-value discounted revenue collected for a given penalty case. We exploit the penalty rate decrease reform as well as a series of leave-one-out estimator designs to estimate elasticities of appeal probability of around 0.6. This finding demonstrates concretely that higher penalties come with real costs from a tax administrative perspective. We anticipate that this finding generalizes to other settings that deal with pecuniary penalties, such as in other kinds of financial crime and regulatory noncompliance: penalties indeed increase revenues through mechanical increases in collections as well as potential deterrence responses (which we do not explicitly study), but also come with real administrative costs that should be considered in the calibration of this instrument.

We conclude by considering the optimal tax noncompliance penalty rate in an optimal tax administrative framework. We model penalties within a broader class of tax administrative instruments that generate fiscal revenues both through improvements in compliance as well as through increased collections on infra-marginally underreported income that is successfully detected by the tax authorities. We find that accounting for dual revenue instruments such as penalties increases the optimal linear income tax rate relative to a baseline with underreporting in absence of these instruments, because these instruments—such as penalties—also generate revenue off of income underreporting cases that are successfully detected and penalized. We also find that to a first-order approximation, the statutory penalty rate should be set so that the actuarial penalty rate—the in-expectation net payment rate per unit of underreported income—equals the optimal linear income tax rate. The penalty rate falls short of this level insofar as the penalties induce real administrative costs and welfare costs to those penalized. We calibrate this model to the Norwegian environment and estimate an optimal penalty rate of 200%—a large but globally not uncommon penalty size.

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Appendix A Additional graphs and tables to main text

A.1 Additional descriptive results on penalties

Table A.1: Descriptive statistics of tax noncompliance penalties
Panel (a): Characteristics of penalties (2011-2021)

	Mean	SD	p10	p50	p90	N
Penalty amount	19,499	115,402	0	6,431	41,801	88,264
Penalty amount (log)	9.11	1.25	7.62	9.05	10.7	77,475
Positive penalty amount (binary)	.88	.33	0	1	1	88,264
Penalty share of income	.08	.21	0	.02	.12	88,264
Penalty appeal	.025	.156	0	0	0	88,264
Any penalty count	88,562					
Elevated penalty count	11,251					
Penalized population share	.002					
Penalized share of income tax receipts	.003					
Penalty share of income tax receipts	.0004					

Panel (b): Penalty recipient and non-recipient characteristics

	Non-penalized	Penalized	Difference
Total income	444,445	622,356	183,846
	[833,008]	[1,609,389]	(7,442)
Log total income	12.6	12.6	-.0224
	[1.23]	[1.9]	(.01)
Any self-employment income	.07	.33	.26
	[.26]	[.47]	(.0021)
Any capital income	.87	.9	.02
	[.33]	[.3]	(.0013)
Income tax	97,091	165,398	69,927
	[224,505]	[460,228]	(2,001)
Log income tax	11.2	11.5	.35
	[1.16]	[1.18]	(.01)
Log gross wealth	13.1	12.6	-.4
	[2.6]	[3.28]	(.02)
Log net taxable wealth	12.3	12.8	.56
	[2.46]	[2.77]	(.02)
Has a majority business ownership stake	.04	.24	.21
	[.19]	[.43]	(.0019)
Any transfer abroad	.17	.35	.18
	[.38]	[.48]	(.002)
Any transfer with havens	.02	.04	.03
	[.13]	[.2]	(.0008)
Age	47.9	44	-3.63
	[18.8]	[12.3]	(.06)
Immigrant	.16	.33	.18
	[.37]	[.47]	(.0023)
Female	.49	.18	-.305
	[.5]	[.39]	(.0018)
Married	.4	.4	-.0001
	[.49]	[.49]	(.0023)
University	.32	.26	-.0614
	[.47]	[.44]	(.0022)
Count individual-years	50,140,952	88,264	
Unique individuals	5,769,673	61,769	

Note: This table displays descriptive statistics of tax noncompliance penalties in Panel (a) and the attributes of penalty (non-)recipients in Panel (b), pooling observations from 2011 to 2021. The "Non-penalized" and "Penalized" columns in Panel (b) present means and standard deviations (in hard parentheses) of taxpayer characteristics stratified by penalty recipiency status for a given individual-year (where a given taxpayer can appear in both columns in separate years). The "Difference" column presents the coefficients of a series of univariate regressions of each row variable on an indicator of penalty assignment and year fixed effects, with standard errors clustered at the taxpayer level in soft parentheses. All monetary values are expressed in real NOK 2015.

Table A.2: Covariate balance by penalty amount penalized individuals

Dependent variable	Log penalty amount: Unsaturated	Log penalty amount: Controls	Non-zero penalty amount: Unsaturated	Non-zero penalty amount: Controls
Corrected income	291,907	324,694	-93,203	-96,670
	(20,240)	(21,131)	(53,659)	(54,892)
Log corrected income	.27	-	.8	-
	(.0027)	-	(.04)	-
Any self-employment income	.11	.12	.26	.23
	(.0016)	(.0016)	(.01)	(.01)
Any capital income	.01	-.0023	.02	-.002
	(.001)	(.0004)	(.0049)	(.0015)
Log gross wealth	-.0166	-	-1.2	-
	(.01)	-	(.05)	-
Log net taxable wealth	.36	.02	-1.09	-.111
	(.01)	(.01)	(.06)	(.02)
Has a majority business ownership stake	.04	.04	-.168	-.142
	(.0014)	(.0014)	(.01)	(.01)
Any transfer abroad	.01	.01	-.0688	-.0782
	(.0016)	(.0017)	(.01)	(.01)
Any transfer havens	.01	.01	-.0299	-.0261
	(.0007)	(.0007)	(.0034)	(.0036)
Age	.51	.52	-1.46	-.486
	(.04)	(.04)	(.18)	(.19)
Immigrant	-.0229	-.0278	-.006	-.0563
	(.0018)	(.0018)	(.01)	(.01)
Female	-.048	-.0521	.02	.02
	(.0014)	(.0015)	(.01)	(.01)
Married	-.0018	-.003	-.0633	-.0571
	(.0018)	(.0019)	(.01)	(.01)
University education	-.0096	-.008	-.0771	-.0515
	(.0017)	(.0017)	(.01)	(.01)

Independent variable: Log penalty amount; Non-zero penalty amount.

Note: This table displays results from a series of regressions where of each respective row variable on the logarithm of penalty amount or an indicator for observing a non-zero penalty amount, respectively. All regressions use a pooled sample of penalized taxpayers from 2011 to 2021 and include year fixed effects and feature both an unsaturated version and a variant with log total corrected income and log gross wealth as control variables. Standard errors clustered at the taxpayer level are in parentheses. All monetary values are expressed in real NOK 2015.

Table A.3: Descriptive statistics of tax noncompliance penalties by penalty type (2015)
Panel (a): Ordinary penalties

	Mean	SD	p10	p50	p90	N
Penalty amount	8,742	21,807	0	2,000	23,000	12,300
Penalty amount (log)	8.93	1.18	7.6	8.85	10.4	6,911
Positive penalty amount (binary)	.56	.5	0	1	1	12,300
Penalty share of income (winsorized)	.03	.08	0	.0038	.08	11,048

Panel (b): Elevated penalties

	Mean	SD	p10	p50	p90	N
Penalty amount	7,878	34,895	0	0	14,000	1,349
Penalty amount (log)	10.5	1.13	8.99	10.5	12.1	160
Positive penalty amount (binary)	.12	.32	0	0	1	1,349
Penalty share of income (winsorized)	.04	.15	0	0	.03	1,213

Note: This table displays descriptive statistics of 2015 tax noncompliance penalties, stratifying by ordinary/elevated penalty status in Panels (a) and (b) respectively. Effective penalty rate is calculated as penalty amount divided by gross income, dropping negatively-valued observations and observations in excess of one.

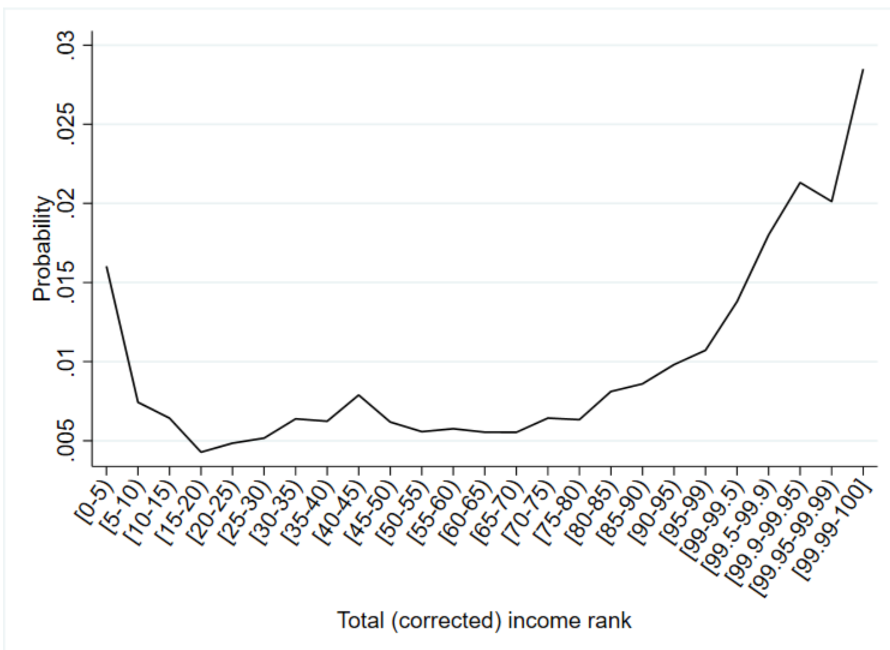
Table A.4: Covariate balance by penalty amount among penalized individuals (2015)

Dependent variable	Log penalty amount		Non-zero penalty amount	
	Unsaturated	Controls	Unsaturated	Controls
Corrected income	152,973	167,895	-61,112	-125,580
	(13,402)	(12,459)	(36,966)	(40,809)
Log corrected income	.18	-	.83	-
	(.01)	-	(.04)	-
Any self employment income	.22	.22	.36	.34
	(.0046)	(.01)	(.01)	(.01)
Any capital income	-.0005	-.005	-.0229	-.0043
	(.0035)	(.0016)	(.01)	(.0024)
Log gross wealth	-.427	-	-1.37	-
	(.04)	-	(.06)	-
Log net taxable wealth	.08	-.074	-1.13	-.12
	(.05)	(.02)	(.08)	(.03)
Has a majority business ownership stake	.0001	.01	-.213	-.189
	(.0036)	(.004)	(.01)	(.01)
Any transfer abroad	-.0136	-.0118	-.0712	-.0895
	(.005)	(.01)	(.01)	(.01)
Any transfer havens	.0025	.0032	-.0202	-.0148
	(.0018)	(.0021)	(.0038)	(.0043)
Age	-.0898	.25	-1.97	-.717
	(.13)	(.14)	(.23)	(.24)
Immigrant	.02	-.0045	-.0044	-.0705
	(.01)	(.01)	(.01)	(.01)
Female	-.0502	-.0474	-.0056	.0016
	(.0042)	(.0043)	(.01)	(.01)
Married	-.0166	-.0088	-.0989	-.0757
	(.01)	(.01)	(.01)	(.01)
University	-.0386	-.033	-.0624	-.0377
	(.0049)	(.0049)	(.01)	(.01)

Independent variable: Log penalty amount; Non-zero penalty amount.

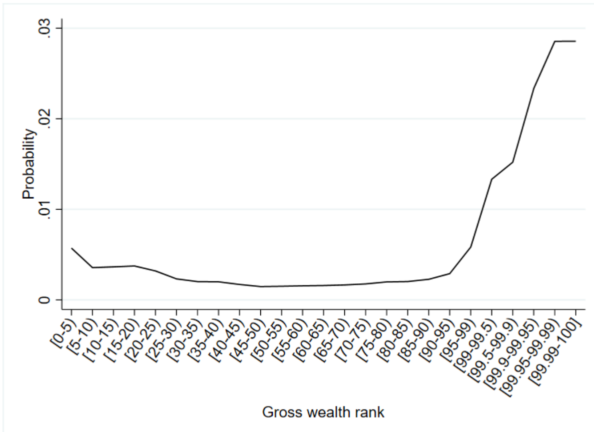
Note: This table displays results from a series of regressions of each respective row variable on the logarithm of penalty amount or an indicator for observing a non-zero penalty amount, respectively. All regressions use the universe of penalized taxpayers in 2015 and feature both an unsaturated version and a variant with log total corrected income and log gross wealth as control variables. Standard errors clustered at the taxpayer level are in parentheses. All monetary values are expressed in real NOK 2015.

Figure A.1: Penalty assignment v. income rank
Conditional on audit flag

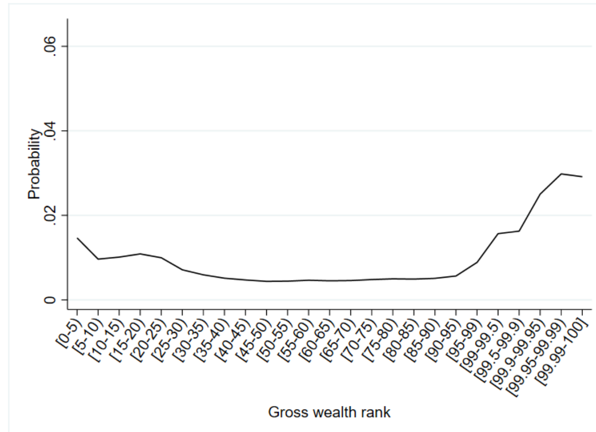


Note: This figures plots the likelihood of penalty assignment within (corrected) income rank bin conditional on being flagged by the tax authorities for irregularities. Income ranks are calculated following correction for detected underreporting.

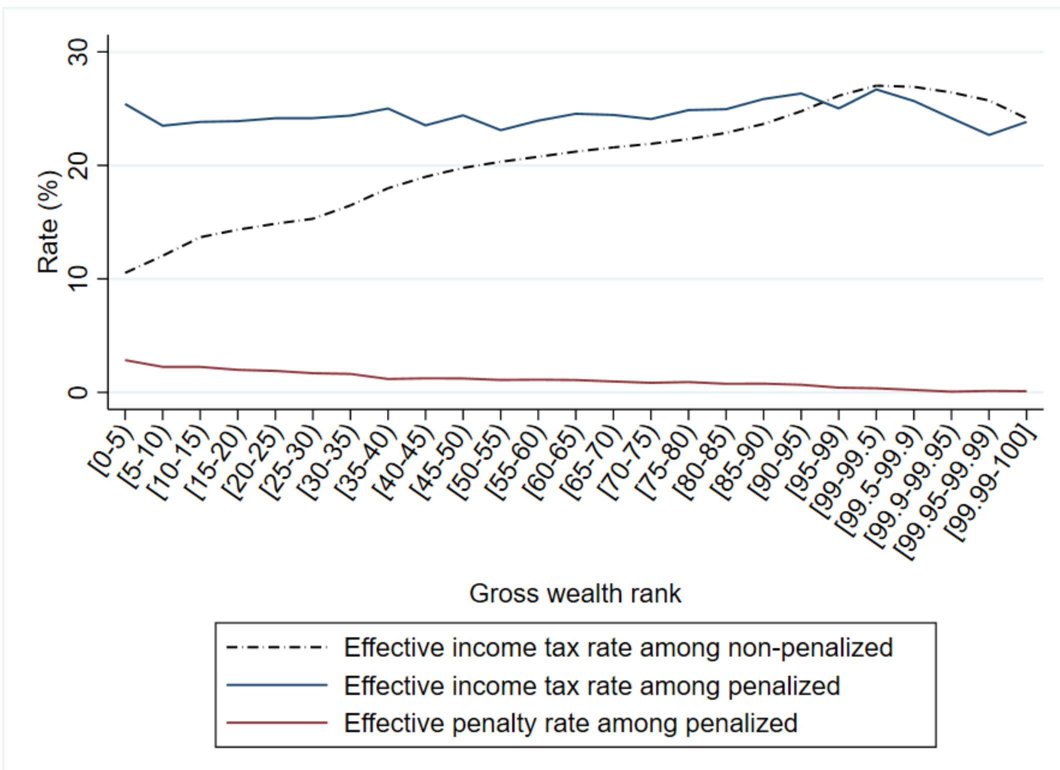
Figure A.2: Distributional characteristics of penalty receipt and effective penalty rates
By wealth rank ((2015))



Panel (a): Penalty assignment v. gross wealth rank



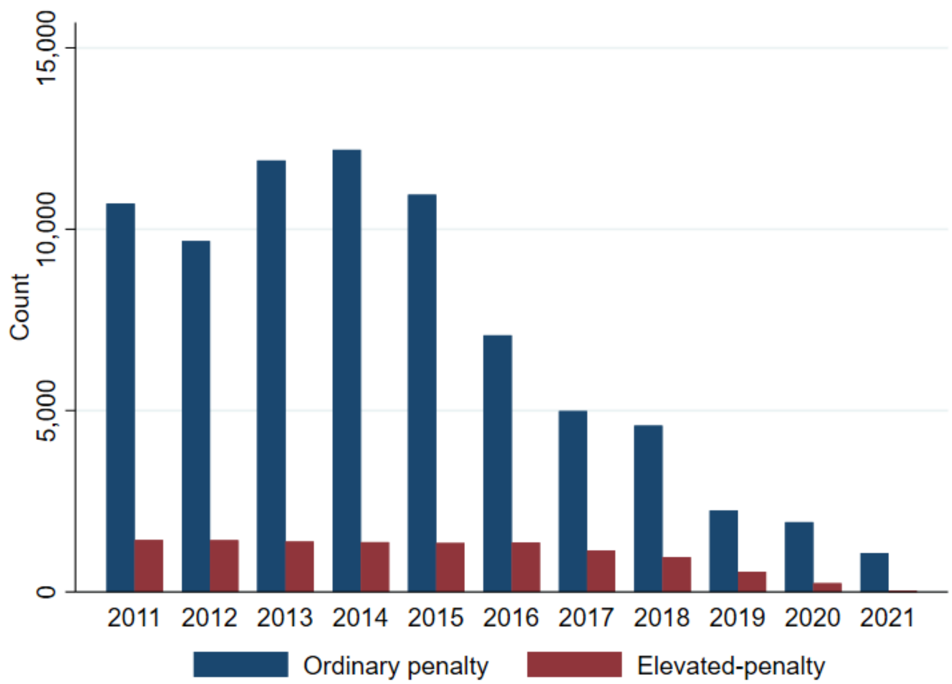
Panel (b): Penalty assignment v. gross wealth rank conditional on flag



Panel (c): Effective income tax rates and penalty rates by gross wealth rank

Note: These figures plot the distributional characteristics of penalty recipiency by income rank for taxpayer-years between 2011 and 2015. Income ranks are calculated following correction for detected underreporting and are based on pooled observations deflated to NOK 2015. Effective penalty rates refers to penalty amount divided by positive total income, with observations with effective penalty rates in excess of one excluded.

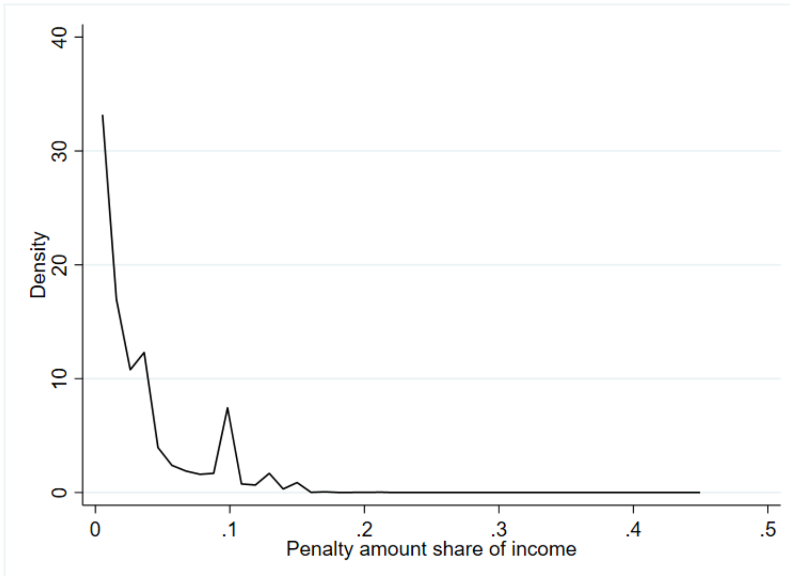
Figure A.3: Penalties by type and year



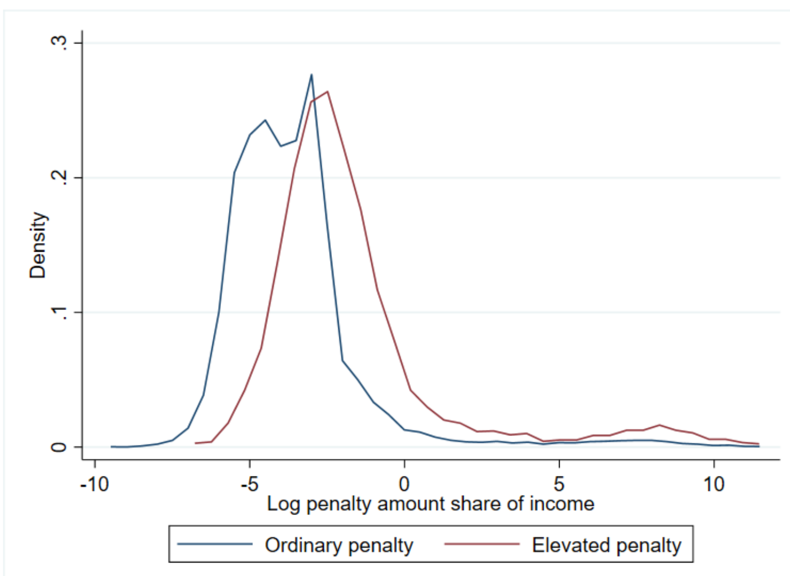
Note: This figure displays the occurrence of ordinary rate- and elevated penalty assignments by year over the 2011-2021 period.

Figure A.4: Distribution of effective penalty rates

(a) Any penalty (< 100%)

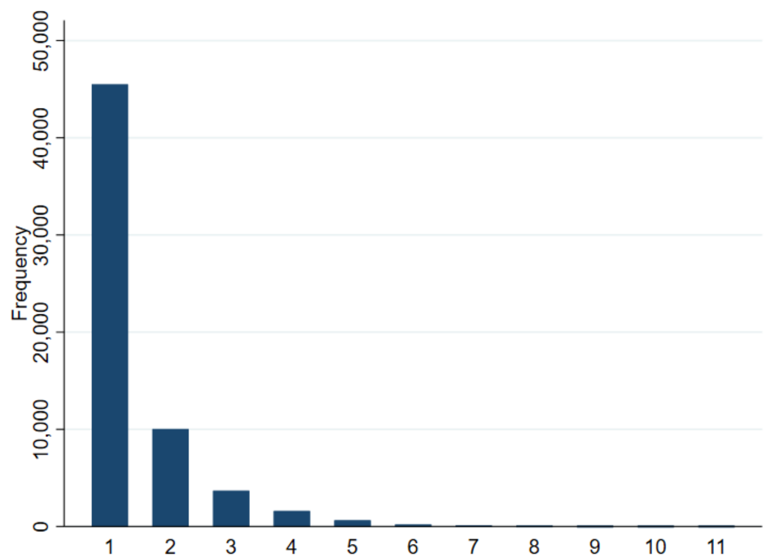


(b) Log effective penalty rate, by ordinary/elevated statutory rate



Note: These figures plot the distribution of observed empirical effective penalty rates from 2011 to 2021. Effective penalty rate is defined as income tax penalty amount divided by the sum of gross income and imputed underreported income. Panel (a) plots all penalty rates, pooling all penalty regimes. Panel (b) plots the logarithm of the observed effective penalty rate, gross of underreporting correction, without restriction of the value, stratifying between ordinary and elevated penalty rate regimes.

Figure A.5: Number of penalties assigned per taxpayer (11 years)



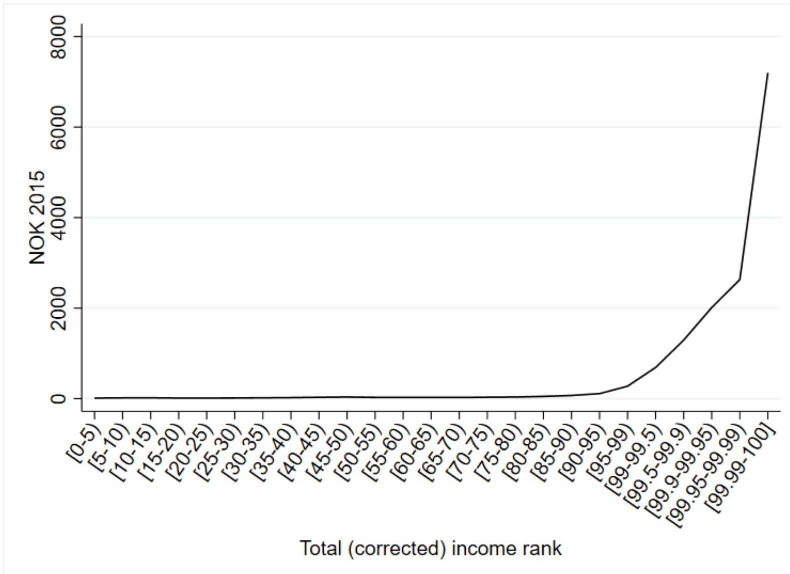
Note: This figure shows the frequency of number of years in which an individual received any tax noncompliance penalty over the 2011-2021 period.

Figure A.6: Conditional distribution of penalty size

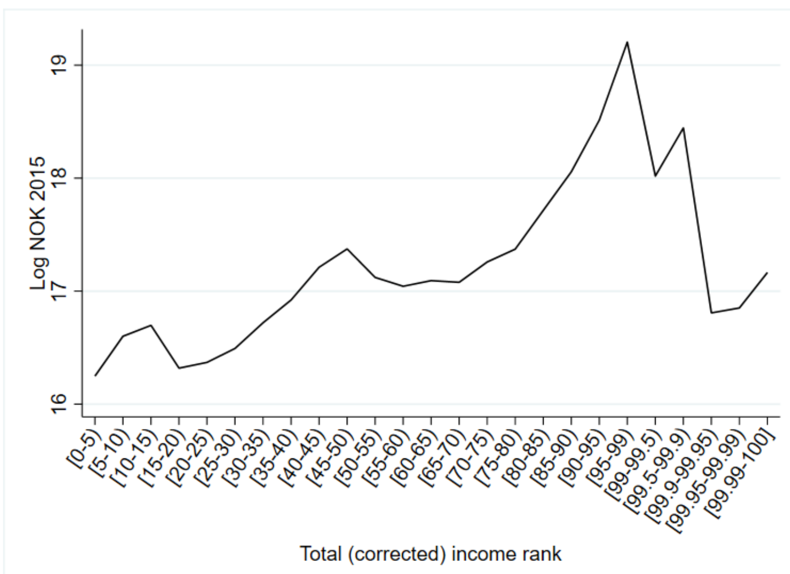
Note: This figure shows the distribution of log penalty size along income rank conditional on non-zero penalty recipiency from 2011 to 2015, stratifying by penalty type.

Figure A.7: Penalties raised

(a) Per capita penalty



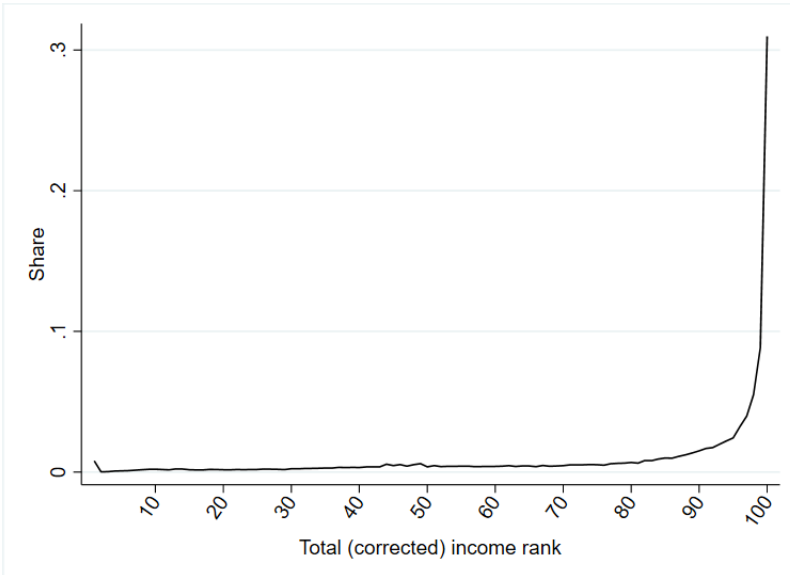
(b) (Log) aggregate penalty



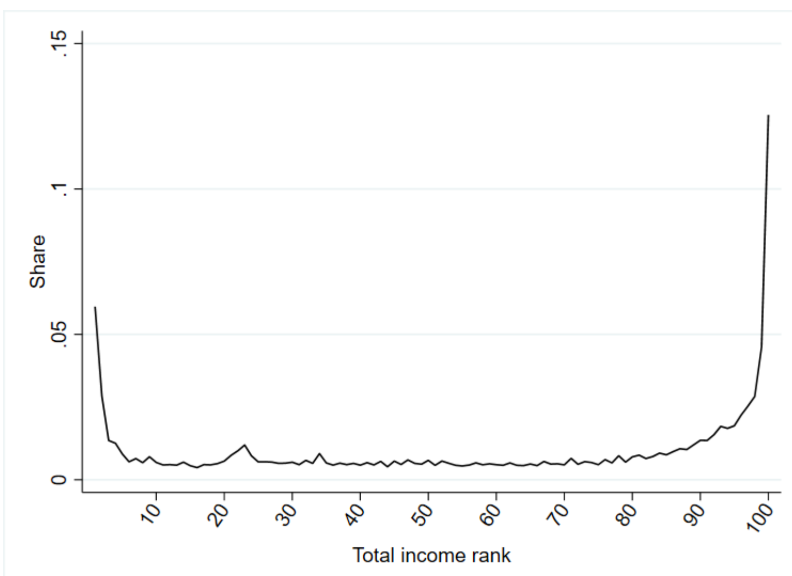
Note: These figures plot the distribution of penalties raised in terms of NOK 2015 from 2011-2015. Panel (a) plots the unconditional average penalty assigned per capita within income rank bin, including non-penalized individuals. Panel (b) plots the log sum of penalties within income rank bins.

Figure A.8: Distribution of aggregated income corrections as a share of all corrections

(a) By corrected income rank



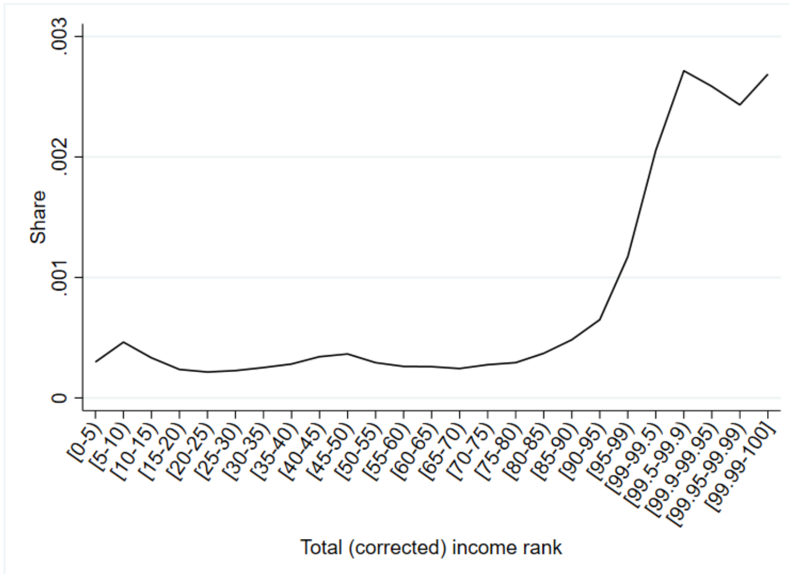
(b) By pre-correction income rank



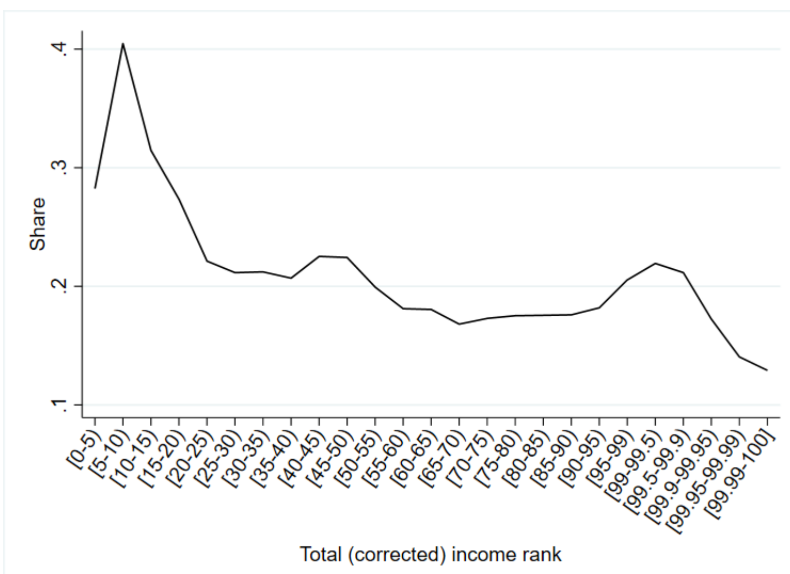
Note: These figures plot the distribution of the share of corrected (imputed underreported) income as a share all income corrections (initial total plus correction), pooling observations from 2011 to 2021. Panel (a) plots these shares after correcting for income rank; Panel (b) shows the result before adjusting income ranks for underreporting. penalty rates, pooling all penalty regimes.

Figure A.9: Underreported income share by corrected income rank

(a) Aggregate within corrected income rank, including non-penalized



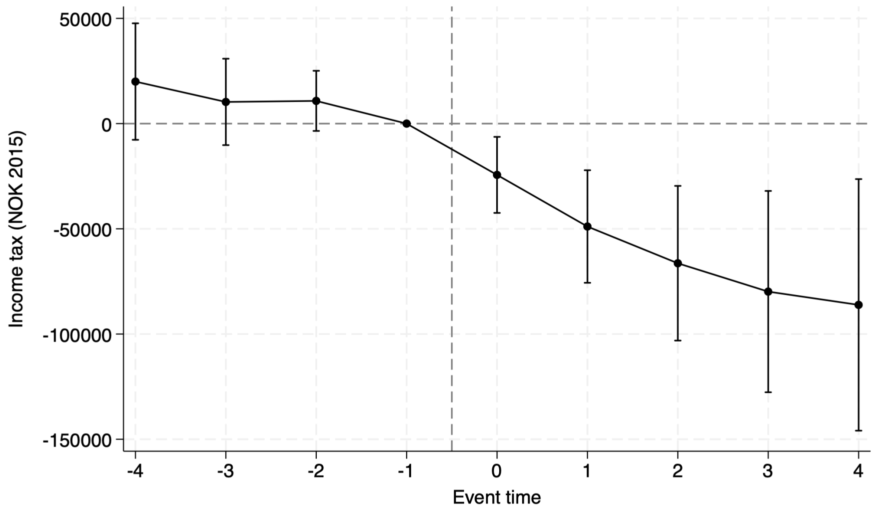
(b) Conditional on penalty



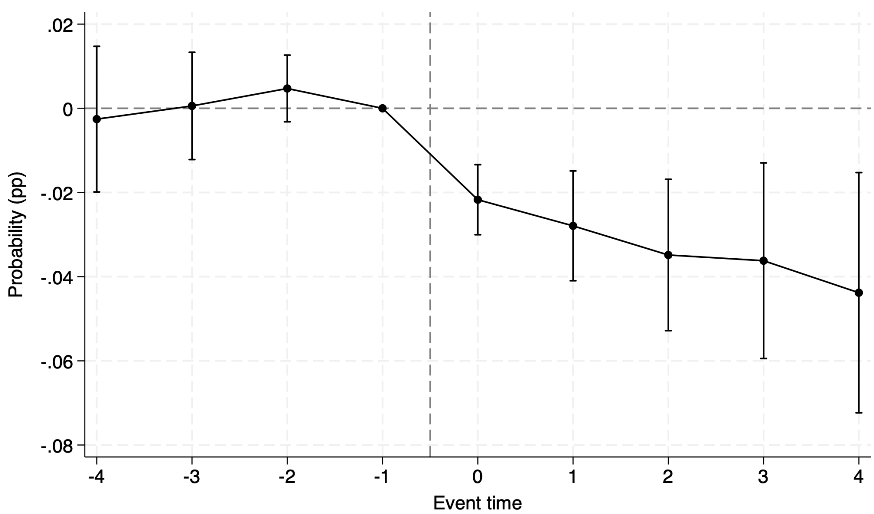
Note: These figures plot the distribution of the share of corrected (imputed underreported) income as a share total corrected income (initial total plus correction) within each income rank bin, pooling observations from 2011 to 2021. Panel (a) plots these shares, including individuals that are not penalized, thereby facing zero income correction. Panel (b) conditions on penalty receipt.

Appendix B Additional results for the 2015-2016 penalty reform

Figure B.1: Additional income tax responses to the decrease in statutory penalty rate



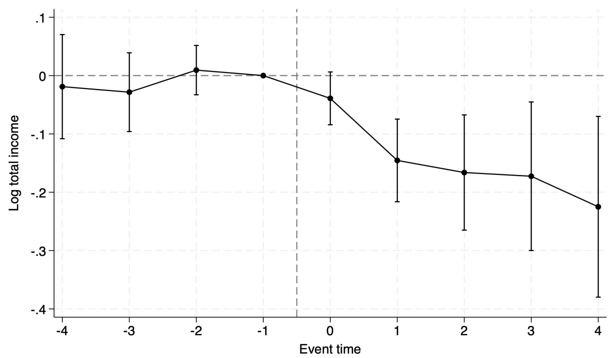
Panel (a): Levels income tax



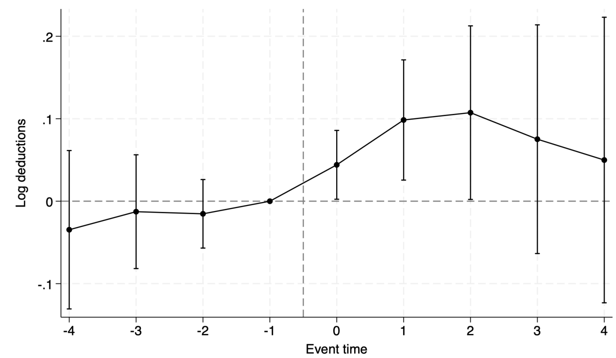
Panel (b): Pays positive income tax

Note: These figures plot the coefficients from the difference-in-differences event study design, determining the impact of the statutory penalty rate decrease's impact on taxpayers' ex-post behavior. The designs both use stratified two way fixed effects. The dashed gray line demarcates the pre- and post-event periods. Confidence intervals are constructed using standard errors clustered on the taxpayer level.

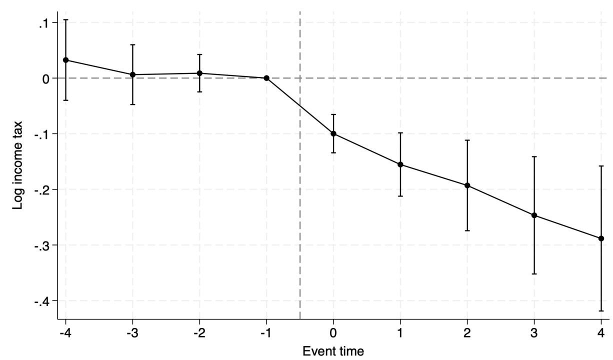
Figure B.2: Net effects of the penalty rate reform:
Difference-in-differences event study estimates



Panel (a): Total income



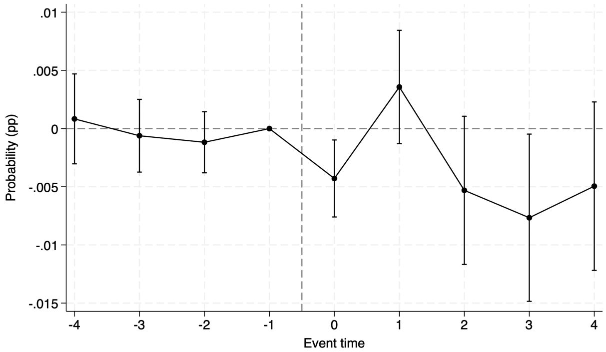
Panel (b): Deductions



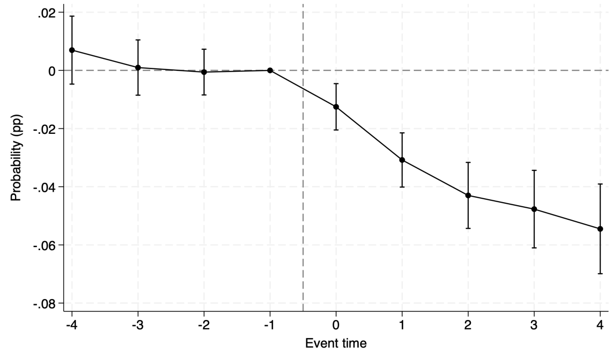
Panel (c): Income tax paid

Note: These figures plot the coefficients from the difference-in-differences event study design, determining the impact of the statutory penalty rate decrease's impact on taxpayers' ex-post behavior. The designs all use stratified two way fixed effects on the taxpayer and post-reform status $\times$ year level. The dashed gray line demarcates the pre- and post-event periods. Confidence intervals are constructed using standard errors clustered on the taxpayer level.

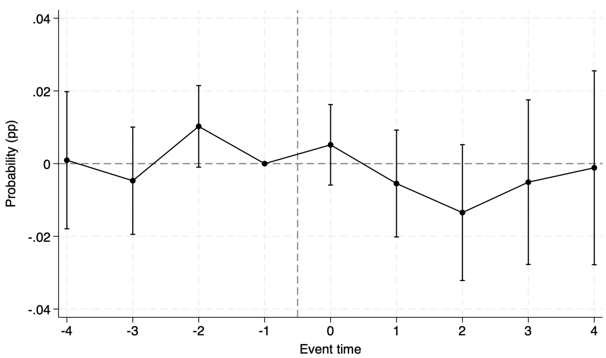
Figure B.3: Auxiliary responses to the decrease in statutory penalty rate



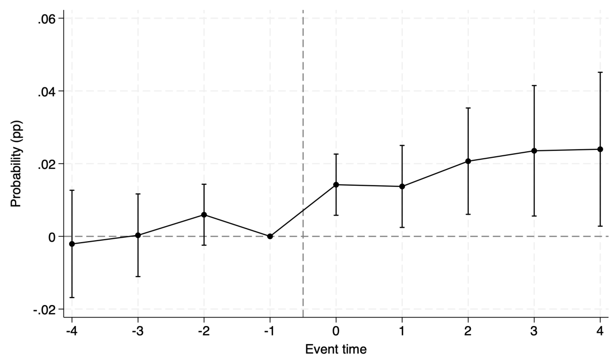
Panel (a): Bankruptcy



Panel (b): Experiences a year-to-year loss in majority business ownership



Panel (c): Move within municipality



Panel (d): Move to different municipality

Note: These figures plot the coefficients from the difference-in-differences event study design, determining the impact of the statutory penalty rate decrease's impact on taxpayers' ex-post behavior. The designs all use stratified two way fixed effects on the taxpayer and post-reform status $\times$ year level. The dashed gray line demarcates the pre- and post-event periods. Confidence intervals are constructed using standard errors clustered on the taxpayer level.

Table B.1: First-stage effect of the penalty rate reform on penalty size

	(1) Log penalty	(2) Effective penalty rate	(3) Log effective penalty rate
Year $\in [2016, 2018]$	-0.21	-0.014	-0.50
	(0.036)	(0.0020)	(0.033)
Year $\in [2019, 2021]$	0.64	0.0015	0.20
	(0.030)	(0.0025)	(0.030)
Constant	8.87	0.056	-3.86
	(0.039)	(0.0010)	(0.052)
Years	All Years	All Years	All Years
Observations	51,904	52,340	47,853
F-statistic	2,158	54.6	358
Adjusted R-squared	.0257	.00127	.0145

Standard errors in parentheses

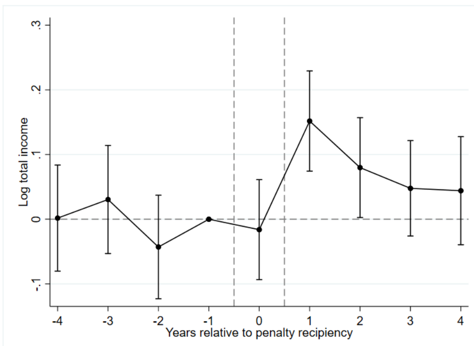
Note: This table displays results from a series of regressions with different parameterizations of penalty size as dependent variables on binary indicators of whether the penalty assignment occurred within the time span 2016-2018 and 2019-2021, respectively. Standard errors clustered on the year level are included in parentheses.

Table B.2: IV event study difference-in-differences:
Impact of penalty rate on declared income and taxes paid (penalty rate reform IV)

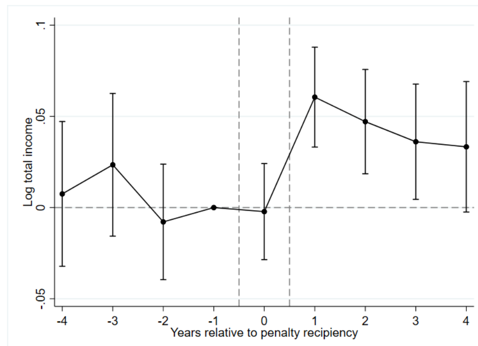
	Log income				Log taxes paid			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform × Post-event	-0.072				-0.092			
	(0.022)				(0.015)			
Log penalty × Post-event		0.50				0.65		
		(0.16)				(0.12)		
Effective penalty rate × Post-event			0.090				0.16	
			(0.046)				(0.040)	
Log effective penalty rate × Post-event				0.21				0.25
				(0.061)				(0.043)
Specification	Reduced form	IV: reform	IV: reform	IV: reform	Reduced form	IV: reform	IV: reform	IV: reform
Observations	384,956	384,956	418,842	380,905	360,167	360,167	392,034	356,454
Number of clusters	45,916	45,916	49,420	44,948	45,483	45,483	49,024	44,601
F-statistic	23	20.5	5.96	21.6	184	135	124	185

Note: This table displays results from a series of instrumental variable difference-in-differences events study regressions corresponding with the reduced form $y_{it} = \alpha_i + \delta_{t,d(i)} + \sum_{k=-5}^{5} \gamma_k EventTime_{k(it)} + \sum_{k=-5}^{5} \beta_k \theta_i EventTime_{k(it)} + \varepsilon_{it}$. In each instrumental variables regression, the penalty rate reform serves as an instrument for the change in the respective parameterization of the penalty rate for taxpayers penalized starting in 2016. Effective penalty rate is defined as penalty divided by total corrected income in the year of penalty. All regressions include stratified two way fixed effects on the taxpayer level and on the reform-status × year level. Standard errors clustered on the taxpayer level are included in parentheses.

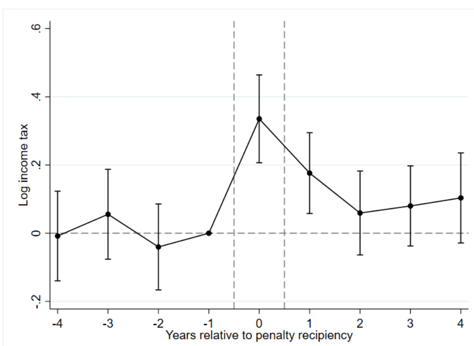
Figure B.4: Event study IV difference-in-differences:
Impact of penalties on declared income and taxes paid



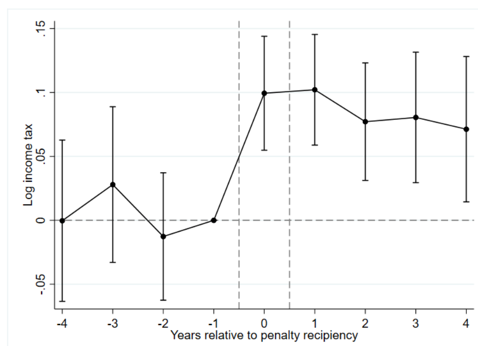
(a) Log income v.
Log penalty amount



(b) Log income v.
effective penalty rate



(c) Log taxes paid v.
Log penalty amount

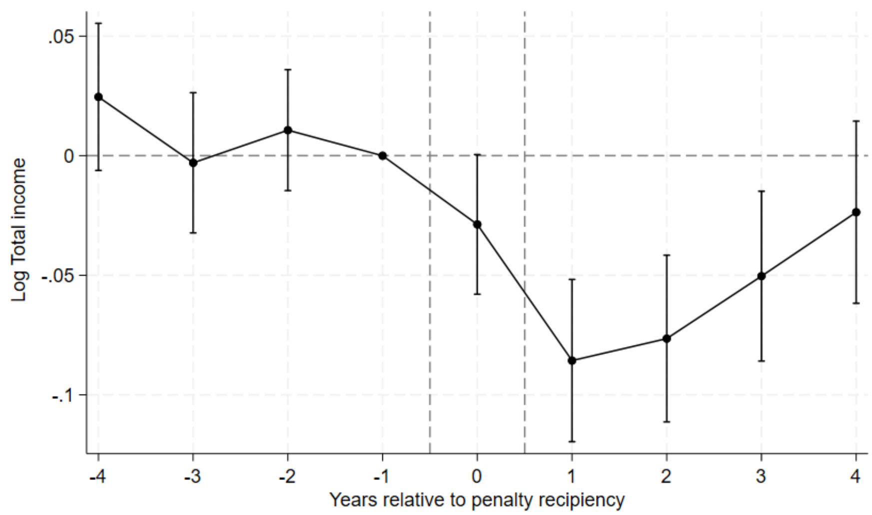


(d) Log taxes paid v.
effective penalty rate

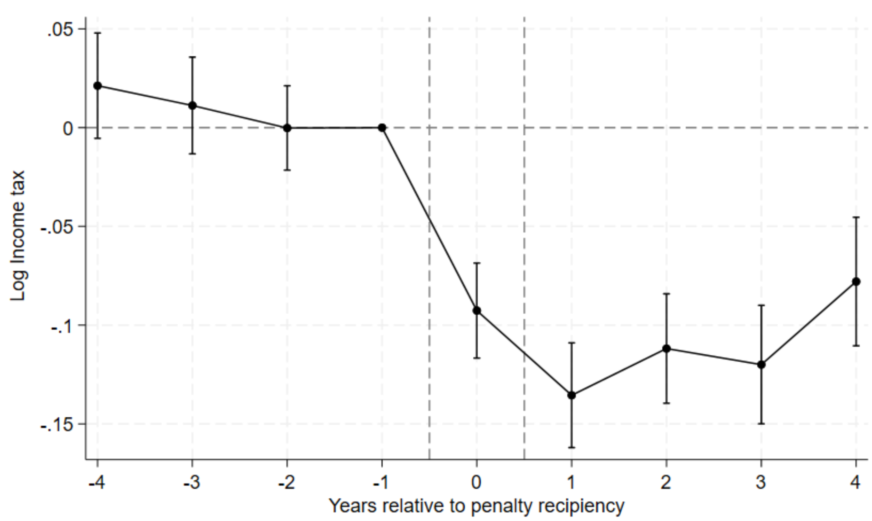
Note: These figures plot the treatment effect coefficients $\{\hat{\beta}_k\}$ and 95% confidence bands associated with the instrumental variables difference-in-differences event study $y_{it} = \alpha_i + \delta_{t,d(i)} + \sum_{k=-5}^{5} \gamma_k EventTime_{k(it)} + \sum_{k=-5}^{5} \beta_k \theta_i EventTime_{k(it)} + \varepsilon_{it}$, omitting the estimation bins. The underlying approach leverages an instrumental variable design, using a binary indicator for penalty receipt during post reform years 2016-2018 as an instrument for the respective penalty variable. The estimating equation includes taxpayer and regime $\times$ year fixed effects. Standard errors are clustered on the taxpayer level.

Figure B.5: Income- and tax payment responses to the decrease in statutory penalty rate
Correlated Random Effects

(a) Income declared



(b) Income taxes paid



Note: These figures display the dynamic effects of the decrease in the statutory penalty rate on the logarithms of total declared income (Panel (a)) and income taxes paid (Panel (b)) using a Correlated Random Effects (CRE) design. Both panels plot the treatment effect coefficients $\{\hat{\beta}_k\}$ and 95% confidence bands associated with the event study $y_{it} = \alpha_0 + \Gamma X_i + \sum_{k=-5}^{5} \gamma_k EventTime_{k(it)} + \sum_{k=-5}^{5} \beta_k Event\ Time_{k(it)} \cdot 1\{Penalty\ Year_i \geq 2016\} + \varepsilon_{it}$. Here, X_i represents a set of time-invariant characteristics consisting of measures of pre-event income. Standard errors are clustered at the taxpayer level.

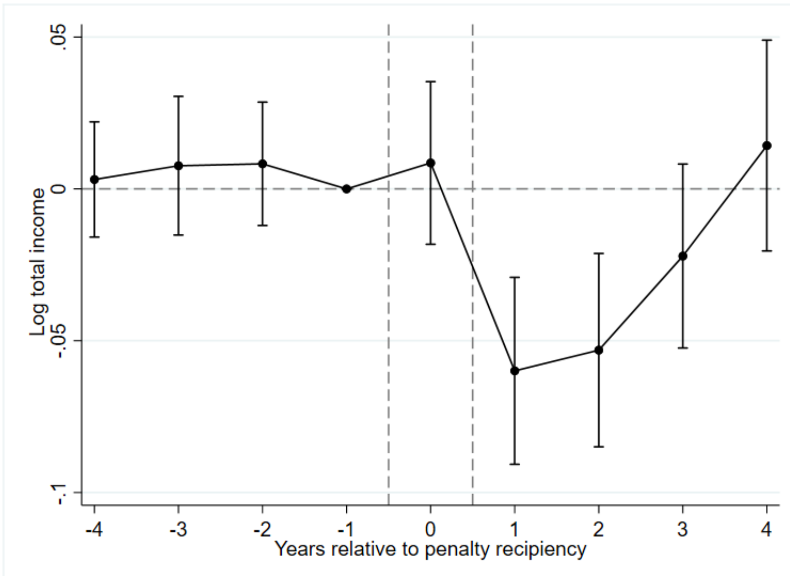
Table B.3: Pre-penalty balance of matching sample of ordinary-rate penalty recipients
Post- v. pre-reform penalty receipients

Dependent variable	Pre-reform penalty recipients	Post-reform penalty recipients	Difference
Log total income	13.3	13.4	.02
	[.69]	.73	(.0018)
Penalty size	13,548	12,938	-609
	[57,141]	32,935	(626)
Effective penalty rate	.02	.02	-.0002
	[.03]	.03	(.0004)
Has self employment income	.26	.23	-.0288
	[.39]	.37	(.01)
Log income tax	11.9	11.9	.0044
	[.91]	.95	(.0023)
Log gross wealth	13.9	14.1	.1
	[2.29]	2.38	(.01)
Any transfer abroad	.29	.33	.04
	[.36]	.37	(.01)
Any transfer havens	.03	.04	.01
	[.13]	.14	(.002)
Has a majority business ownership stake	.29	.4	.11
	[.43]	.46	(.01)
Age	45	45.6	.59
	[11.6]	11.5	(.15)
Immigrant	.18	.18	-.0039
	[.38]	.38	(.01)
Female	.19	.2	.01
	[.39]	.4	(.01)
Married	.51	.5	-.014
	[.48]	.48	(.01)
University	.33	.35	.02
	[.47]	.48	(.01)
Unique taxpayers	36,005	7,201	

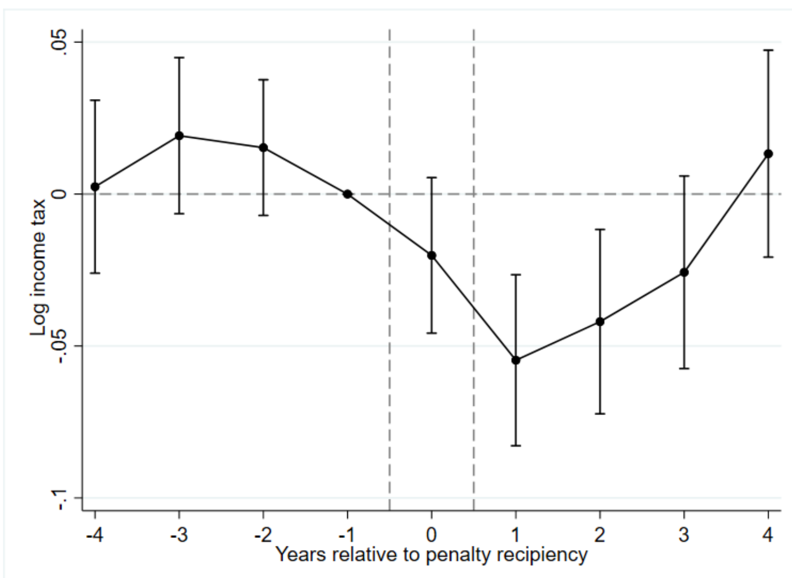
Note: This table displays four-to-one year pre-event average descriptive statistics of Norwegian ordinary-rate personal income tax noncompliance penalty recipients based on whether they received their penalty under the pre-reform 30% statutory rate or the post-reform 20% statutory rate. Here, each post-reform penalty recipient is matched based on pre-event income declared, wealth declared, and taxes paid to five pre-reform ordinary-rate penalty recipients using a Mahalanobis matching procedure with replacement. Hard brackets indicate standard deviations; soft brackets indicate heteroskedasticity-robust standard errors. The difference column does not necessarily correspond with the simple difference between the post- and pre-reform columns, as the regression that computes the difference includes match-group fixed effects. All monetary values are expressed in real NOK 2015.

Figure B.6: Income- and tax payment responses to the decrease in statutory penalty rate
Mahalanobis five nearest neighbor with-replacement matching

(a) Income declared



(b) Income taxes paid



Note: These figures display the dynamic effects of the decrease in the statutory penalty rate on the logarithms of total declared income (Panel (a)) and income taxes paid (Panel (b)) using a Mahalanobis five nearest with-replacement matching procedure. Both panels plot the treatment effect coefficients $\{\hat{\beta}_k\}$ and 95% confidence bands associated with the event study $y_{it} = \alpha_i \sum_{k=-5}^{5} \gamma_k EventTime_{k(it)} + \sum_{k=-5}^{5} \beta_k Event\ Time_{k(it)} \cdot 1\{Penalty\ Year_i \geq 2016\} + \varepsilon_{it}$ omitting the timing bins from the figure. The estimating equation in Panel (b) also contains a group term $\sum_{g \in \mathcal{G}} \zeta_g 1\{g(i) = g\}$ to represent the match effect. Standard errors are clustered at the taxpayer level.

Table B.4: Alternate specifications for penalty rate reform:
Correlated random effects controlling for income, wealth, and penalty amount

	Log income declared			Log income taxes		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-reform × Post-event	-0.075	-0.068	-0.078	-0.10	-0.12	-0.13
	(0.013)	(0.012)	(0.012)	(0.0098)	(0.010)	(0.010)
Constant	12.8	2.77	3.10	10.3	2.64	2.15
	(0.045)	(0.070)	(0.069)	(0.039)	(0.12)	(0.12)
Income controls	No	Yes	Yes	No	Yes	Yes
Penalty size controls	Yes	No	Yes	Yes	No	Yes
Wealth controls	No	No	Yes	No	No	Yes
Observations	328,289	343,479	303,648	310,682	325,439	288,521
Number of clusters	37,735	38,809	34,309	37,668	38,775	34,282
F-statistic	293	7,397	5,231	612	1,381	1,803
Adjusted R-squared	.013	.39	.39	.036	.33	.35

Note: This table displays results from a series of difference-in-difference event study designs evaluating the impact of the decrease in penalty rate on penalized individuals. The columns report coefficients estimated from correlated random effects models corresponding with a period-coarsened version of the difference-in-differences event study design given by the reduced form Equation (4), however replacing the taxpayer fixed effect with taxpayer-fixed characteristic indicated in the bottom half of the table. Individual-clustered standard errors are included in parentheses.

Table B.5: Alternate specifications for penalty rate reform:
Correlated random effects controlling for audit risk score
Impacts on log reported income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform × Post-event	-0.065	-0.063	-0.073	-0.068	-0.079	-0.078	-0.088	-0.084
	(0.014)	(0.013)	(0.013)	(0.012)	(0.014)	(0.013)	(0.014)	(0.012)
Constant	12.7	12.8	2.84	2.82	13.0	12.8	3.14	3.07
	(0.0090)	(0.0068)	(0.077)	(0.070)	(0.050)	(0.046)	(0.079)	(0.072)
Risk score control	Log score	Score rank	Risk score	Score rank rank	Risk score	Score rank	Risk score	Score rank
Income controls	No	No	Yes	Yes	No	No	Yes	Yes
Penalty size controls	No	No	No	No	Yes	Yes	Yes	Yes
Observations	288,252	360,885	273,902	343,448	269,164	328,224	255,866	312,488
Number of clusters	33,152	41,458	30,966	38,803	30,962	37,710	28,938	35,324
F-statistic	322	177	5,728	4,894	280	154	5,045	4,109
Adjusted R-squared	.016	.022	.39	.39	.02	.024	.39	.38

Note: This table displays results from a series of difference-in-difference event study designs evaluating the impact of the decrease in penalty rate on penalized individuals. The columns report coefficients estimated from correlated random effects models corresponding with a period-coarsened version of the difference-in-differences event study design given by the reduced form Equation (4), however replacing the taxpayer fixed effect with taxpayer-fixed characteristics indicated in the bottom half of the table. The dependent variable in all columns in log total income declared. “Log score” corresponds with the logarithm of the risk score assigned to a given tax return (excluding all returns with a risk score of zero), and “Score rank” controls for categories of within-year risk score percentiles and percentiles of the top 1%. Individual-clustered standard errors are included in parentheses.

Table B.6: Alternate specifications for penalty rate reform:
Correlated random effects controlling for audit risk score
Impacts on log income taxes paid

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform × Post-event	-0.096	-0.097	-0.13	-0.12	-0.10	-0.10	-0.14	-0.13
	(0.011)	(0.0097)	(0.011)	(0.010)	(0.011)	(0.0098)	(0.011)	(0.010)
Constant	11.4	11.5	2.91	2.69	10.3	10.3	2.17	2.05
	(0.0074)	(0.0058)	(0.13)	(0.12)	(0.042)	(0.040)	(0.13)	(0.12)
Risk score control	Log score	Score rank	Risk score	Score rank rank	Risk score	Score rank	Risk score	Score rank
Income controls	No	No	Yes	Yes	No	No	Yes	Yes
Penalty size controls	No	No	No	No	Yes	Yes	Yes	Yes
Observations	272,412	341,535	259,101	325,412	254,337	310,627	242,035	296,083
Number of clusters	33,097	41,378	30,945	38,769	30,915	37,645	28,920	35,296
F-statistic	403	291	1,072	1,136	471	448	1,181	1,275
Adjusted R-squared	.023	.027	.33	.33	.041	.046	.33	.34

Note: This table displays results from a series of difference-in-difference event study designs evaluating the impact of the decrease in penalty rate on penalized individuals. The columns report coefficients estimated from correlated random effects models corresponding with a period-coarsened version of the difference-in-differences event study design given by the reduced form Equation (4), however replacing the taxpayer fixed effect with taxpayer-fixed characteristics indicated in the bottom half of the table. The dependent variable in all columns in log income taxes paid. “Log score” corresponds with the logarithm of the risk score assigned to a given tax return (excluding all returns with a risk score of zero), and “Score rank” controls for categories of within-year risk score percentiles and percentiles of the top 1%. Individual-clustered standard errors are included in parentheses.

Table B.7: Alternate specifications for penalty rate reform:
Five nearest neighbor Mahalanobis matching

	(1) Log total income	(2) Log income tax
Post-reform × Post-event	-0.026	-0.035
	(0.011)	(0.011)
Constant	13.3	11.9
	(0.0028)	(0.0027)
ID fixed effects	Yes	Yes
Observations	419,448	414,114
Number of clusters	24,020	24,020
F-statistic	32	15
Adjusted R-squared	.51	.62

Note: This table displays results from a series of difference-in-difference event study designs evaluating the impact of the decrease in penalty rate on penalized individuals. The columns report coefficients estimated from models corresponding with a period-coarsened version of the difference-in-differences event study design given by the reduced form Equation (4), however implementing a Mahalanobis matching procedure of each post-reform taxpayer with five pre-reform taxpayers with replacement based on pre-event declared income, wealth, and taxes paid. Individual-clustered standard errors are included in parentheses.

Table B.8: Spillovers effects of the penalty rate decrease

Panel (a): Spillovers onto family members

	(1) Log total income	(2) Log income tax
Post-reform × Post-event	-0.071	-0.093
	(0.012)	(0.010)
Constant	12.6	11.1
	(0.0028)	(0.0023)
ID fixed effects	Yes	Yes
Observations	319,083	293,014
Number of clusters	30,410	29,742
F-statistic	229	562
Adjusted R-squared	.6	.64

Panel (b): Spillovers onto business partners

	(1) Log total income	(2) Log income tax
Post-reform × Post-event	-0.080	-0.066
	(0.018)	(0.019)
Constant	13.1	11.7
	(0.0043)	(0.0046)
ID fixed effects	Yes	Yes
Observations	78,697	76,895
Number of clusters	7,541	7,522
F-statistic	154	145
Adjusted R-squared	.55	.58

Note: These tables display result from a series of difference-in-difference event study designs evaluating the impact of the decrease in penalty rate on the family and business peers of penalized taxpayers. Panel (a) uses the subset of co-equal business partners of penalized individuals; Panel (b) uses adult family members of penalized individuals. Individual-clustered standard errors are included in parentheses.

Appendix C Additional results on appeals and appeals causal design

Table C.1: Balance on observable characteristics: appealed v. non-appealed penalties

	All	Non-appealed	Appealed	Difference
Corrected income	725,482	711,946	1,257,195	463,860
	[1,857,790]	1,643,154	[5,717,874]	(128,374)
Log corrected income	12.9	12.9	13.3	.27
	[1.32]	1.32	[1.46]	(.04)
Log penalty size	9.18	9.17	9.69	.56
	[1.26]	[1.25]	[1.44]	(.05)
Non-zero penalty	.13	.12	.3	.18
	[.33]	[.33]	[.46]	(.01)
Effective penalty rate (winsorized)	.09	.09	.1	.02
	[.21]	[.21]	[.23]	(.01)
Any self employment income	.32	.32	.31	-.0055
	[.47]	[.47]	[.46]	(.01)
Any capital income	.9	.9	.94	.04
	[.3]	[.3]	[.24]	(.01)
Log gross wealth	12.6	12.6	14.1	1.34
	[3.28]	[3.28]	[2.75]	(.09)
Has a majority business ownership stake	.25	.24	.39	.15
	[.43]	.43	[.49]	(.02)
Log net taxable wealth	12.8	12.7	14	.99
	[2.78]	[2.78]	[2.43]	(.1)
Any transfer abroad	.35	.35	.44	.07
	[.48]	[.48]	[.5]	(.01)
Any transfer with havens	.04	.04	.08	.03
	[.2]	[.2]	[.27]	(.01)
Count individual-years	87,468	85,301	2,167	
Unique individuals	61,307	60,287	1,484	

Note: This table displays descriptive statistics on appeal behavior for the sample of penalty recipients during the tax year corresponding with their penalty. The table presents means and standard deviations (in hard parentheses) of taxpayer characteristics stratified by appeal decision. The "Difference" column presents the coefficients of a series of regressions of each row variable on an indicator of appeal status and year fixed effects, with standard errors clustered at the taxpayer level in soft parentheses. All monetary values are expressed in real NOK 2015.

Table C.2: OLS: penalty characteristics and appeals outcomes versus penalty

Panel (a): Log penalty amount

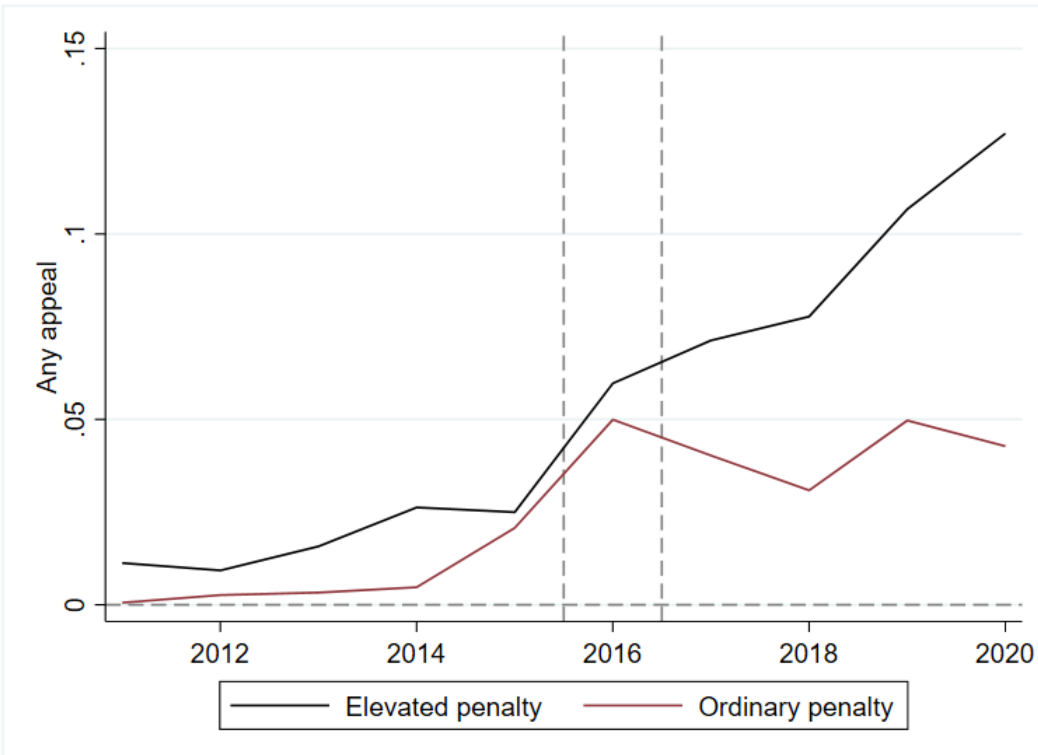
	Time to initial audit decision (log years)		Case has closing		Removed share of penalty		Time to appeal decision (log years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log penalty amount	0.092	0.084	-0.13	-0.14	-0.030	-0.028	0.14	0.17
	(0.014)	(0.013)	(0.0073)	(0.0085)	(0.0079)	(0.0068)	(0.040)	(0.044)
Log income		-0.046		0.030		0.0017		-0.017
		(0.0083)		(0.0052)		(0.0031)		(0.023)
Log gross wealth		0.041		0.021		-0.0086		-0.064
		(0.0037)		(0.0025)		(0.0025)		(0.018)
Constant	0.23	0.33	1.93	1.41	0.38	0.46	-3.16	-2.38
	(0.14)	(0.11)	(0.072)	(0.10)	(0.078)	(0.100)	(0.38)	(0.45)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,634	2,286	2,637	2,286	2,637	2,286	1,000	873
F-statistic	42.4	61.8	323	137	14.1	5.75	11.8	9.39
Adjusted R-squared	.282	.315	.294	.331	.0678	.0684	.015	.0221

Panel (b): Effective penalty rate

	Time to initial audit decision (log years)		Case has closing		Removed share of penalty		Time to appeal decision (log years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effective penalty rate (pp)	0.0040	0.0031	-0.0049	-0.0052	-0.00060	-0.00097	0.010	0.013
	(0.0014)	(0.0015)	(0.00058)	(0.0010)	(0.00031)	(0.00049)	(0.0027)	(0.0041)
Log income		-0.025		-0.0083		-0.0051		0.068
		(0.0062)		(0.0090)		(0.0046)		(0.045)
Log gross wealth		0.046		0.014		-0.011		-0.056
		(0.0035)		(0.0026)		(0.0028)		(0.021)
Constant	1.09	0.78	0.77	0.68	0.11	0.32	-1.94	-2.05
	(0.013)	(0.071)	(0.0054)	(0.13)	(0.0029)	(0.088)	(0.013)	(0.57)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,068	2,039	2,068	2,039	2,045	2,017	818	806
F-statistic	8.06	82.8	71.8	34.5	3.6	4.89	15.1	10.4
Adjusted R-squared	.285	.323	.147	.155	.0376	.0494	.0122	.0132

Note: These tables display results from ordinary least squares regressions of penalty characteristics and appeals outcomes on parameterizations of penalty size. Panel (a) uses log inflation-adjusted penalty amount as the main independent variable; Panel (b) uses effective penalty rate (penalty amount divided by income) winsorized at 0 and 1. Parentheses contain standard errors clustered on the penalty-year level.

Figure C.1: Appeals by type over time



This figure plots the evolution of appeals linked to elevated- and ordinary rate penalties over the 2011-2020 period.

Table C.3: Regressions of binary penalty appeal on penalty size
Controlling for risk score, penalty regime, and taxpayer observable characteristics

	(1)	(2)	(3)	(4)
Log penalty	0.0053	0.0061	0.0054	0.0061
	(0.0018)	(0.0022)	(0.0018)	(0.0021)
Constant	-0.030	-0.036	-0.060	-0.061
	(0.016)	(0.020)	(0.024)	(0.027)
Penalty regime × Year Fixed effects	Yes	Yes	Yes	Yes
Risk score control	Log score	Score rank	Log score	Score rank
Penalty size controls	No	No	Yes	Yes
Income and wealth controls	59,049	75,921	53,633	69,329
Observations	.0316	.0292	.0333	.0316

Note: This table displays results from a series of regressions of the form $Appeal_j = \beta_0 + \beta_1 \log Penalty_j + \Gamma X_j + \varepsilon_{t(j)}$ testing the relationship between penalty and taxpayer characteristics and appeal probability. These regressions also feature controls for audit risk score assigned by the tax authorities to a tax declaration. "Log score" corresponds with the logarithm of the risk score assigned to a given tax return (excluding all returns with a risk score of zero), and "Score rank" controls for categories of within-year risk score percentiles and percentiles of the top 1%.

Table C.4: Impact of the penalty rate decrease on appeal probability
Controlling for risk score

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ordinary regime × Post	-0.038	-0.025	-0.036	-0.024	-0.040	-0.025	-0.037	-0.024
	(0.017)	(0.0093)	(0.017)	(0.0097)	(0.017)	(0.0096)	(0.017)	(0.0099)
Ordinary regime	-0.012	-0.010	-0.014	-0.011	-0.0026	-0.0016	-0.0051	-0.0025
	(0.0039)	(0.0025)	(0.0034)	(0.0025)	(0.0054)	(0.0040)	(0.0044)	(0.0037)
Post	0.077	0.062			0.078	0.065		
	(0.014)	(0.0077)			(0.014)	(0.0076)		
Log penalty					0.0051	0.0063	0.0052	0.0064
					(0.0018)	(0.0023)	(0.0017)	(0.0023)
Constant	0.017	-0.067	0.041	-0.041	-0.038	-0.078	-0.014	-0.052
	(0.0037)	(0.027)	(0.0048)	(0.027)	(0.018)	(0.031)	(0.018)	(0.031)
Fixed effects	None	None	Year	Year	None	None	Year	Year
Risk score control	Log score	Score rank	Log score	Score rank	Log score	Score rank	Log score	Score rank
Penalty size controls	No	No	No	No	Yes	Yes	Yes	Yes
Income and wealth controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	59,049	69,706	59,049	69,706	59,049	69,706	59,049	69,706
Adjusted R2	.0256	.0276	.0278	.0291	.0275	.0296	.0297	.0311

Note: These tables display results from a series of regressions of a binary indicator of appealing a penalty as dependent variable on interactions of penalty regime assignment and a post-2016 year indicator. These regressions also feature controls for audit risk score assigned by the tax authorities to a tax declaration. "Log score" corresponds with the logarithm of the risk score assigned to a given tax return (excluding all returns with a risk score of zero), and "Score rank" controls for categories of within-year risk score percentiles and percentiles of the top 1%. Standard errors clustered on the year-level are included in parentheses.

Table C.5: First-stage for leave-one-out penalty instruments

Panel (a): Parameterizations of leave-one-out mean penalty

	Log penalty		Effective penalty rate		Log effective penalty rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Log of leave out mean penalty	0.852	1.062				
	(0.0089)	(0.011)				
Rate of leave-out mean penalty			0.689	0.684		
			(0.0061)	(0.0061)		
Log rate of leave-out mean penalty					0.991	1.007
					(0.0025)	(0.0025)
Constant	0.816	-1.234	0.447	0.501	-0.646	-0.599
	(0.087)	(0.11)	(0.040)	(0.041)	(0.0084)	(0.0083)
Year fixed effects	No	Yes	No	Yes	No	Yes
Observations	76,192	76,192	80,396	80,396	70,640	70,640
F-statistic	9,157	8,901	12,675	12,494	161,141	167,861
Adjusted R-squared	.114	.131	.583	.588	.729	.735

Panel (b): Leave-one-out mean parameterizations of penalty

	Log penalty		Effective penalty rate		Log effective penalty rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Leave-out mean log penalty	0.998	0.998				
	(0.0089)	(0.0096)				
Leave-out mean penalty rate			0.996	0.995		
			(0.018)	(0.020)		
Leave-out mean log penalty rate					0.997	0.997
					(0.014)	(0.015)
Constant	0.0142	0.0167	0.0312	0.0374	-0.0101	-0.0109
	(0.081)	(0.087)	(0.13)	(0.15)	(0.054)	(0.056)
Year fixed effects	No	Yes	No	Yes	No	Yes
Observations	76,192	76,192	80,396	80,396	70,640	70,640
F-statistic	12,713	10,867	3,224	2,601	4,866	4,384
Adjusted R-squared	.144	.144	.108	.107	.136	.136

Note: These tables display result from a series of first-stage regressions of different parameterizations of penalty amounts on respective parameterizations of leave-one-out means estimators. Panel (a) uses instruments based on parameterizations $g(x)$ of leave-out mean penalty θ as $g(\bar{\theta}_{-i})$; Panel (b) uses leave-one-out mean parameterizations of the parameterized penalty amount: $\bar{g}_{-i}(\theta)$. Heteroskedasticity-robust standard errors are included in parentheses.

Table C.6: IV results: impact of penalty rate on taxpayer-appeal decision (alternate instrument)

	(1)	(2)	(3)	(4)	(5)	(6)
Log penalty	0.0071	0.014	0.015			
	(0.0023)	(0.0039)	(0.0039)			
Penalty rate				0.000094	0.0014	0.0024
				(0.000082)	(0.00030)	(0.00052)
Log gross income			-0.000035			0.018
			(0.00052)			(0.0050)
Log gross wealth			0.0021			0.0019
			(0.00084)			(0.00086)
Constant	-0.044			0.024		
	(0.021)			(0.00068)		
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Specification	OLS	IV	IV	OLS	IV	IV
Observations	76,192	76,192	69,332	80,396	80,396	78,831
Second stage F-statistic	9.55	13.1	6.93	1.31	22.4	18.1

Note: This table displays results from a series of instrumental variable regressions with a binary indicator of appealing a penalty as dependent variable on different parameterizations of penalty severity, using the penalty-rate reform and the leave-out mean penalty rate as instrumental variables (constructed as the leave-one-out mean of the parameterization of the penalty). Standard errors clustered on the year-level are in parentheses.

Table C.7: IV results: Appeal versus penalty size,
By penalty type

	(1)	(2)	(3)	(4)	(5)	(6)
Log penalty	0.0072	0.0082	0.020	0.027	0.014	0.018
	(0.0019)	(0.0022)	(0.0064)	(0.0092)	(0.0037)	(0.0052)
Log gross income		-0.0020		-0.013		-0.0082
		(0.0013)		(0.0065)		(0.0036)
Log gross wealth		0.0016		0.0094		0.0030
		(0.00056)		(0.0023)		(0.00085)
Fixed effects	Year	Year	Year	Year	Penalty type × Year	Penalty type × Year
Specification	IV	IV	IV	IV	IV	IV
Penalty sample	Ordinary	Ordinary	Elevated	Elevated	All	All
Observations	68,141	62,190	8,051	7,520	76,192	69,710
Second stage F-statistic	13.7	4.54	9.94	5.83	14.1	5.72

Note: This table displays results from a series of IV regressions of penalty appeal on log penalty size, instrumenting log penalty size using log leave-out-mean penalty size. Columns estimate this relationship on different subsamples of penalized taxpayers. Columns (1)-(2) isolate ordinary penalty recipients. Panels (3)-(4) use elevated penalty recipients. Panel (5)-(6) pool all penalty recipients, also including stratified regime-year fixed effects. Standard errors clustered at the year level are in parentheses.

Table C.8: IV results: impact of penalty rate on taxpayer-appeal decision:
Municipality × year × Penalty regime leave-one-out mean penalty instrument

	(1)	(2)	(3)	(4)
Log penalty	0.0067	0.0063	0.011	0.012
	(0.0017)	(0.0021)	(0.0030)	(0.0040)
Log implied corrected income		-0.00034		-0.0042
		(0.0017)		(0.0029)
Log gross wealth		0.0021		0.0024
		(0.00031)		(0.00044)
Fixed effects	Yes	Yes	Yes	Yes
Specification	All	All	$N_{trk} \geq 40$	$N_{trk} \geq 40$
Observations	67,957	65,615	50,110	48,268
First stage F-statistic	95.5	80.4	42.1	52.2
Coefficient	-3.59	-3.42	-31.4	-30.5
	(.367)	(.37)	(4.83)	(4.75)

Note: This table displays results from a series of instrumental variable regressions with a binary indicator of appealing a penalty as dependent variable on log penalty amount, using a modified leave-out mean penalty rate as an instrumental variable. In all columns, the instrument is constructed as the leave-one-out mean penalty on the Municipality × Year × penalty regime level. The first stage regresses log penalty amount on the log instrument. Standard errors in parentheses are clustered on the commune × year × penalty regime level.

Table C.9: Appeal versus log penalty size
Taxpayer fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
Log penalty	0.0036	0.0036	0.0042	0.0047	0.0048	0.0057
	(0.0014)	(0.0014)	(0.0019)	(0.0023)	(0.0023)	(0.0030)
Log gross income			-0.0018			-0.0030
			(0.0033)			(0.0036)
Log gross wealth			-0.00021			-0.00021
			(0.00069)			(0.00069)
Constant	-0.015	-0.016	0.0062			
	(0.013)	(0.013)	(0.035)			
Fixed effects	ID + Year	ID + Year+ Penalty order	ID + Year + Penalty order	ID + Year	ID + Year + Penalty order	ID + Year + Penalty order
Specification	OLS	OLS	OLS	IV	IV	IV
Observations	24,306	24,306	22,163	24,306	24,306	22,163
F-statistic	6.28	6.4	2.08	4.23	4.26	1.33

Note: This table displays results from a series of OLS regressions of penalty appeal on log penalty size, including taxpayer fixed effects, thereby isolating variation in penalty size among taxpayers that receive multiple penalties. "Penalty order" fixed effects correspond with categorical variables indicating the ordinal penalty number within a taxpayer's sequence of penalties from 2011 to 2021 (e.g. fixed effects for a taxpayer's 3rd penalty). Standard errors clustered at the taxpayer level are in parentheses.

Appendix D Additional model results

D.1 Additional conceptual model results on the impacts of the greater penalty rate on taxpayer behavior.

D.1.1 Income underreporting, audits, and penalties with two underreporting vehicles and legal expense reimbursement

Consider an identical setup to Section 3, except that for that in the case of appeal, the unsuccessful party pays the legal expenses of the successful party.

The taxpayer can either concede the alleged underpayment plus fee or engage in appeal at cost $\alpha + F$ with probability of success $r(\alpha, \beta; \theta, z)$. The taxpayer ultimately bases her appeal decision and effort (extensive and intensive margins) to maximize payoff:

$$\begin{cases} \textbf{No appeal: } -\Big(T(x+z) - T(x) + \phi(z;x)\Big)\ w.p.\ 1 \\ \\ \textbf{Appeal win: } -F,\ w.p.\ 1 - r(\alpha^*, \beta; \theta, z) \\ \\ \textbf{Appeal loss: } -\Big(T(x+z) - T(x) + \phi(z;x) + F + \alpha + \beta\Big),\ w.p.\ r(\alpha^*, \beta; \theta, z) \end{cases}$$

Conditional on appeal, the taxpayer chooses appeal effort to maximize

$$-F - r(\alpha)(\Lambda + \alpha + \beta)$$

for the stakes of the negative outcomes (from the taxpayer's perspective) $\Lambda := T(x^* + z^*) - T(x^*) + \phi(z^*; x^*)$ and shorthand $r(\alpha) := r(\alpha, \beta; \theta, z)$ for the probability that the taxpayer loses (i.e. that the tax authority wins). Importantly, the taxpayer takes all other arguments in her appeal failure probability, including tax authorities' appeal efforts, as exogenous to her appeal effort. This setup yields a first-order condition for optimal appeal effort:

$$
\begin{aligned}
&\underbrace{\frac{\partial r(\alpha)}{\partial \alpha}(\Lambda+\alpha+\beta)+r(\alpha)}_{-f}=0 \\
&\implies \underbrace{\varepsilon_{r,\alpha^*}}_{<0}\underbrace{(1+\frac{\Lambda+\beta}{\alpha^*})}_{>1}=-1.
\end{aligned}
$$

At the optimal level of appeal on the intensive margin, the product of the elasticity of taxpayer loss probability with respect to taxpayer appeal expenses and a second piece: one plus the ratio of the sum of alleged tax deficiency, penalty, and tax appeal expense to the taxpayer's appeal expense, equals negative one.

As a more direct expression of optimal appeal expense,

$$
\alpha^*=\frac{-\varepsilon_{r,\alpha^*}}{1+\varepsilon_{r,\alpha^*}}\cdot(\Lambda+\beta), \tag{23}
$$

with taxpayer optimal appeal expense increasing in the elasticity of taxpayer loss probability with respect to taxpayer appeal expenses (where $-1<\varepsilon_{r,\alpha^*}<0$ for an interior solution) as well as in alleged tax deficiency, penalty, and tax authority appeal expense. This result here implies that the taxpayer's optimal appeal expense is an affine transformation of the penalty schedule; however, this result also relies on the linearity of disposable income in the taxpayer's utility function.

The second-order condition for this interior solution to produce a maximum is:

$$
\begin{aligned}
&\frac{\partial^2 r(\alpha)}{\partial \alpha^2}|_{\alpha^*}\cdot(\Lambda+\alpha^*+\beta)+2\frac{\partial r(\alpha)}{\partial \alpha}|_{\alpha^*}>0 \\
&\implies \varepsilon_{\frac{\partial r}{\partial \alpha},\alpha^*}<\frac{-2\alpha^*}{\Lambda+\alpha^*+\beta}.
\end{aligned}
$$

This expression reveals that at an optimum the elasticity of the derivative of the appeal loss probability function (from the perspective of the taxpayer) is bounded below $\frac{-2\alpha^*}{\Lambda+\alpha^*+\beta}$.

Applying the implicit function theorem to the first-order condition allows us to gauge the response of optimal appeal effort as the penalty varies (considering a perturbation of the linear component of the penalty rate):

$$\begin{aligned}\frac{\partial \alpha^*}{\partial \phi} &= -\frac{f_\phi}{f_\alpha} \\ &= \frac{-\frac{\partial r(\alpha^*)}{\partial \alpha^*}|_{\alpha^*}}{\frac{\partial^2 r(\alpha)}{\partial \alpha^2}|_{\alpha^*} \cdot (\Lambda + \alpha^* + \beta) + 2\frac{\partial r(\alpha)}{\partial \alpha}|_{\alpha^*}} > 0 \\ &= \frac{-1}{\varepsilon_{\frac{\partial r(\alpha^*)}{\partial \alpha}, \alpha^*}(1 + \frac{\Lambda+\beta}{\alpha^*}) + 2} > 0.\end{aligned}$$

This comparative static generates an important piece of intuition: that optimal appeal effort/expenses is increasing in the penalty rate.

D.1.2 Taxpayer's joint optimization problem for underreporting and litigation

Here we fully develop the taxpayer's joint optimization problem over allocation (z, L, α) for underreporting $z \in \mathbb{R}_+$, extensive margin appeal decision $L \in \{0, 1\}$, and intensive margin appeal effort/expense $\alpha \in \mathbb{R}_+$. To simplify the analysis, consider the agent's joint optimization problem in the presence of only a single underreporting vehicle. An agent optimizes jointly the amount of underreporting, whether to litigate upon detection, and the amount of appeal effort/expense.

Let $L := 1\{F + \alpha^* + r(\alpha^*_{L=1})\Lambda < \Lambda\}$, i.e. whether the taxpayer decides to litigate upon alleged underpayment. Intuitively, the taxpayer decides to litigate if the expected cost of appeal is less than the repayment and fines required in acquiescing to the terms of underpayment ("the stakes"). The taxpayer performs

$$\max_{z,\alpha \in \mathbb{R}_+, L \in \{0,1\}} y - \xi(z) - T(y - z) - \rho\Big((1 - L)\Lambda + L((1 - r(\alpha))F + r(\alpha)\Lambda)\Big).$$

As before, conditional on litigating, the taxpayer expends $\alpha^* = \varepsilon_{\tilde{r}, \alpha^*}\Lambda$ in appeal effort/expense (recall that $r(\alpha)$ and $\varepsilon_{\tilde{r}, \alpha}$ read in the direction of a ruling in favor of the tax authorities).

The central insight that comes with specifying a joint optimization deals with optimal underreporting. Consider the underreporting occurring under two different optima $L = 0$ and $L = 1$. As before, when the taxpayer does not litigate from an ex-ante perspective, optimal underreporting is characterized by the first-order condition:

$$z^*_{L=0} : T'(y - z^*_{L=0}) = \xi'(z^*_{L=0}) + \rho\big(T'(y - z^*_{L=0}) + \phi'(z^*_{L=0})\big).$$

However, in the case where ex-ante, the taxpayer intends on litigating, optimal underreporting is characterized by the alternate first-order condition:

$$z^*_{L=1} : T'(y - z^*_{L=1}) = \xi'(z^*_{L=1}) + \rho r(\alpha)\big(T'(y - z^*_{L=1}) + \phi'(z^*_{L=1})\big). \tag{24}$$

It is clear that $z^*_{L=1} \geq z^*_{L=0}$, with strict inequality for interior solutions. The intuition is straightforward. Litigating serves to lower the probability that the taxpayer will have to pay back any underpayment or penalty, so that the taxpayer compensates by engaging in greater underreporting relative to the no-appeal baseline.

An analogous set of results arises from the government's perspective (aside from the appeal decision, as we do not consider that the government selectively engages in auditing in anticipation of these costs). The benefits of higher penalties in decreasing underreporting and increasing revenue on infra-marginally detected underreporters are weighed down by increased probability of appeal (lower probability of collection) and increased administrative costs from the perspective of the tax authorities.

D.2 Additional optimal tax administration model results

D.2.1 Additional first-order conditions

Tax rate first-order condition. Differentiating Equation (10) with respect to the linear tax rate and applying the envelope theorem gives the necessary condition for an interior optimum:

$$W_t = -(z + r_t) + v' \cdot \Big(z + tz_t + r_e e_t + r_t\Big) = 0. \tag{25}$$

We can modify this expression to give a generalization of the standard inverse elasticity rule:

$$\frac{t}{1-t} = \frac{1}{\varepsilon_{z,1-t}} \cdot \Big(\frac{v'-1}{v'} \cdot (1 + \epsilon_{r,t} \frac{r}{tz}) + \epsilon_{r,e} \varepsilon_{e,t} \frac{r}{tz} \Big). \tag{26}$$

In this expression, $\frac{r}{tz}$ represents the ratio of collections on underreporting to initial pre-audit income tax collections. In the case that the tax administration makes no use of post-audit revenue collection instruments, this expression collapses to the well-known condition: $\frac{1}{1-t} = \frac{1}{\varepsilon_{z,1-t}} \cdot \frac{v'-1}{v'}$. We use $\epsilon_{a,b}$ as opposed to $\varepsilon_{a,b}$ to describe a mechanical elasticity of object a with respect to b that holds agent behavior constant.

This expression shows the standard inverse elasticity result that greater the base elasticity, the lower the optimal tax rate. However, the coefficient on the marginal utility of public goods consumption relative to private consumption is augmented by the fiscal externality of the income tax on collections on discovered underreporting: $\epsilon_{r,t}$, reflecting the fact that mechanically, a higher income tax results in greater collections on infra-marginal successfully detected underreporting. The optimal income tax sees additional upward pressure through the addition of collections on marginal underreporting that is successfully detected $\epsilon_{r,e}\varepsilon_{e,t}$. Note also that by the envelope condition, the increased revenues raised on induced underreporting do not weigh against private welfare changes.

Lastly, take note that the condition to generate a positive optimal income tax rate is slightly relaxed here relative to the standard benchmark $v' > 1$. Here, we have the requirement that $v' > \frac{1}{b+1}$ for $b = \frac{\epsilon_{r,e}\varepsilon_{e,t}}{\frac{tz}{r} + \epsilon_{rt}} = \frac{r_e e_t}{z + r_t} \geq 0$, which weighs the revenue response on marginally-induced detected underreporting against the mechanical increase in collections from an increase in the tax rate. The difference here is that the expression takes into account the fiscal spillovers of successfully captured underreporting. We nonetheless maintain for simplicity the stronger assumption that $v' > 1$, i.e. that government spending is socially desired relative to private consumption.

Note also that for tax instruments $i \in \{\alpha, \xi\}$ where $\xi \in \Xi$, $z_i = -e_i$, because they have no direct effect on earnings and we assume real earnings is exogenous to underreporting activity.

Enforcement instrument first-order condition. Taking first-order conditions with respect to α, we can derive an analogue for the adjusted marginal cost-revenue ratio:

$$W_\alpha = -c_\alpha + v'(tz_\alpha + r_e e_\alpha - a_\alpha) = 0 \tag{27}$$

$$\frac{\alpha \cdot (c_\alpha/v') + \alpha a_\alpha}{(t - r_e)z} = \varepsilon_{z,\alpha} := \phi. \tag{28}$$

In Keen and Slemrod (2017), ϕ refers to the *adjusted marginal cost-revenue ratio*, where the numerator approximates the sum of compliance and administrative costs and the denominator measures the collections on reported income. Equation (28) differs from that in Keen and Slemrod (2017) in that the denominator accounts for the actuarial penalty rate on underreported income (a decrease in revenues relative to the no penalty instruments case). More directly, the optimal level of enforcement should be set at

$$\alpha^* = \frac{(t - r_e)z}{(c_\alpha/v') - a_\alpha} \cdot \varepsilon_{z,\alpha}. \tag{29}$$

This expression differs from the benchmark case without revenue-collection spillovers based on the term $t - r_e$, where a greater actuarial penalty rate relative to the statutory tax rate decreases the optimal level of enforcement. Here, the collections on detected underreported undermine the benefit of discouraging underreporting. Indeed, in the hypothetical scenario that $r_e > t$ (perhaps where underreporting agents are risk loving or exhibit behavioral internalities that prevent them from internalizing that underreporting delivers losses on average), the optimal α is actually negative in order to encourage underreporting (delivering more revenue to the social planner in expectation than via the income tax).

D.2.2 Optimal compliance and recoup gap

We turn to characterizing the optimal compliance gap; this procedure is nearly identical to that in Keen and Slemrod (2017). Take the reporting gap $G = te/twl = e/wl$. Noting that $\varepsilon_{z,\alpha} = -\frac{e}{z}\varepsilon_{e,\alpha}$ and that $e/z = \frac{G}{1-G}$, it follows that

$$\frac{G}{1-G} = \frac{-\varepsilon_{z,\alpha}}{\varepsilon_{e,\alpha}} = \frac{-\phi}{\varepsilon_{e,\alpha}}, \tag{30}$$

where this relationship implies higher optimal underreporting (from an tax administrative perspective) when administrative costs are higher, when underreporting exhibits lower elasticity to enforcement measures, and when collections on declared earnings (relative to the hypothetical collections if those declared earnings in fact went unreported) are lower. One important implication of incorporating collections on underreporting is that the optimal tax gap increases as the actuarial penalty rate on underreported income approaches the linear income tax rate: the tax administration tolerates more underreporting if in expectation more underreported revenues are re-captured.[36]

Consider the recouped collections gap, say $\mathcal{T} := r/e$ To a first-order approximation, if $r = 0$ for $e = 0$, we simply can think of this object as $r_e \cdot e/e = r_e$. From Equation 11 we have that

$$\frac{r(t,e,\xi^*)}{e} = \frac{r(t,e,\xi^*) - r(t,0,\xi^*)}{e-0} \approx r_e(\xi^*) = t + \left(1 - \frac{a_\xi}{r_\xi} - \frac{1}{v'}\right) \cdot \frac{r_\xi}{z_\xi} \tag{31}$$

This relationship gives intuition for the optimal gap on recouped collections. As a baseline when $\frac{a_\xi}{r_\xi} = \frac{v'-1}{v'}$, the optimal collections recoup as share of overall underreporting is precisely equal to the tax rate. However, the optimal recoup share departs from the tax rate based on the marginal social value of the public good and on the marginal cost of increasing instrument ξ relative to marginal collections. When the marginal cost of shifting the instrument at ξ^* is very low or the marginal welfare impact of further spending on the public good is very high, a social optimum features a recoup rate greater than the tax rate (and vice versa).

D.2.3 Complementarity and substitutibility among instruments

To what extent do these different tax administration instruments operate as strategic complements or substitutes in optimality? This question is relevant if one or multiple instruments cannot be feasibly adjusted (perhaps due to political economy constraints). To illustrate, Keen and Slemrod (2017) pose the ask: if α is constrained below its optimum, is

[36] An identical relationship between z and e holds for instrument ξ, such that an analogous condition arises with nearly identical interpretation:

$$\frac{G}{1-G} = \frac{\epsilon_{r\xi}((1-\frac{a_\xi}{r_\xi}) - \frac{1}{v'})}{-\varepsilon_{e,\xi}} \cdot \frac{r}{z(t-r_e)}.$$

it better to increase the statutory tax rate t to offset the weak administration or is it better to set a lower tax rate to expand the base in light of suboptimally high underreporting? We ask this and related questions in our extended environment.

First, note that following an identical proof to Keen and Slemrod (2017) Equation (14) (as demonstrated in their Footnote 23, replacing α with ξ), there is a symmetry in the taxable income elasticities with respect to the penalty instrument and with the tax rate, that $\frac{\partial \varepsilon_{z,1-t}}{\partial \ln \alpha} = \frac{\partial \varepsilon_{z,\xi}}{\partial \ln(1-t)}$. This relation comes with the convenient implication: "Suppose, for instance, it is known that an increase in the [penalty instrument] reduces the elasticity of taxable income; then it must be the case that an increase in the tax rate increases the [elasticity of reported income with respect to the penalty instrument]."

We ask six questions here pertaining to the strategic interactions of the tax rate, administrative enforcement effort, and penalizing instruments:

1. Is it better to raise funds with ξ, α, or t?
2. If ξ is constrained, (when) should the tax administration raise t? I.e. what is the slope of the best response function $t(\xi)$?
3. If ξ is constrained, (when) should the tax administration raise α? I.e. what is the slope of the best response function $\alpha(\xi)$?
4. If t is constrained, (when) should the tax administration raise ξ?
5. If α is constrained, (when) should the tax administration raise ξ?
6. How should the tax authorities prioritize different dual revenue raising instruments?

Here, we present results for questions (1) and (6). In answering these questions, we assume constant v' for simplicity.

D.2.4 Is it better to raise funds with t, ξ, or α?

Lastly, we turn to the highly practical question: should policymakers finance additional public spending by raising the statutory tax rate, by increasing enforcement, or by increasing the penalty instrument ξ?

First, we follow the approach of Keen and Slemrod (2017) for our penalty instrument. This question is equivalent to determining whether there is a welfare gain from increasing ξ while reducing t so as to leave government spending unchanged. Evaluating $dW = W_t \frac{\mathrm{d}t}{\mathrm{d}\xi}|_g + W_\xi$ yields a rule for when it is better for the social planner to raise funds by increasing ξ rather than increasing t:

$$\frac{z_\xi(t - r_e) + (r_\xi - a_\xi)}{z + r_t + (tz_t + r_e e_t)} > \frac{r_\xi}{z + r_t} \tag{32}$$

Alternatively, as a sufficient but not necessary condition, (using the fact that $e_t < -z_t$) it is better to raise funds using the statutory tax rate t rather than raising ξ if

$$\frac{z_\xi(t - r_e) + (r_\xi - a_\xi)}{z_t(t - r_e) + z + r_t} < \frac{r_\xi}{z + r_t}. \tag{33}$$

Lastly, we turn to the relationship between α and ξ. Analogously, we ask whether there is a welfare gain from increasing ξ while reducing α so as to leave public spending g constant. We evaluate the sign of $dW = W_\alpha \frac{\mathrm{d}\alpha}{\mathrm{d}\xi}|_g + W_\xi$ to yield a simple rule:

$$\frac{(r_\xi - a_\xi) + z_\xi(t - r_e)}{-a_\alpha + z_\alpha(t - r_e)} > \frac{r_\xi}{c_\alpha} \tag{34}$$

This rule states that the social planner should prefer to raise the penalty instrument ξ over the enforcement instrument α when the net marginal revenue ratio of penalties to enforcement exceeds the ratio of privately-borne costs for penalties to enforcement.

D.2.5 The relationship between revenue raising administrative instruments.

Suppose the administration has multiple dual-revenue raising instruments. Consider two such instruments ξ_i and ξ_j (perhaps the audit rate and the penalty rate). For instance, if the audit rate is constrained, should the authorities increase the penalty rate? This question differs from the above in that both of these instruments feature both mechanical and behavioral effects on revenue collections. We can similarly evaluate the welfare impact of increasing ξ_2 while decreasing ξ_1 so as to leave government spending unchanged. We evaluate the sign of $W_{\xi_i} \frac{\partial \xi_1}{\partial \xi_j}|_g + W_{\xi_j}$ in order to determine the complementarity of substitutability of the instruments. Define $R_i := r_i + z_i(t - r_e) - a_i$ as the marginal net fiscal gain from incrementally increasing instrument ξ_i. When the tax administration budget is constrained below the social optimum, all instruments should have equilibrated ratios of marginal net funds raised equal to the ratios of privately-borne marginal costs:

$$\frac{R_{\xi_i}}{R_{\xi_j}} = \frac{r_{\xi_i}}{r_{\xi_j}} \; \forall \; \xi_i, \xi_j \in \Xi. \tag{35}$$

In the above case, when equality does not hold, the social planner should increase ξ_i if the ratio of the marginal net fiscal gains exceed the ratio of privately-borne marginal costs (and vice versa). Empirically, this depends clearly on the infra-marginal revenues associated with increasing each instrument, but also on real marginal administrative costs and behavioral responses of taxpayers to each instrument.

D.2.6 Special cases of optimal penalty rate under specific parametric form

This section highlights special cases of the optimal penalty rate for the parametric form of the penalty function $r(e, t, \Xi) := r(e, t, \rho, \eta, \theta) = \rho\eta(t + \theta)e$.

Case 1. $a_\theta = 0$: we highlight this benchmark as of interest because changing the penalty rate indeed incurs zero nominal cost. Of course, we detail the potential behavioral responses to increased penalty rates that may induce increased administrative expenses, but these have not been empirically verified. In this case, the first-order condition is:

$$\frac{1}{\varepsilon_{e,\theta}} \frac{v' - 1}{v'} = \frac{\Delta}{\rho\eta\theta}. \tag{36}$$

Note that $\varepsilon_{e,\theta} < 0$, and $v' > 1$ necessarily implies a negative actuarial penalty gap when $\alpha_\theta = 0$. More explicitly, we have

$$\theta^* = \frac{t(\frac{1}{d} - 1)}{1 + \frac{1}{\varepsilon_{e,\theta}} \cdot \frac{v'-1}{v'}}. \tag{37}$$

Case 2. $\varepsilon_{z,\theta} = 0$, i.e. taxpayers are unresponsive to the penalty rate, the first-order condition reduces to a perhaps more simple optimality condition:

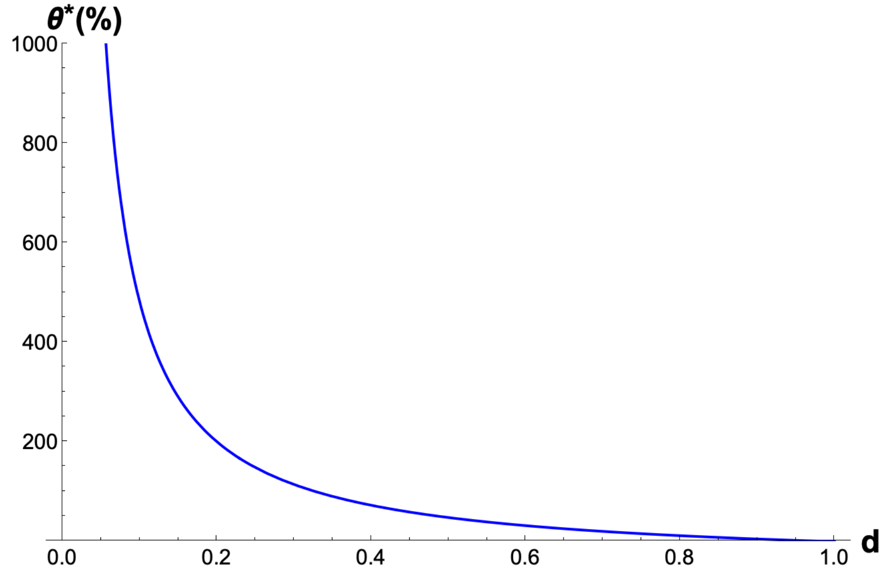
$$a_\theta = D \cdot \frac{v' - 1}{v'}. \tag{38}$$

Case 3. $\rho\eta = 1$; *i.e.* $D = e$, all underreported income is perfectly discovered by the tax authority. Clearly the utility of this case is conceptual, as agents have no reason to underreport income in this case. i.e. taxpayers are unresponsive to the penalty rate, the first-order condition reduces to a perhaps more simple optimality condition:

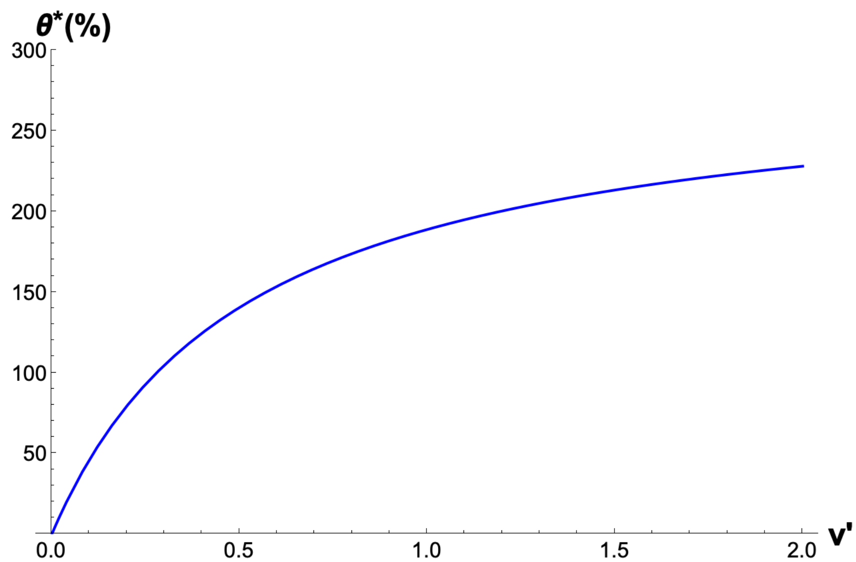
$$\varepsilon_{e,\theta} = \frac{v' \cdot (1 - \frac{a_\theta}{e}) - 1}{v'} \tag{39}$$

Case 4. $\rho\eta = 0$; *i.e.* $D = 0$, no underreported income is discovered by the tax authorities. The utility of this case is again clearly conceptual, as agents have no reason to report income in this case other than based on underreporting costs they perceive. The first-order condition reduces to another more simple optimality condition:

Figure D.1: Calibration of optimal penalty rate:
Optimal penalty rate as a function of other discretionary model parameters



(a) Optimal penalty rate as a function of discovery rate



(b) Optimal penalty rate as a function of marginal value of social spending

Note: These figures plot parameterizations of the optimal penalty rate as a function of different model parameters from Section 7.4.2. Panel (a) plots the optimal penalty rate as a function of the discovery rate, defined as a value between zero and one corresponding with the share of underreported income that is discovered by the tax authorities. Panel (b) uses as the independent variable the marginal value of social spending relative to private consumption. All other parameters are fixed as specified in-text.