

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, demonstrating 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 the penalty rate has a 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.

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. Assigned tax noncompliance penalties typically amount to around 2% of taxpayers' 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. 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 2016 reform that reduced the standard Norwegian income tax noncompliance penalty rate from 30% to 20% of the underpaid tax, a 33% relative decrease in the statutory rate. Because the reform changed the penalty rate for all taxpayers at once, we cannot compare taxpayers facing different rates going forward. Instead, we compare taxpayers penalized under the old and new regimes in a difference-in-differences event study design, which relies on a parallel trends assumption. We therefore estimate how the experience of a penalty shapes subsequent behavior ("specific" deterrence), not how the prospect of a penalty shapes the behavior of the population at large ("general" deterrence) (Andreoni et al. (1998)). The validity of this design also relies on a comparability assumption that rules out heterogeneous treatment effects of the two penalized cohorts. We document that the composition of penalized taxpayers indeed changes around this reform,

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

so we validate our estimates by implementing models that attempt to alleviate concerns pertaining to changing composition. In particular, we estimate a matching design and a correlated random effects model that control for non-compliance risk score (as determined by the tax authorities) as well as pre-audit levels and changes in income. We also test for spillover effects onto business and family peers, which allows us to rule out certain versions of heterogeneous treatment effect models.

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. 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 owned-businesses. 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 difference-in-differences design approach that leverages the 2016 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 as well as mechanically through increased collections on inframarginally 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%.

1.1 Placing in the broader literature

We view this paper as contributing to the literatures on tax administration and tax non-compliance, 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 taxpayers’ 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 (2019a)). In the context of Norway, Bott et al. (2020) use manipulated moral and detection letters in an experimental design to show that moral suasion significantly in-

creases 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 (2019a) 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) provide 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) find a negative impact of longer prison sentences on criminal recidivism; Huttunen et al. (2026) document large negative effects 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) explore 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 larger 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.

Our work also speaks to the literature on post-tax income and wealth inequality. Tax

noncompliance and the policies designed to regulate it have first-order implications for the income distribution through two distinct channels. First, penalty assignment directly affects the distribution of post-tax income through its mechanical impact on after-penalty income. Second, and more subtly, penalty rates shape the extent of income underreporting, which itself biases estimates of pre-tax inequality—an issue of particular relevance given evidence that underreporting is concentrated at the top of the income distribution (Guyton et al. (2023); Alstadsæter et al. (2019); Alstadsæter et al. (2018)). Existing work has documented large gaps between pre- and post-tax income inequality in the US (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)), but the role of noncompliance penalties in shaping these patterns has received limited attention. We contribute to this literature by providing novel descriptive evidence on the distribution of penalty assignments and effective penalty rates across the income and wealth distribution.

2 Institutional background and data description

Our analysis builds on 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 accessed 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 conducted 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.

2.1 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%.²

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

²The exchange rate to USD is approximately NOK 10 per USD as of October 2026.

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 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 with the underpaid taxes due plus 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 this distinction between the two different penalty types as 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.

Change in the penalty rate. 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, 2015.³ 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

³<https://www.regjeringen.no/no/dokumentarkiv/regjeringen-solberg/aktuelt-regjeringen-solberg/fin/pressemeldinger/2015/forenkler-reglene-for-skatteforvaltning-og-styrker-rettssikkerheten/id2466803/>

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, and in our data environment, we are able to disentangle late filing fees from tax noncompliance penalties. The Tax Administration Act was passed unanimously by the Norwegian Parliament on May 3, 2016 and formally enacted on May 27, 2016. The new penalty rate entered into force 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 decision 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 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.

2.2 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 reciprocity and appeal characteristics. 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. The data on appeals contain information on opening and closing dates of the appeal case, as well as the monetary amount that has been waived.

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. These penalties are assigned in response to operational audits of prior declarations: e.g. for a penalty observed in 2015, the penalty was implemented in response to an operational audit of a tax declaration in either 2015 or before. In practice, nearly all audits are performed either in the same year the declaration is filed or one year later. As described, 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 constrain 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 Norwegian tax authorities 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.

2.3 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 about who receives penalties and how penalties impact overall tax liability and net collections. Additionally, a distinctive feature of our analysis is the ability to incorporate information on appeals of penalties. This feature 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 2015.⁴ 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

⁴Table A.1 provides analogous descriptive statistics for pooled observations from 2011 to 2021.

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 4).⁵ 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.

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 (28pp relative to a non-penalized baseline of 7%), 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.

Figure 1 shows the frequency of penalty assignment along income. Panel (a) depicts a degree of progressivity in penalty assignment on the extensive margin: in a given year nearly 1.5% of the top 1% of earners receive a penalty, rising to around 3% among the top 0.01%. The pattern does not reflect more frequent auditing of high earners, persisting after restricting the sample to audited taxpayers (Figure A.1). Penalty size follows the same gradient: conditional on penalty receipt, amounts increase in income, taxable wealth, self-employment income, and the presence of international transfers, with most relationships persisting after controlling for log corrected income and gross wealth (Table A.3).⁶ Taxpayers assigned zero-amount penalties differ systematically from those assigned positive amounts, the latter of which hold less wealth, and exhibit less business ownership and fewer transfers abroad.

Panel (b) plots the evolution in effective tax and penalty rates among Norwegian tax-

⁵Table A.2 provides summary statistics for penalties disaggregated by ordinary/elevated rate status.

⁶Figure A.2 displays the distribution of log penalty amount along income rank conditional on non-zero penalty reciprocity.

payers 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. 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)). 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.⁷

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 between 2011 and 2021. Table 2 gives descriptive statistics on outcomes for the 4% of penalties that saw administrative complaint/appeal during that time span. 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 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 A.4 reports balance on taxpayer covariates for penalized taxpayers that appeal versus those that do not engage in appeal. Appellants have significantly higher income and

⁷Figure A.3 replicates this figure, ranking taxpayers by gross wealth rank, resulting in similar findings.

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. 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 that may be unavailable for lower-income individuals.

Table A.5 explores the correlation between appeal probability and penalty size as well as other case characteristics. Cost indicators such as time-to-collect and penalty removal share covary positively with penalty size and effective penalty 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.⁸ These descriptive statistics provide a baseline perspective on why a tax authority would view appeals as costly and desirable to avoid, from a purely net revenue maximization standpoint: appeals are associated with decreased probability of collection, increased time-to-collect (which requires greater time discounting), and increased direct expenses to resolve these appeals (e.g. litigation, allocating tax law or collection specialist time).

⁸Figure A.4 shows these relationships graphically.

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

3.1 Empirical strategy

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.⁹ 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 staggered for both groups, but with the post-2016 cohorts exposed to a lower penalty rate

⁹We 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.

than the pre-reform cohorts.

Equation (1) 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 summation 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.

$$y_{it} = \alpha_i + \delta_t + \underbrace{\sum_{k=-5}^5 \gamma_k \text{EventTime}_{k(it)}}_{\text{Extensive-margin penalty effect}} + \underbrace{\sum_{k=-5}^5 \beta_k \text{EventTime}_{k(it)} \cdot 1\{\text{Penalty Year}_i \geq 2016\}}_{\text{Intensity effect}} + \varepsilon_{it}. \quad (1)$$

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 1 denote taxpayer- and regime status $\times$ year fixed effects, respectively. We cluster standard errors at the taxpayer level.

3.2 Internal validity

We are interested in estimating an effect β of the differential effect on dependent variable y associated with penalty receipt following the statutory rate decrease in 2016. The validity of our estimation strategy requires a set of assumptions. First, a parallel trends assumption requires that $\beta_j \in \{\hat{\beta}_k\}_{k<0} = 0$: 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.6 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 these concerns, Section 3.4 tests the robustness of our results in several ways. We estimate a series of matching and correlated random effects models that directly control for differences in observable pre-penalty characteristics as well as risk scores assigned by the tax authorities. Second, we test for spillover effects onto business and family peers, which allows us to rule out certain 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 (1) risks introducing bias from heterogeneous treatment effects (Borusyak et al. (2024) and Callaway and Sant’Anna (2021)). For this reason, we 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.

On the distinction between ex-post and ex-ante deterrence responses. Because we study the differential response of penalized individuals, our design estimates an ex-post response to penalties rather than the ex-ante deterrence response induced by the penalty rate. This distinction corresponds to the definitions of “specific” and “general” deterrence in Andreoni et al. (1998) and Slemrod (2019b): the former describes how enforcement experience shapes the subsequent behavior of subject agents, the latter how the prospect of

enforcement shapes the behavior of the population at large. In the canonical Allingham and Sandmo (1972) environment, our estimand is identically zero: a taxpayer with correct perceptions faces the same 20% statutory rate from 2016 onward regardless of the rate at which her own penalty was assessed, so her subsequent reporting should not depend on whether she was penalized before or after the reform. The deterrence effect common to both cohorts is differenced out by construction, and what remains is non-zero only insofar as the *experience* of a 30% rather than a 20% penalty shapes subsequent behavior through beliefs. We make this channel explicit rather than treat our estimate as a measure of deterrence.

Central to the design, then, is a behavioral assumption that taxpayers’ beliefs about the penalty rate are shaped by their own enforcement experience and adjust only partially to statutory changes not experienced directly. Under this assumption, we treat a taxpayer penalized under the pre-reform regime as remaining “exposed” to the 30% rate after the statutory rate falls to 20%. The available evidence on penalty beliefs is consistent with such stickiness: while Bergolo et al. (2023) find that firms’ beliefs about penalty rates are unbiased on average, they document wide dispersion in those beliefs across firms and little updating in response to salient enforcement signals such as audit threat letters.

3.3 Results

Panels (a) and (b) in Figure 3 show the baseline event study evidence underlying our approach. The figures present event studies of log reported income and income taxes paid around penalty receipt, estimated separately for taxpayers penalized under the pre- and post-reform regimes using the estimator of Callaway and Sant’Anna (2021). The two groups follow similar trajectories prior to penalty receipt. Both cohorts exhibit a decline in reported income following penalty receipt, which differs from the post-audit increases in income documented in other settings such as Kleven et al. (2011), although the comparison is not direct: our sample conditions on detected noncompliance and penalty assignment following non-random operational audit rather than on random audit. A gap of roughly 10% emerges after penalty receipt: relative to taxpayers penalized under the 30% regime, those penalized under the 20% regime declare around 10% less income and pay around 10% less in income tax. Contextualizing this response within the relative change in the statutory rate of $-1/3$, this

reduced form estimate implies an elasticity of reported income and taxes paid ex-post to the statutory penalty rate of around 0.2-0.3.

We complement our empirical strategy with an instrumental variable (IV) approach to identify the ex-post compliance effects of changing the penalty rate. Table 4 displays the estimates for our designs. Columns (1) and (5) provide 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.¹⁰ Figure 4 shows the event 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 the *effective* penalty rate: a 1pp increase in the effective penalty rate increases income declared and taxes paid by 4.9% and 6.2% respectively. 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.¹²

Table 5 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 A.7 Panel (b)). In addition, we find 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

¹⁰Table A.6 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.

¹¹The specifications reported here are instrumented and require inference on the difference in responses across cohorts, which would entail jointly bootstrapping cohort-specific heterogeneity-robust estimators, which is computationally infeasible at the scale of our data. We therefore estimate the model with only unit fixed effects in order to mitigate the threat of treatment effect heterogeneity bias.

¹²Figure A.8 and Table A.7 estimate these designs using stratified two way fixed effect designs that also include penalty regime status $\times$ year fixed effects. While these designs may be more susceptible to bias via heterogeneous treatment effects, they yield nearly identical results.

income,

Table 5 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 A.7 Panel (b)), and decreases in reported income from sources more susceptible to misreporting, principally self-employment (Kleven et al. 2011). We also document a decrease in reported wage income. Because wage income is subject to third-party reporting, it offers little scope for unilateral misreporting, and the response is therefore unlikely to reflect changes in reporting behavior alone. However, in the Norwegian setting, (Bjørneby et al. (2021)) provide evidence of the existence of wage collusion between employers and employees.

3.4 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 systematically correlate 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 in order to align characteristics of penalty recipients subject to the different penalty rates.

First, we estimate analogues of Equation (1) omitting taxpayer fixed effects while directly controlling for within-taxpayer time-invariant characteristics that could account for differential sample composition between penalty regimes. Table A.8 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 for penalty size (and penalty size in combination with income and wealth) allows us to directly compare similar individuals between penalty regimes, at least based on the severity of their penalties. Figure A.9 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 A.9 and Table A.10 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 A.11 displays the balance of the matching procedure. Compared to Table 3, Table A.11 demonstrates balance on observable characteristics of the two groups, namely with income (as expected), but also with 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 concerns about the differential selection between the samples.

Figure A.10 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 A.12 summarizes the findings for this design.

From this exercise, we conclude that our estimated response 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 constant, 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 A.13 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” non-pecuniary 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 A.11 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.

4 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. Section B develops a full conceptual model extension of Allingham and Sandmo (1972) that endogenizes the administrative appeal decision, yielding a positive causal relationship between penalty size and the probability of appeal under relatively weak assumptions on the appeals process. The intuition is that appealing is worth more when there is more at stake: a larger penalty raises the amount a successful appeal saves in proportion to its size, while the expense/effort a taxpayer rationally devotes to the case rises less than proportionally. Taxpayers therefore appeal above a threshold level of the amount at stake, and a higher penalty rate pushes more penalty recipients across this threshold. The same logic operates on the intensive margin: a taxpayer with more at stake invests more in contesting her case, so higher penalties should raise not only the probability of appeal but the resources devoted to appeals that are lodged, which we examine through processing times and resolution outcomes. We set out an abbreviated argument more formally here.

4.1 Conceptual basis of the relationship between penalty rate and appeal decision

Consider a taxpayer accused by the tax authorities of underreporting income z . For a tax function $T(\cdot)$ and a penalty function $\phi(\cdot)$, having already paid taxes on reported income x , the taxpayer is asked to pay the difference in taxes plus a fine on the underreported amount $T(x + z) - T(x) + \phi(z) := \Lambda$, jointly referred to as “the stakes”.

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 $r(\alpha)$.¹³ It is straightforward to demonstrate the taxpayer’s “extensive” and “intensive” appeal decision rules: that is, respectively, *whether* to appeal and *how much* effort/resources to expend.

On the extensive margin, as an expected utility maximizer the taxpayer decides to appeal simply if $(1 - r(\alpha^*))\Lambda^* > F + \alpha^*$, given interior optimal values notated with a star superscript. 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.

On the intensive margin, the taxpayer minimizes expected losses as $\alpha + r(\alpha)\Lambda$, resulting in a simple semi-elasticity rule for optimal appeal effort:

$$\alpha^* = |\varepsilon_{\tilde{r}, \alpha^*}| \cdot \Lambda^* \quad (2)$$

The optimal level of intensive-margin appeal effort increases in both the responsiveness of the success-function in effort and the stakes. Applying the implicit function theorem to the first-order condition allows us to gauge the response of optimal appeal effort as the penalty

¹³We suppress the tax authority’s appeal effort here. Section B.3 treats it explicitly, and Section B.4.1 considers the alternative legal environment in which the losing party bears the winning party’s legal expenses, showing that the results are qualitatively unchanged.

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.

4.2 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, a second feature of the 2016 reform rules out applying the event-study design used above directly to appeals. The reduction in the ordinary penalty rate coincided with a separate reform that simplified the administrative complaint process, making it easier for penalized taxpayers to appeal. A design comparing ordinary-regime penalty recipients before and after 2016 would therefore conflate the effect of the lower penalty rate with the effect of the procedural change.

Identifying the appeals response to the change in penalty rate response therefore requires a comparison group whose recipients faced the same reformed complaint process but no change in their penalty rate. For this reason, we compare the evolution in the appeals rate for ordinary penalty rate recipients against that for elevated penalties. Elevated-rate penalties were unaffected by the 2016 rate reduction, while taxpayers penalized under that

regime benefited from an identical appeals simplification. Differencing appeal rates across the two regimes removes any common change in the ease of appealing, leaving the differential response to the penalty rate itself.

Figure A.12 plots the time series of appeal rate by penalty rate regime. Appeal rates rise across both regimes from 2016, reflecting the simplified complaint process. Separately, appeal rates under the ordinary regime fall relative to the elevated regime from 2016 onward, following the reduction in the ordinary rate from 30% to 20%. The divergence is the variation our difference-in-differences design exploits, and its direction matches the mechanism set out in Section B: lower penalties reduce the stakes of conceding, and so the return to appealing.

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

$$1\{Appeal_j\} = \beta_0 + \beta_1 1\{OrdinaryRegime_j\} + \beta_2 1\{Year_j \geq 2016\} + \beta_{dd} 1\{OrdinaryRegime_j\} 1\{Year_j \geq 2016\} + \Gamma X_j + \varepsilon_j. \quad (3)$$

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 prior to the reform. Second, there is an exclusion restriction that the reform is not accompanied 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 the 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. Hence, 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 (3) 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 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 manner¹⁴, which may be attributable to the presence of learning effects or knowledge spillovers regarding the appeals process.

Table 6 Panel (a) presents 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 log income and wealth, producing very similar estimates. Columns (5)-(8) reproduce the first four columns while additionally controlling for log penalty amount, attributing the differential impact to the penalty *rate* holding the penalty *amount* constant. The estimated effects are largely unchanged, and if anything slightly larger in magnitude than their analogues in Columns (1)-(4), indicating that the reduced form is not driven by a shift in the size distribution of penalties across the reform. Table A.14 replicates these results controlling for assigned risk score, with nearly identical estimates, alleviating concerns that compositional changes drive the estimated appeal effects.

We can contextualize this decrease in appeals 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: $\hat{\varepsilon}_{\text{Appeal},\theta} \approx \frac{-.025/(.063+.014)}{-.33\%} = 0.97$, which is statistically indistinguishable from one. The coefficients on log penalty amount in Columns (5)-(8) imply a substantially smaller average elasticity of 0.43, but the two estimates in fact describe different objects, where the latter is likely attenuated due to selection effects. The reform difference-in-difference coefficients recover the response to a change in the rate applied to a given case, while the cross-sectional gradient compares taxpayers facing penalties of different sizes, and penalty size is likely correlated with case characteristics that independently affect the decision to appeal. The model in Section B suggests the direction of the resulting attenuation: the appeal response

¹⁴One 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 .

scales with the probability of exoneration, so if larger penalties are assigned in cases where the tax authority’s evidence is stronger, the cross-sectional gradient understates the causal response to penalty size.

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.

4.3 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 to 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 4.4. We instrument for the size of a penalty faced by a taxpayer using a leave-one-out average penalty rate constructed from other taxpayers’ 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:

$$Appeal_{it} = \alpha_t + \beta \times PenaltySize_{it} + \varepsilon_{it}. \quad (4)$$

$Appeal_{it}$ 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 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).¹⁵ Equation (4) 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.¹⁶ 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 A.4 and Table A.5.

Table A.15 Panels (a) and (b) show that our instruments are highly predictive of individuals' respective penalty burdens and display strong first stages across all specifications. We

¹⁵There 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. $\overline{\ln(\theta)}_{-it}$.

¹⁶Year $\times$ 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.

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 of this approach. Columns (1)-(3) give estimates of the impact of log penalty amount. Column (1) documents an OLS estimate of 0.007: 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. A pattern emerges when analyzing the impact of changes in the effective penalty rate on appeal. Column (4) reports that, 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.¹⁷

4.4 Robustness

We substantiate the robustness of this result using three additional strategies complementary approaches. First, Table A.17 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, these columns result in similar, possibly 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.

¹⁷Table A.16 displays analogous results using our alternate set of leave-one-out mean parameterization instruments, 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 inhabitants 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 A.18 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 larger 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) in Table A.19 present OLS estimates. Columns (4)-(6) give causal IV estimates using our year $\times$ penalty regime leave-one-out mean strategy from Section 4.3. We find compelling evidence that even within an individual, after controlling for corrected income and wealth as well as “penalty order” (the ordinal sequence of penalty receipt), higher penalties *within* individual induce higher 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 B: 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.

5 Optimal tax administration with dual-revenue instruments

In the preceding sections we documented that higher penalty rates increase reported income and tax payments, but also induce appeals that generate administrative costs and may reduce the effective collection of assessed liabilities. Penalties are therefore neither costless nor purely deterrent. Here we ask how the tax authority should set the penalty rate in light of both margins, extending the framework of Keen and Slemrod (2017) to account for the dual-revenue properties of penalties, which raise revenue both by deterring underreporting and by collecting on underreporting that is detected. We develop the central results here and relegate peripheral results, together with extensions that follow more directly from Keen and Slemrod (2017), to Section C.

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

$$W = x - \psi(l) + v(g), \tag{5}$$

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.

$$x = wl - t(wl - e) - c(e, \alpha) - r(e, t, \Xi) \quad (6)$$

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. Also assume that detection is associated with perfect specificity (no false positives), and that, unlike in Norris and Rose (2024), penalty recipients do not default on their penalties, an assumption we revisit later.

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:

$$g + a(\alpha, \Xi) = t(wl - e) + r(e, t, \Xi). \quad (7)$$

Combining this equation with Equation (5) and Equation (6) yields an alternative expression for social welfare:

$$W(t, \Xi) = wl - tz - c(e, \alpha) - r(e, t, \Xi) - \psi(l) + v\left(tz + r(e, t, \Xi) - a(\alpha, \Xi)\right), \quad (8)$$

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.

5.1 The optimal dual-revenue instrument

We fully develop and discuss first-order conditions for the tax rate and the tax enforcement instrument in Section C.1. Incorporating collections on detected underreporting modifies both: the optimal income tax faces upward pressure relative to the standard inverse elasticity rule, since marginal underreporting is partly recovered, and the benefit of non-revenue enforcement is correspondingly dampened. Here we characterize the first-order

condition for an arbitrary instrument $\xi \in \Xi$ with dual revenue effects:

$$W_\xi = -r_\xi + v' \cdot (r_\xi + z_\xi(t - r_e) - a_\xi) = 0. \quad (9)$$

The term $-r_\xi$ gives the welfare loss from reduced private consumption. The second term is the net revenue change valued at the marginal social value of public spending, and comprises three components: 1) the mechanical increase in revenue from infra-marginal collections, 2) the change in collections as taxpayers substitute away from underreporting (paying the income tax rather than the actuarial rate on detected underreporting), and 3) the administrative costs of raising the instrument. The third component is the margin our appeals estimates speak to, and the one the literature has largely left implicit.

Rearranging gives an elasticity rule:

$$\varepsilon_{e,\xi} = \frac{r}{e(t - r_e)} \cdot \frac{\epsilon_{r\xi}(v'(1 - \frac{a_\xi}{r_\xi}) - 1)}{v'}, \quad (10)$$

where $\frac{r}{e(t - r_e)}$ is the ratio of collections on underreporting to the tax gap net of those collections. The instrument should be set such that the elasticity of underreporting with respect to ξ equals the welfare-weighted mechanical change in collections relative to the tax gap. Dual-revenue instruments thus follow an elasticity rule opposite in sign to that governing the optimal income tax: a larger behavioral response justifies a higher rather than a lower level of the instrument, since the response moves income from the actuarial rate onto the income tax base. Section C reports the implied expression for ξ^* , the limiting cases in which taxpayers are unresponsive or the instrument is costless, and the condition relating optimal levels of ξ and α .

Evaluating Equation (9) at the optimum yields the relationship central to the calibration below:

$$\frac{r_e(\xi^*)}{t} = 1 + \left(\frac{v' - 1}{v'} - \frac{a_\xi}{r_\xi} \right) \cdot \frac{1}{\varepsilon_{et}} \cdot \frac{r_\xi}{e} \cdot m, \quad (11)$$

for $m = \frac{1 - r_{et}}{r_{e\xi}}$. Since ε_{et} , r_ξ , e , $m > 0$, the actuarial rate exceeds the income tax rate at the optimum whenever $\frac{a_\xi}{r_\xi} < \frac{v' - 1}{v'}$, which holds when the marginal administrative cost of

the instrument is small relative to marginal collections. With risk-neutral taxpayers who correctly perceive the penalty environment, the planner would then set $r_e(\xi^*) \equiv t$: the expected recovery on an evaded Krone equals the tax on an honestly reported one. Whether that condition binds in practice depends on the magnitude of administrative costs, which we estimate directly.

Two further sets of results, developed in Section C.3 and Section C.4, follow from the same framework. First, incorporating collections on detected underreporting raises the optimal tax gap: the administration tolerates more underreporting as the actuarial rate on underreported income approaches the income tax rate, since more of it is recovered in expectation. Second, characterizing the strategic interactions among the tax rate, non-revenue enforcement, and dual-revenue instruments, we find that the tax rate and the penalty instrument operate as strategic complements from the perspective of the tax administration, and we derive conditions under which enforcement effort and the penalty instrument operate as strategic substitutes or complements.

5.2 Parametric application and empirical calibration to Norway

To apply these results we give the penalty instruments an explicit parametric form, specifying an audit rate ρ , an expected discovery share η , and a linear penalty schedule under which a taxpayer pays a penalty rate θ plus the underpaid taxes t on detected underreporting, so that $r(e, t, \rho, \eta, \theta) = \rho\eta(t + \theta)e$. Let $\Delta := t - \rho\eta(t + \theta)$ denote the difference between the statutory tax rate and the expected collection rate on underreported income, $D := \rho\eta e$ the aggregate amount of discovered underreporting, and $d := \rho\eta$ the discovery rate. Inserting these into Equation (9) and rearranging gives

$$\theta^* = \frac{t(\frac{1}{d} - 1)}{1 + \frac{1}{\varepsilon_{e,\theta}} \cdot \frac{v' \cdot (1 - \frac{a_\theta}{D}) - 1}{v'}}, \quad (12)$$

Re-expressed this equation in terms of estimable objects yields

$$\theta^* = \frac{t \cdot z \cdot \varepsilon_{z,\theta} \cdot (1 - d) - b \cdot \varepsilon_{b,\theta}}{d \cdot z \cdot \varepsilon_{z,\theta} - D \cdot (\frac{v'-1}{v'})}, \quad (13)$$

where we use $\varepsilon_{z,\theta}$ rather than $\varepsilon_{e,\theta}$, since we estimate the former directly while the latter suffers from severe censoring, and where $a \cdot \varepsilon_{a,\theta}$ is decomposed into $b \cdot \varepsilon_{b,\theta}$ for b the costs of the complaint and appeals procedure.¹⁸

The calibration requires the following values.

- $\varepsilon_{z,\theta} = .45$
- $\varepsilon_{b,\theta} = .6$
- 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))

The first point to note is that the administrative cost margin, which our empirical results establish to be real, is quantitatively negligible at Norwegian magnitudes. Expenditures on the appeals board amount to roughly a thousandth of discovered underreporting, and discovered underreporting is itself only around 3% of aggregate declared personal income.¹⁹ The term in $b \cdot \varepsilon_{b,\theta}$ therefore contributes little, and Equation (13) reduces to

$$\theta^* \approx t \cdot \frac{1-d}{d}, \quad (14)$$

the condition that the expected recovery on an evaded Krone equal the tax on an honestly reported one. The estimated elasticities, which are the paper's principal empirical contribution, drop out of the expression almost entirely. We state the result plainly because it bears

¹⁸This parameterization abstracts from non-payment risk, which can be incorporated by augmenting the collections function as $r := \rho\eta(t + \theta)e \cdot (1 - q(\theta))$, where $q(\theta)$ denotes a probability of non-payment or an expected non-payment share, increasing in the penalty rate, giving an optimal penalty rate analogous to Equation (13) of $\theta^* = \frac{t \cdot z \cdot \varepsilon_{z,\theta} - (r \cdot q \cdot \varepsilon_{q,\theta} - b \cdot \varepsilon_{b,\theta}) / (1-q)}{d \cdot z \cdot \varepsilon_{z,\theta} - D \cdot (\frac{v'}{v} - 1)}$. Because both aggregate collections on underreported income r and the non-payment probability q are low in Norway, including non-payment risk alters the calibrated optimum only negligibly, though greater non-payment risk strengthens the forces driving the optimal rate downward.

¹⁹The low level of expenditure on tax enforcement and complaints is consistent with estimates of a very high marginal value of public funds associated with tax enforcement spending (e.g. Boning et al. (2024)).

directly on how the calibrated figure should be read: the optimal penalty rate we report is an actuarial benchmark, and the empirical content of our exercise lies in having shown that the administrative cost channel is too small to displace it rather than in disciplining it.

Evaluated at the parameters above, Equation (14) gives an optimal penalty rate of approximately 200%, an order of magnitude above the prevailing statutory rate in Norway. The figure is highly sensitive to the discovery rate, which enters as $1/d$ and is the parameter estimated with least precision: Alstadsæter et al. (2019) find that audits detect around 20–30% of evasion among the wealthiest Scandinavian taxpayers, and the implied optimum ranges from roughly 110% to 190% across that interval alone. Figure C.1 plots the optimal rate against both the discovery rate and the marginal value of public funds, holding remaining parameters fixed. Because d is borrowed from external estimates and is conceptually distinct from anything we estimate, the level of the calibrated optimum should be treated as indicative. Rather, the exercise establishes that the optimal rate likely lies well above the status quo.

6 Conclusion

This paper is the first to study the impact of the income tax noncompliance penalty 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 policies 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 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. This thereby reduces 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 raise 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 pe-

nalized. 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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Main exhibits

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

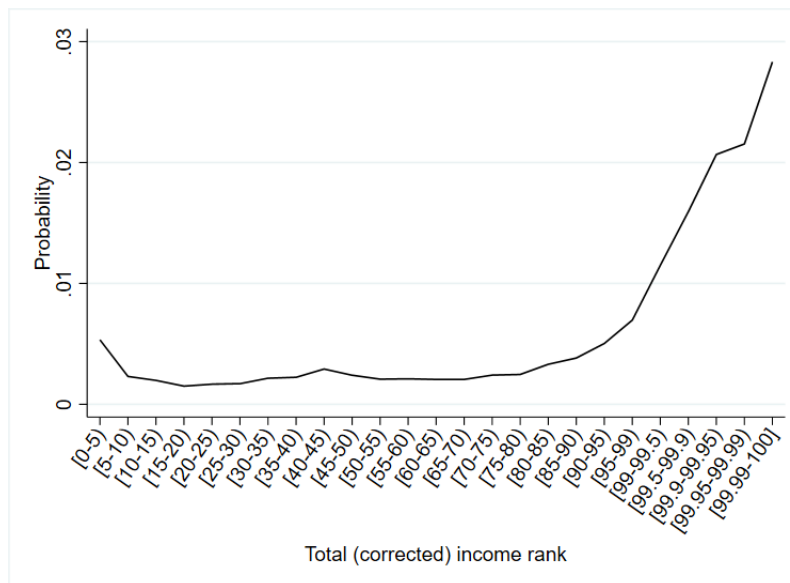
Note: This table shows descriptive statistics of 2015 tax noncompliance penalties in Panel (a) and the attributes of penalty (non-)recipients in Panel (b). In Panel (a), some taxpayers receive both ordinary and elevated penalties. The “Non-penalized” and “Penalized” columns in Panel (b) present means and standard deviations (in hard parentheses) of taxpayer characteristics stratified by penalty reciprocity status for a given individual-year (where a given taxpayer can appear in both columns in separate years). The “Difference” column reports coefficients from a series of univariate regressions of each row variable on an indicator of penalty assignment; coefficients in Column (4) result from regressions that also control for audit flag status. Soft parentheses contain heteroskedasticity-robust standard errors. This table is referred to in Section 2.3.

Table 2: Summary statistics on appeals outcomes

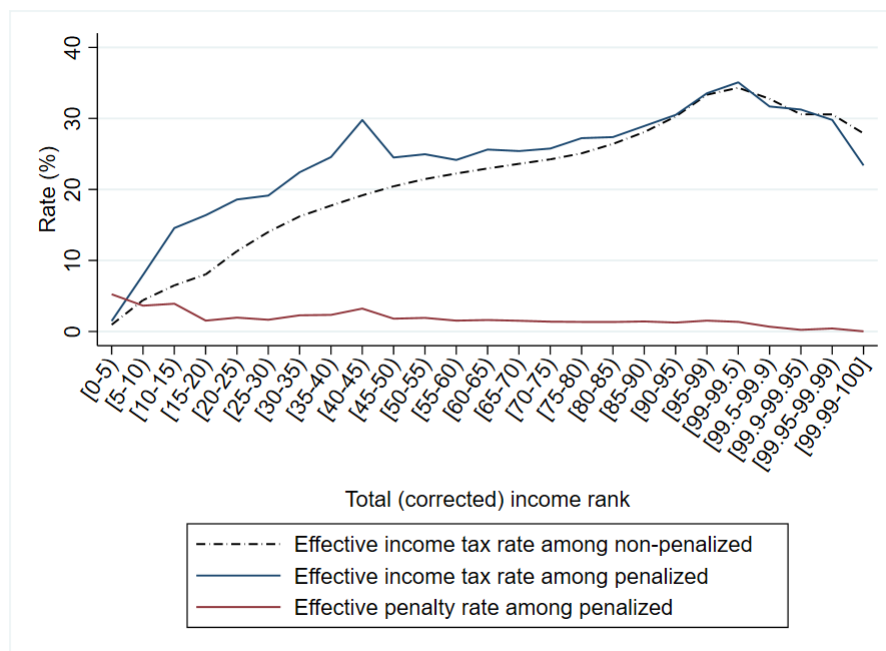
Variable	Mean	SD	N
All appeals			
Has a closing	.735	.441	3,562
Collected	.636	.481	3,562
Years from initial tax year, at opening	3.47	2.10	3,562
Conditional on closing (and positive initial penalty)			
No penalty removal	.778	.416	1,610
Penalty is only partially removed	.096	.295	1,610
Penalty is fully removed	.122	.328	1,610
Share removed (conditional on positive 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. This table is referred to in Section 2.3.

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



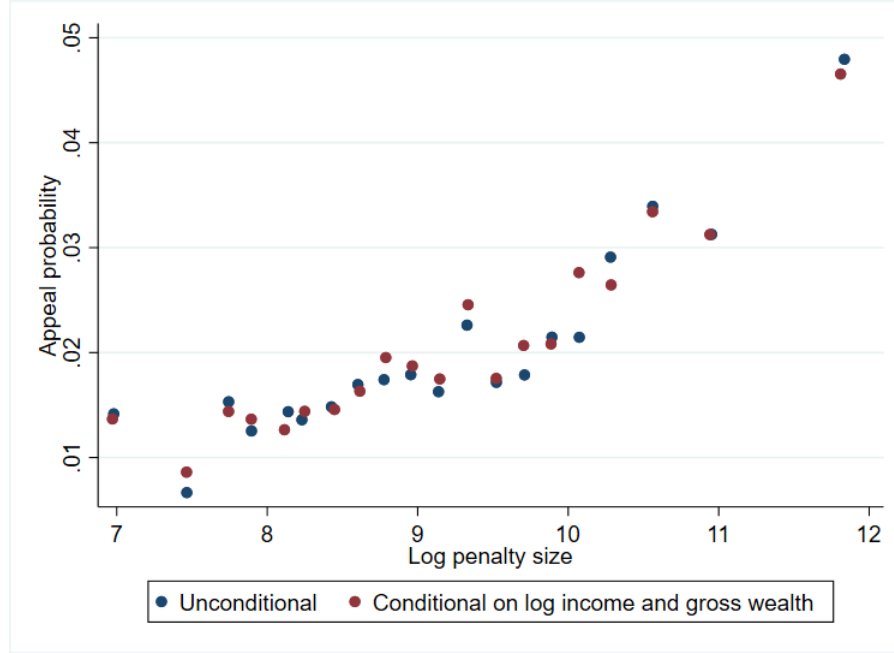
Panel (a): Penalty assignment v. income rank



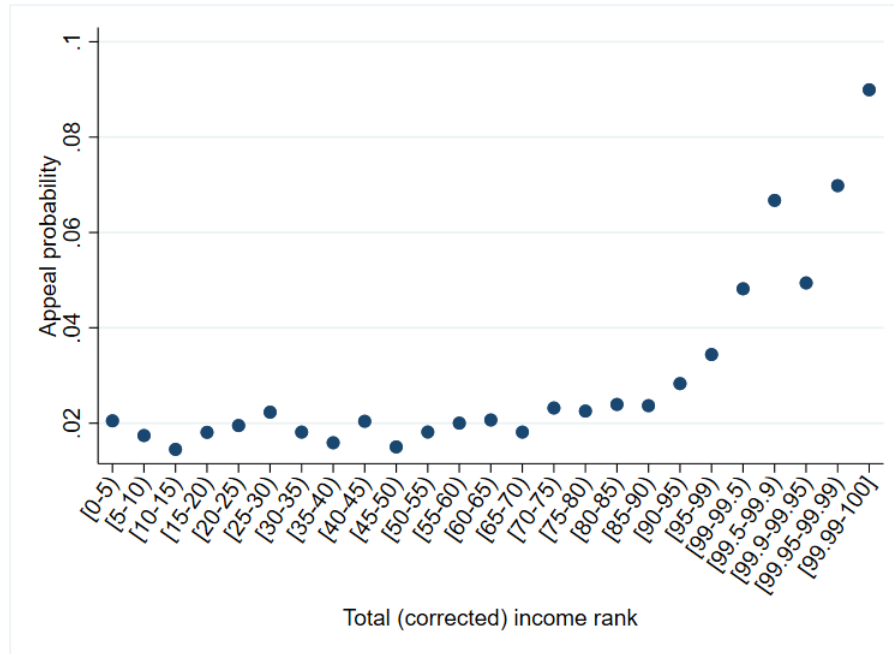
Panel (b): Effective income tax rates and penalty rates by income rank

Note: These figures plot the distributional characteristics of penalty reciprocity 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. Effective income tax rate does not feature correction for underpaid taxes, which we do not directly observe. This figure is referred to in Section 2.3.

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



(a) Appeal versus penalty size



(b) Appeal probability by income rank

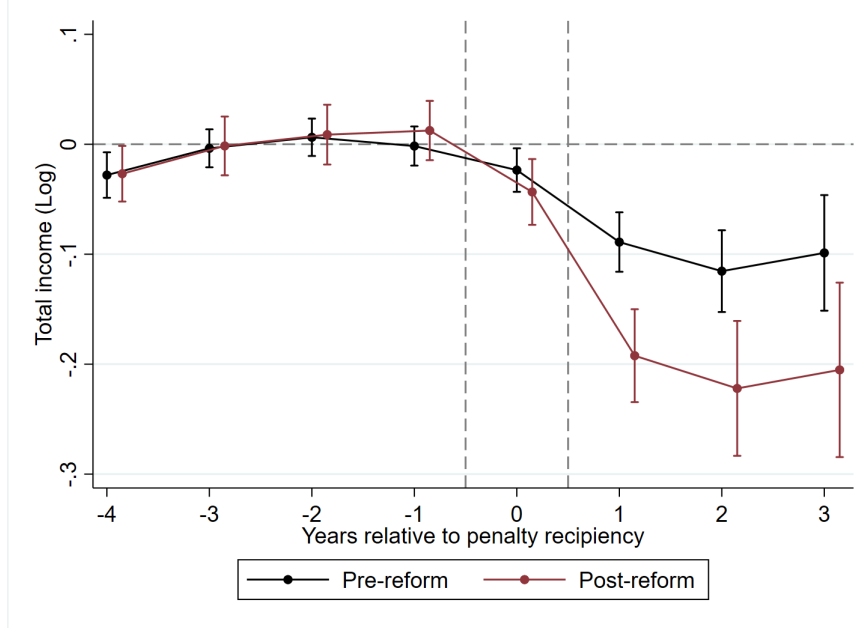
Note: These figures display the binned scatter plots of whether or not a penalty is appealed on characteristics pertaining to the penalty and penalty recipient. Both series in Panel (a) include year fixed effects. Corrected income ranks in Panel (b) are constructed on the annual-level. Both panels by definition condition on receipt of penalty. This figure is referred to in Section 2.3.

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

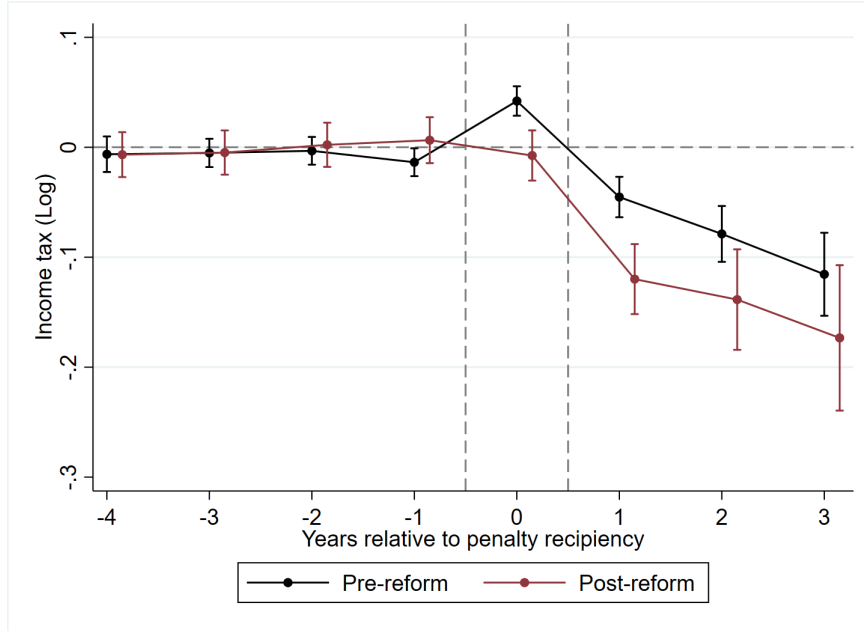
Dependent variable	Pre-reform	Post-reform	Difference
Log corrected income	12.8 [1.09]	13 [1.11]	.24 (.01)
Log penalty size	8.87 [1.16]	8.66 [1.08]	-.205 (.01)
Effective penalty rate	.04 [.04]	.03 [.04]	-.0077 (.0004)
Has self-employment income	.26 [.39]	.2 [.35]	-.0604 (.0039)
Has capital income	.89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

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



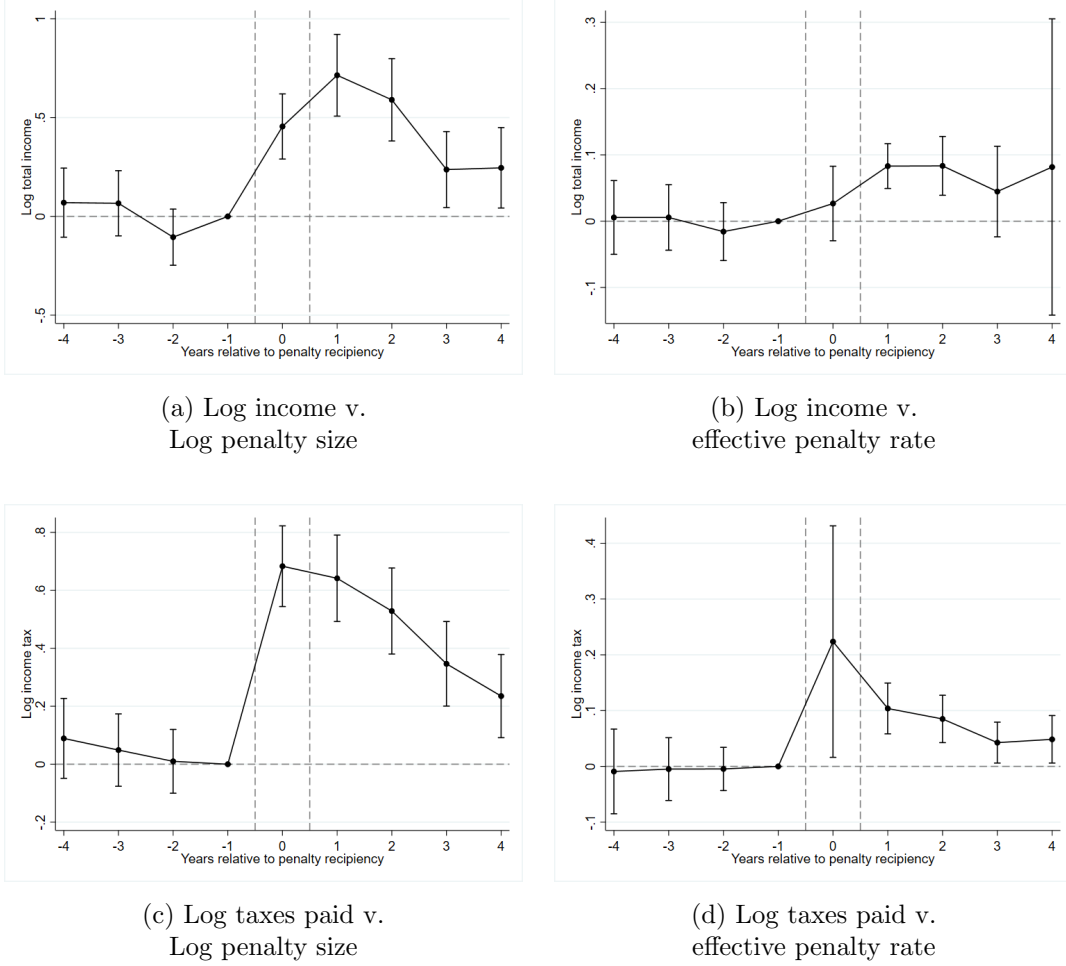
(a) Log income



(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 $y_{it} = \alpha_i + \delta_t + \sum_{k=-5}^5 \gamma_k \text{EventTime}_{k(it)} + \varepsilon_{it}$ 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. This figure is referred to in Section 3.3.

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



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. This figure is referred to in Section 3.3.

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 $\times$ Post-event	-0.067 (0.012)				-0.081 (0.0092)			
Log penalty $\times$ Post-event		0.42 (0.069)				0.46 (0.056)		
Effective penalty rate $\times$ Post-event			0.11 (0.022)				0.14 (0.018)	
Log effective penalty rate $\times$ Post-event				0.20 (0.031)				0.22 (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

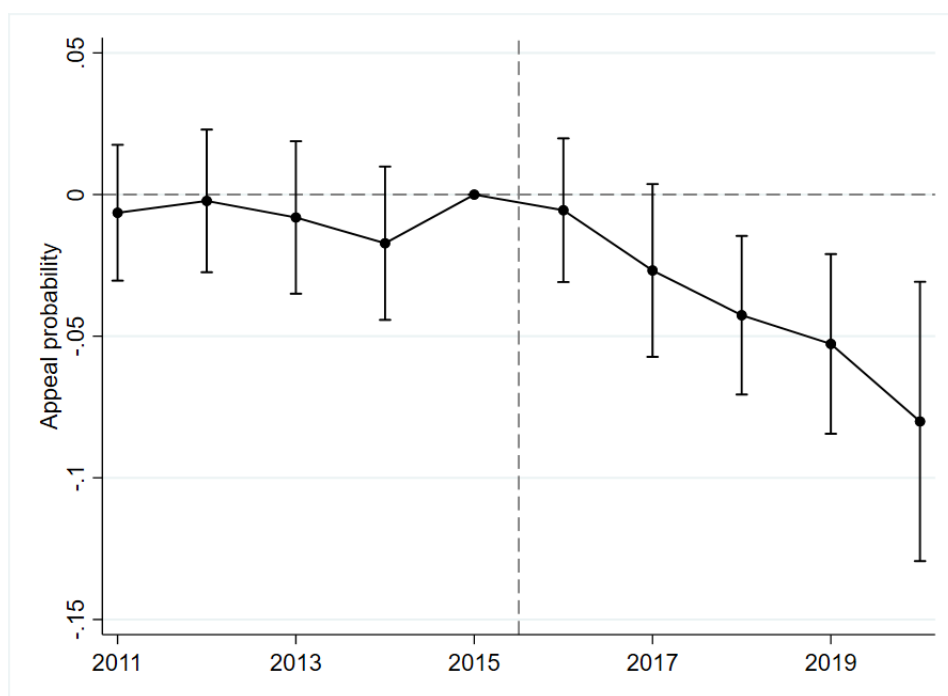
Note: This table displays results from a series of instrumental variable difference-in-differences events study regressions corresponding with the reduced form $y_{it} = \alpha_i + \gamma PostPenaltyEvent_{it} + \beta PostReform_i \cdot PostPenaltyEvent_{it} + \varepsilon_{it}$. In each instrumental 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. This table is referred to in Section 3.3.

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 $\times$ Post-event	0.064 (0.011)	-0.066 (0.011)	-0.014 (0.0030)	-0.074 (0.030)	-0.026 (0.0024)	0.0041 (0.024)	0.010 (0.0014)	0.16 (0.13)
Constant	11.5 (0.0038)	12.5 (0.0026)	0.22 (0.00074)	12.1 (0.0054)	0.87 (0.00055)	6.57 (0.0050)	0.028 (0.00029)	10.5 (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. This table is referred to in Section 3.3.

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. This figure is referred to in Section 4.

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 $\times$ Post	-0.025 (0.010)	-0.026 (0.010)	-0.023 (0.010)	-0.024 (0.010)	-0.027 (0.011)	-0.027 (0.011)	-0.024 (0.011)	-0.024 (0.010)
Ordinary regime	-0.0088 (0.0029)	-0.0080 (0.0031)	-0.011 (0.0021)	-0.011 (0.0024)	0.00038 (0.0047)	0.00038 (0.0048)	-0.0020 (0.0037)	-0.0019 (0.0037)
Post	0.063 (0.0082)	0.062 (0.0078)			0.064 (0.0081)	0.063 (0.0077)		
Log penalty					0.0058 (0.0021)	0.0058 (0.0022)	0.0059 (0.0021)	0.0061 (0.0022)
Constant	0.014 (0.0025)	-0.072 (0.029)	0.036 (0.0030)	-0.043 (0.029)	-0.047 (0.022)	-0.082 (0.032)	-0.025 (0.021)	-0.054 (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 (0.0048)	0.014 (0.0049)	0.015 (0.0043)	0.016 (0.0047)
Constant	-0.14 (0.040)	-0.10 (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. This figure is referred to in Section 4.

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

	(1)	(2)	(3)	(4)
Log penalty	0.0071 (0.0023)	0.017 (0.0052)	0.018 (0.0055)	
Effective penalty rate				0.0065 (0.0015)
Log corrected income			-0.0089 (0.0036)	0.0013 (0.0019)
Log gross wealth			0.0031 (0.00095)	0.0044 (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. This figure is referred to in Section 4.

Appendix A Additional graphs and tables to main text

A.1 Additional exhibits

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 [833,008]	622,356 [1,609,389]	183,846 (7,442)
Log total income	12.6 [1.23]	12.6 [1.9]	-.0224 (.01)
Any self-employment income	.07 [.26]	.33 [.47]	.26 (.0021)
Any capital income	.87 [.33]	.9 [.3]	.02 (.0013)
Income tax	97,091 [224,505]	165,398 [460,228]	69,927 (2,001)
Log income tax	11.2 [1.16]	11.5 [1.18]	.35 (.01)
Log gross wealth	13.1 [2.6]	12.6 [3.28]	-.4 (.02)
Log net taxable wealth	12.3 [2.46]	12.8 [2.77]	.56 (.02)
Has a majority business ownership stake	.04 [.19]	.24 [.43]	.21 (.0019)
Any transfer abroad	.17 [.38]	.35 [.48]	.18 (.002)
Any transfer with havens	.02 [.13]	.04 [.2]	.03 (.0008)
Age	47.9 [18.8]	44 [12.3]	-3.63 (.06)
Immigrant	.16 [.37]	.33 [.47]	.18 (.0023)
Female	.49 [.5]	.18 [.39]	-.305 (.0018)
Married	.4 [.49]	.4 [.49]	-.0001 (.0023)
University	.32 [.47]	.26 [.44]	-.0614 (.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 reciprocity 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. This table is referred to in Section 2.3.

Table A.2: 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. This table is referred to in Section 2.3.

Table A.3: Covariate balance by penalty amount among penalized individuals (2015)

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

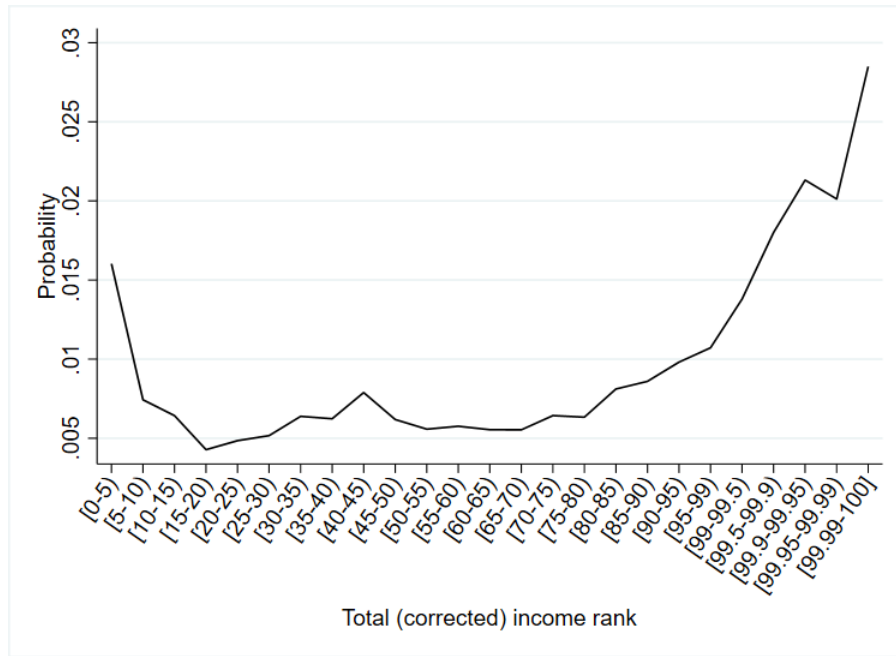
Note: This table displays results from a series of regressions of each respective row variable on the logarithm of penalty amount or an indicator for observing a non-zero penalty amount, respectively. All regressions use the universe of penalized taxpayers in 2015 and feature both an unsaturated version and a variant with log total corrected income and log gross wealth as control variables. Standard errors clustered at the taxpayer level are included in parentheses. All monetary values are expressed in real NOK 2015. This table is referred to in Section 2.3.

Table A.4: Balance on observable characteristics: appealed v. non-appealed penalties

	All	Non-appealed	Appealed	Difference
Corrected income	725,482 [1,857,790]	711,946 1,643,154	1,257,195 [5,717,874]	463,860 (128,374)
Log corrected income	12.9 [1.32]	12.9 1.32	13.3 [1.46]	.27 (.04)
Log penalty size	9.18 [1.26]	9.17 [1.25]	9.69 [1.44]	.56 (.05)
Non-zero penalty	.13 [.33]	.12 [.33]	.3 [.46]	.18 (.01)
Effective penalty rate (winsorized)	.09 [.21]	.09 [.21]	.1 [.23]	.02 (.01)
Any self employment income	.32 [.47]	.32 [.47]	.31 [.46]	-.0055 (.01)
Any capital income	.9 [.3]	.9 [.3]	.94 [.24]	.04 (.01)
Log gross wealth	12.6 [3.28]	12.6 [3.28]	14.1 [2.75]	1.34 (.09)
Has a majority business ownership stake	.25 [.43]	.24 .43	.39 [.49]	.15 (.02)
Log net taxable wealth	12.8 [2.78]	12.7 [2.78]	14 [2.43]	.99 (.1)
Any transfer abroad	.35 [.48]	.35 [.48]	.44 [.5]	.07 (.01)
Any transfer with havens	.04 [.2]	.04 [.2]	.08 [.27]	.03 (.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. This table is referred to in Section 2.3.

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



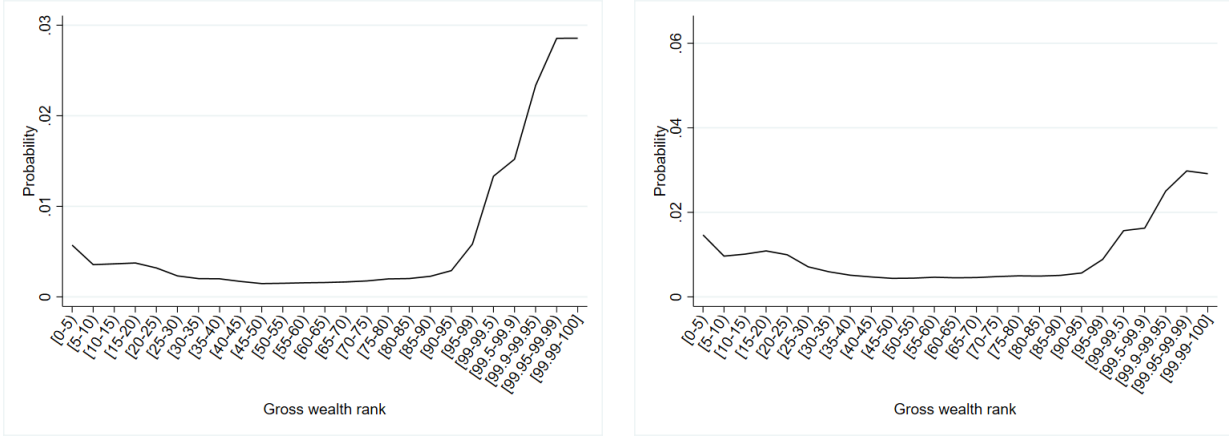
Note: This figure plots the likelihood of penalty assignment within (corrected) income rank bin conditional on being flagged by the tax authorities for irregularities. Income ranks are calculated following correction for detected underreporting.

Figure A.2: Conditional distribution of penalty size



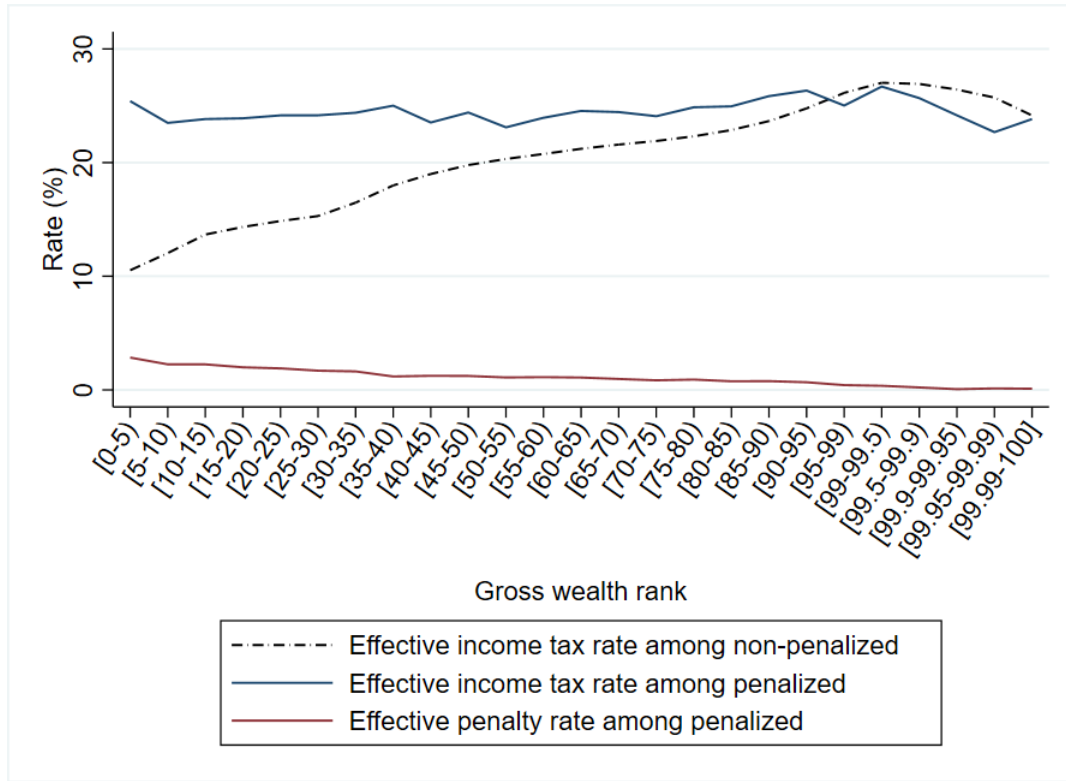
Note: This figure shows the distribution of log penalty size along income rank conditional on non-zero penalty reciprocity from 2011 to 2015, stratifying by penalty type. This figure is referred to in Section 2.3.

Figure A.3: 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 reciprocity 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. Effective income tax rate does not feature correction for underpaid taxes, which we do not directly observe. This figure is referred to in Section 2.3.

Table A.5: OLS: penalty characteristics and appeals outcomes versus penalty

Panel (a): Log penalty amount

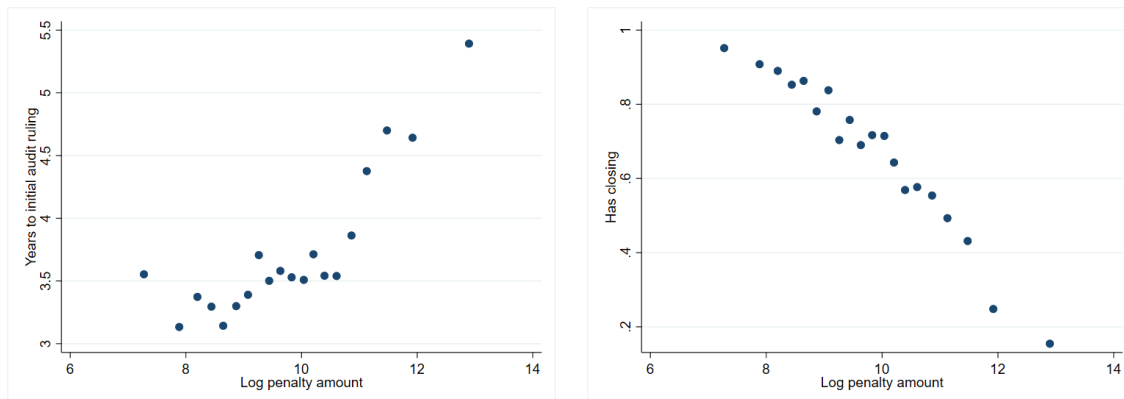
	Time to initial audit decision (log years)		Case has closing		Removed share of penalty		Time to appeal decision (log years)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log penalty amount	0.092 (0.014)	0.084 (0.013)	-0.13 (0.0073)	-0.14 (0.0085)	-0.030 (0.0079)	-0.028 (0.0068)	0.14 (0.040)	0.17 (0.044)
Log income		-0.046 (0.0083)		0.030 (0.0052)		0.0017 (0.0031)		-0.017 (0.023)
Log gross wealth		0.041 (0.0037)		0.021 (0.0025)		-0.0086 (0.0025)		-0.064 (0.018)
Constant	0.23 (0.14)	0.33 (0.11)	1.93 (0.072)	1.41 (0.10)	0.38 (0.078)	0.46 (0.100)	-3.16 (0.38)	-2.38 (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 (0.0014)	0.0031 (0.0015)	-0.0049 (0.00058)	-0.0052 (0.0010)	-0.00060 (0.00031)	-0.00097 (0.00049)	0.010 (0.0027)	0.013 (0.0041)
Log income		-0.025 (0.0062)		-0.0083 (0.0090)		-0.0051 (0.0046)		0.068 (0.045)
Log gross wealth		0.046 (0.0035)		0.014 (0.0026)		-0.011 (0.0028)		-0.056 (0.021)
Constant	1.09 (0.013)	0.78 (0.071)	0.77 (0.0054)	0.68 (0.13)	0.11 (0.0029)	0.32 (0.088)	-1.94 (0.013)	-2.05 (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

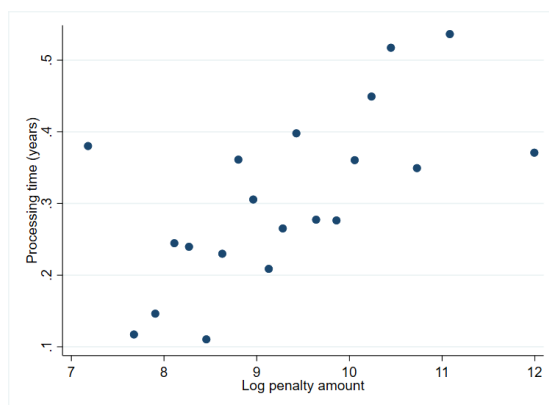
Note: These tables display results from ordinary least squares regressions of penalty characteristics and appeals outcomes on parameterizations of penalty size. Panel (a) uses log inflation-adjusted penalty amount as the main independent variable; Panel (b) uses effective penalty rate (penalty amount divided by income) winsorized at 0 and 1. Parentheses contain standard errors clustered at the penalty-year level. This table is referred to in Section 2.3 and in Section 4.

Figure A.4: Relationship between post-appeal outcomes and penalty amount



(a) Time to initial ruling

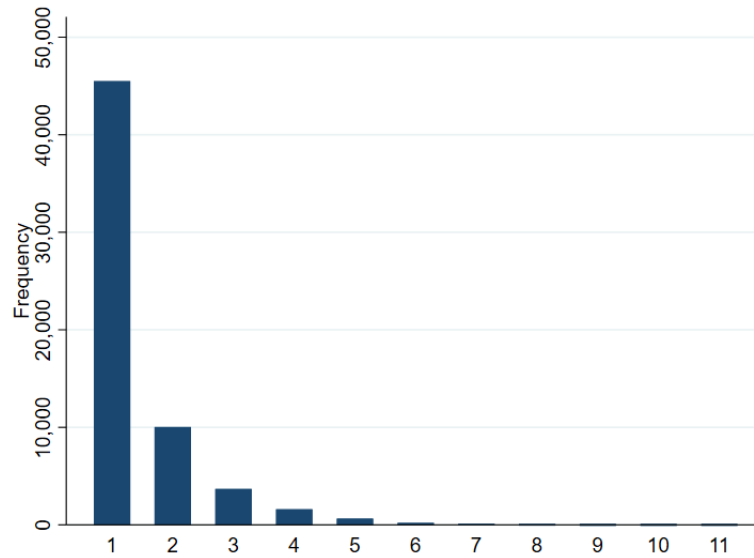
(b) Appeal has closing



(c) Time to appeal ruling

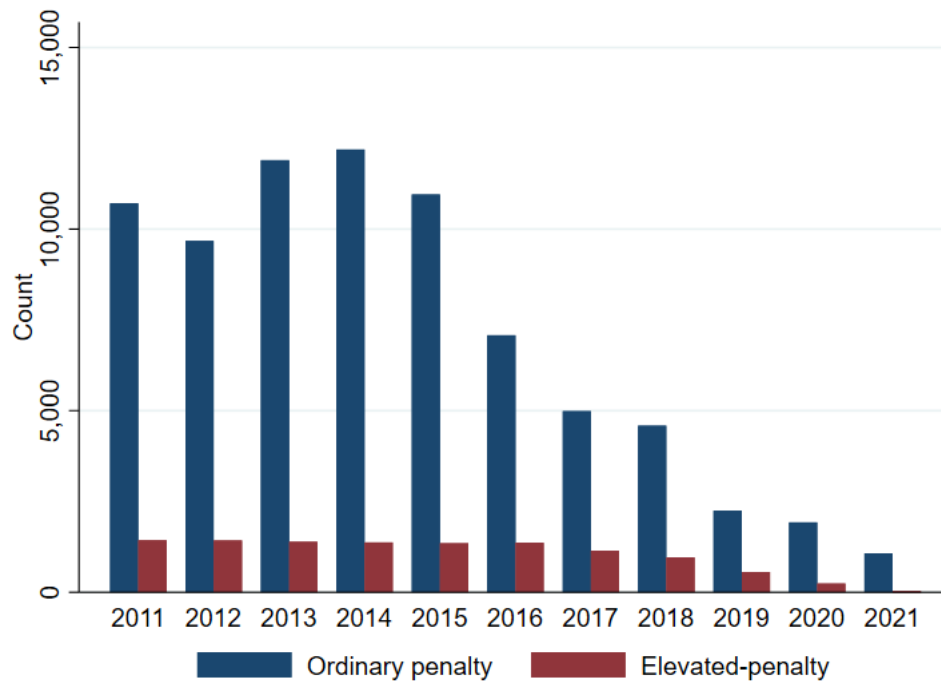
Note: These are binned scatter plots of various appeal-related outcomes on log penalty size. All figures include year fixed effects and control for log income and wealth. The variable “Years to initial audit ruling” (Panel (a)) is in principle applicable to all penalties, but is only observed for appealed penalties. This figure is referred to Section 2.3 and in Section 4.

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. This figure is referred to in Section 3.

Figure A.6: 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. This figure is referred to in Section 3.

Table A.6: First stage effect of the penalty rate reform on penalty size

	(1)	(2)	(3)
	Log penalty	Effective penalty rate	Log effective penalty rate
Year $\in [2016, 2018]$	-0.21 (0.036)	-0.014 (0.0020)	-0.50 (0.033)
Year $\in [2019, 2021]$	0.64 (0.030)	0.0015 (0.0025)	0.20 (0.030)
Constant	8.87 (0.039)	0.056 (0.0010)	-3.86 (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

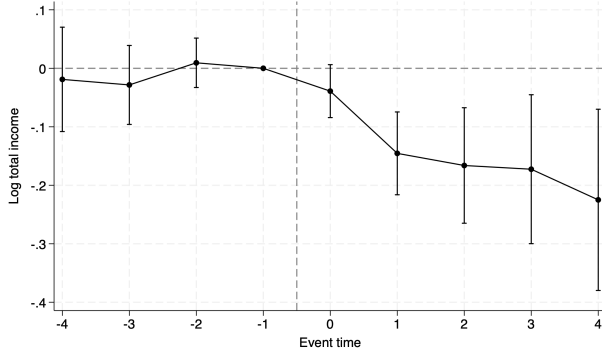
Note: This table displays results from a series of regressions with different parameterizations of penalty size as dependent variables on binary indicators of whether the penalty assignment occurred within the time span 2016-2018 and 2019-2021, respectively. Standard errors clustered at the year level are included in parentheses. This table is referred to in Section 3.

Table A.7: IV event study difference-in-differences:
Impact of penalty rate on declared income and taxes paid (penalty rate reform IV)

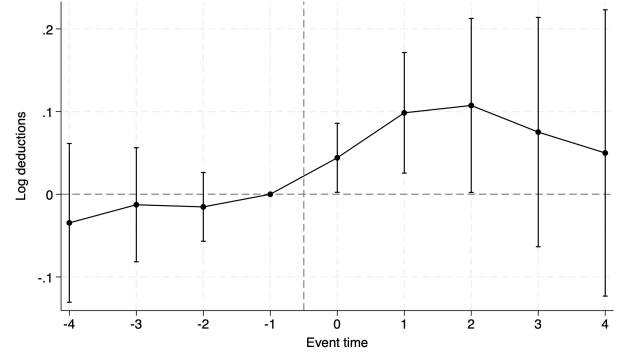
	Log income				Log taxes paid			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform $\times$ Post-event	-0.072 (0.022)				-0.092 (0.015)			
Log penalty $\times$ Post-event		0.50 (0.16)				0.65 (0.12)		
Effective penalty rate $\times$ Post-event			0.090 (0.046)				0.16 (0.040)	
Log effective penalty rate $\times$ Post-event				0.21 (0.061)				0.25 (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 $y_{it} = \alpha_i + \delta_{t,d(i)} + \sum_{k=-5}^5 \gamma_k \text{EventTime}_{k(it)} + \sum_{k=-5}^5 \beta_k \theta_i \text{EventTime}_{k(it)} + \varepsilon_{it}$. 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 $\times$ year level. Standard errors clustered at the taxpayer level are included in parentheses. This figure is referred to in Section 3.

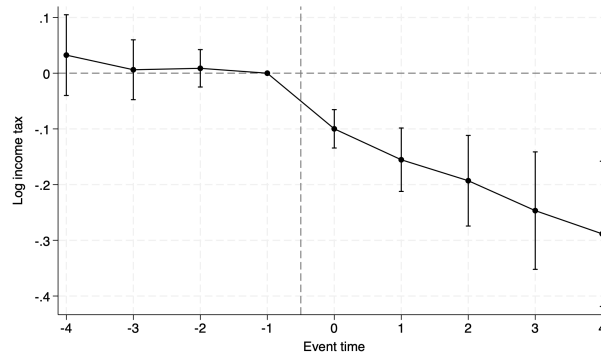
Figure A.7: Net effects of the penalty rate reform:
Difference-in-differences event study estimates



Panel (a): Total income



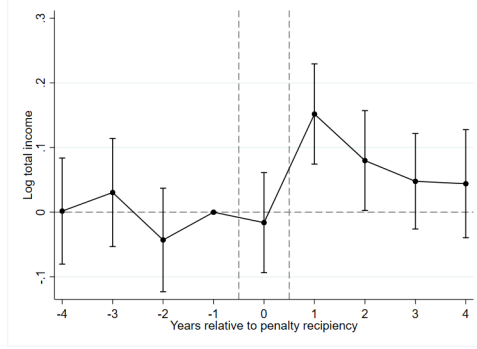
Panel (b): Deductions



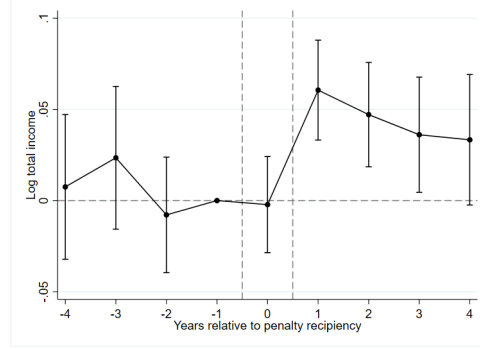
Panel (c): Income tax paid

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 at the taxpayer level. This figure is referred to in Section 3.

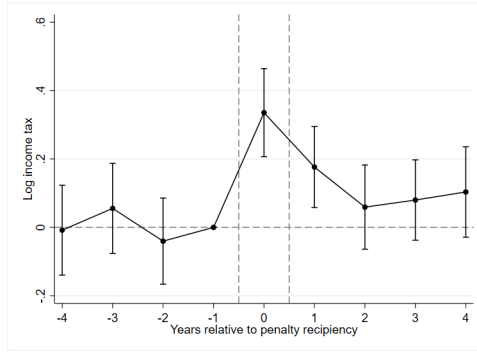
Figure A.8: Event study IV difference-in-differences:
Impact of penalties on declared income and taxes paid



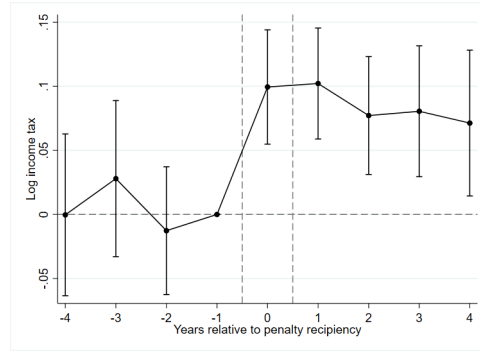
(a) Log income v.
Log penalty amount



(b) Log income v.
effective penalty rate



(c) Log taxes paid v.
Log penalty amount



(d) Log taxes paid v.
effective penalty rate

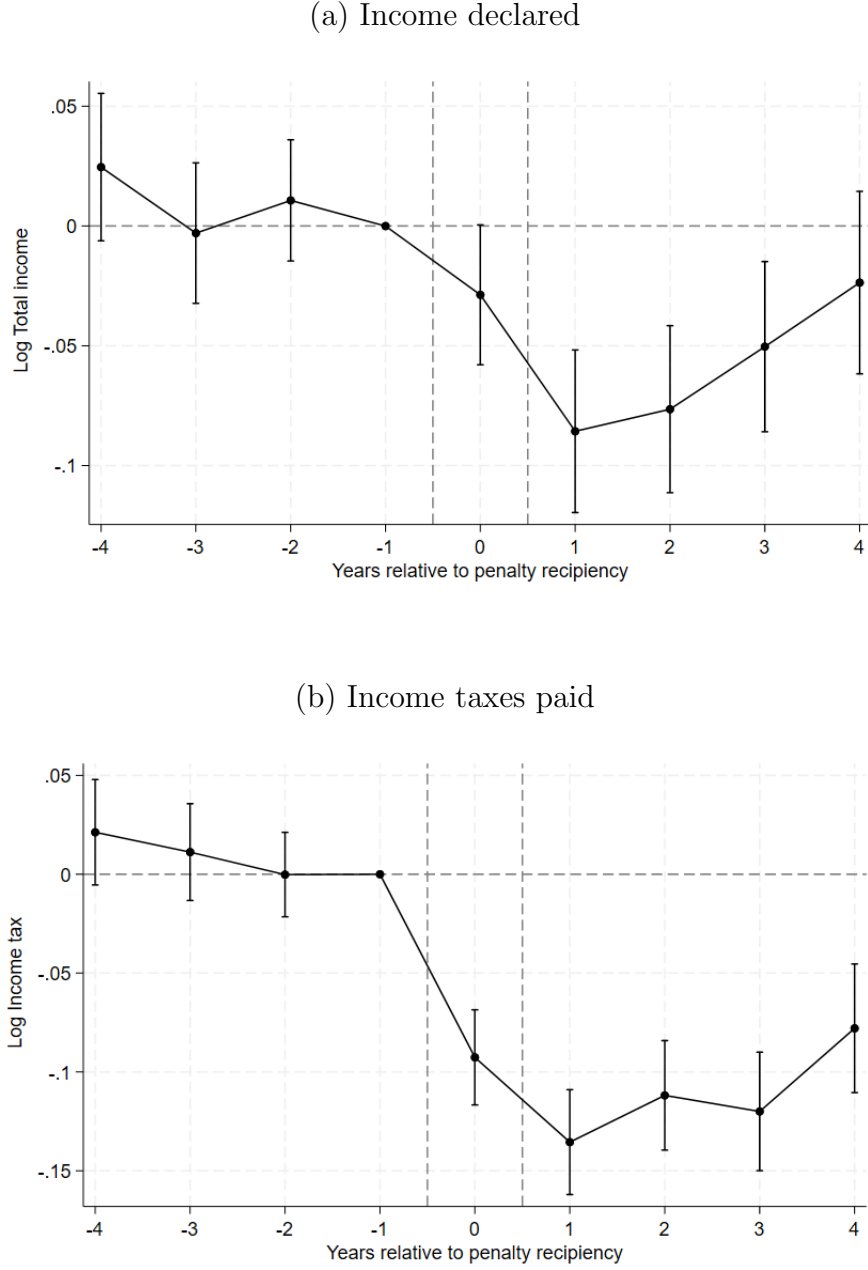
Note: These figures plot the treatment effect coefficients $\{\hat{\beta}_k\}$ and 95% confidence bands associated with the instrumental variables difference-in-differences event study $y_{it} = \alpha_i + \delta_{t,d(i)} + \sum_{k=-5}^5 \gamma_k EventTime_{k(it)} + \sum_{k=-5}^5 \beta_k \theta_i EventTime_{k(it)} + \varepsilon_{it}$, 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 at the taxpayer level. This figure is referred to in Section 3.

Table A.8: 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 $\times$ Post-event	-0.075 (0.013)	-0.068 (0.012)	-0.078 (0.012)	-0.10 (0.0098)	-0.12 (0.010)	-0.13 (0.010)
Constant	12.8 (0.045)	2.77 (0.070)	3.10 (0.069)	10.3 (0.039)	2.64 (0.12)	2.15 (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 (1), 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. This table is referred to in Section 3.4.

Figure A.9: Income- and tax payment responses to the decrease in statutory penalty rate
Correlated Random Effects



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 $y_{it} = \alpha_0 + \Gamma X_i + \sum_{k=-5}^5 \gamma_k \text{EventTime}_{k(it)} + \sum_{k=-5}^5 \beta_k \text{EventTime}_{k(it)} \cdot 1\{\text{Penalty Year}_i \geq 2016\} + \varepsilon_{it}$. 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. This figure is referred to in Section 3.4.

Table A.9: 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 $\times$ Post-event	-0.065 (0.014)	-0.063 (0.013)	-0.073 (0.013)	-0.068 (0.012)	-0.079 (0.014)	-0.078 (0.013)	-0.088 (0.014)	-0.084 (0.012)
Constant	12.7 (0.0090)	12.8 (0.0068)	2.84 (0.077)	2.82 (0.070)	13.0 (0.050)	12.8 (0.046)	3.14 (0.079)	3.07 (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 (1), however replacing the taxpayer fixed effect with taxpayer-fixed characteristics indicated in the bottom half of the table. The dependent variable in all columns is 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. This table is referred to in Section 3.4.

Table A.10: 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 $\times$ Post-event	-0.096 (0.011)	-0.097 (0.0097)	-0.13 (0.011)	-0.12 (0.010)	-0.10 (0.011)	-0.10 (0.0098)	-0.14 (0.011)	-0.13 (0.010)
Constant	11.4 (0.0074)	11.5 (0.0058)	2.91 (0.13)	2.69 (0.12)	10.3 (0.042)	10.3 (0.040)	2.17 (0.13)	2.05 (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 (1), however replacing the taxpayer fixed effect with taxpayer-fixed characteristics indicated in the bottom half of the table. The dependent variable in all columns is 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. This table is referred to in Section 3.4.

Table A.11: Pre-penalty balance of matching sample of ordinary-rate penalty recipients
Post- v. pre-reform penalty recipients

Dependent variable	Pre-reform penalty recipients	Post-reform penalty recipients	Difference
Log total income	13.3 [.69]	13.4 .73	.02 (.0018)
Penalty size	13,548 [57,141]	12,938 32,935	-609 (626)
Effective penalty rate	.02 [.03]	.02 .03	-.0002 (.0004)
Has self employment income	.26 [.39]	.23 .37	-.0288 (.01)
Log income tax	11.9 [.91]	11.9 .95	.0044 (.0023)
Log gross wealth	13.9 [2.29]	14.1 2.38	.1 (.01)
Any transfer abroad	.29 [.36]	.33 .37	.04 (.01)
Any transfer havens	.03 [.13]	.04 .14	.01 (.002)
Has a majority business ownership stake	.29 [.43]	.4 .46	.11 (.01)
Age	45 [11.6]	45.6 11.5	.59 (.15)
Immigrant	.18 [.38]	.18 .38	-.0039 (.01)
Female	.19 [.39]	.2 .4	.01 (.01)
Married	.51 [.48]	.5 .48	-.014 (.01)
University	.33 [.47]	.35 .48	.02 (.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. This table is referred to in Section 3.4.

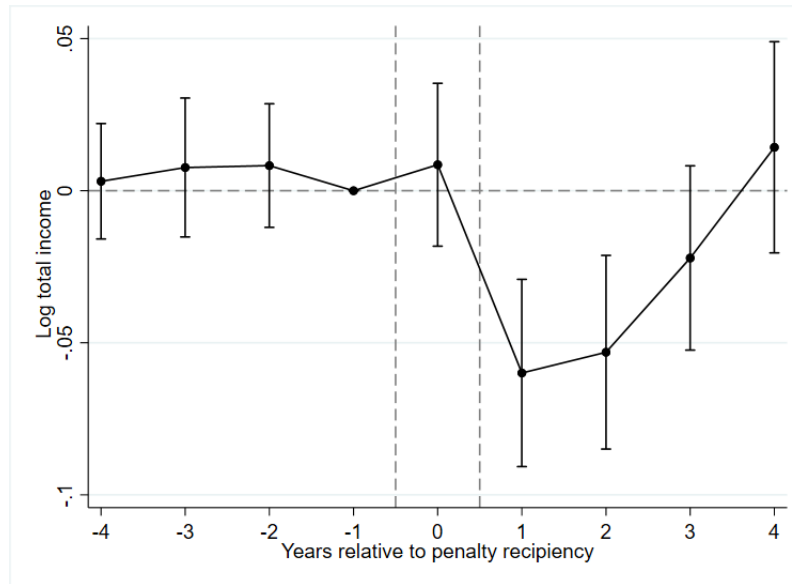
Table A.12: Alternate specifications for penalty rate reform:
Five nearest neighbor Mahalanobis matching

	(1)	(2)
	Log total income	Log income tax
Post-reform $\times$ Post-event	-0.026 (0.011)	-0.035 (0.011)
Constant	13.3 (0.0028)	11.9 (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

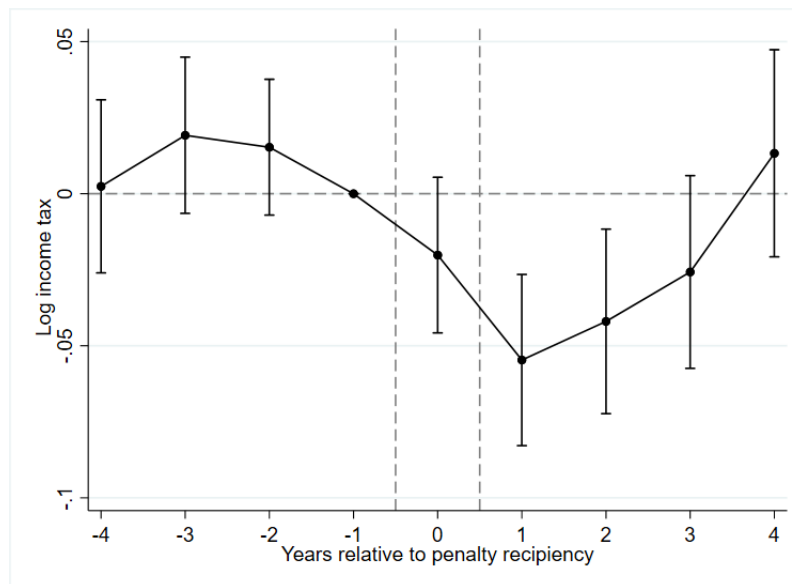
Note: This table displays results from a series of difference-in-difference event study designs evaluating the impact of the decrease in penalty rate on penalized individuals. The columns report coefficients estimated from models corresponding with a period-coarsened version of the difference-in-differences event study design given by the reduced form Equation (1), 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. This table is referred to in Section 3.4.

Figure A.10: 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 $y_{it} = \alpha_i + \sum_{k=-5}^5 \gamma_k \text{EventTime}_{k(it)} + \sum_{k=-5}^5 \beta_k \text{EventTime}_{k(it)} \cdot 1\{\text{Penalty Year}_i \geq 2016\} + \varepsilon_{it}$ 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. This figure is referred to in Section 3.4.

Table A.13: Spillovers effects of the penalty rate decrease

Panel (a): Spillovers onto family members

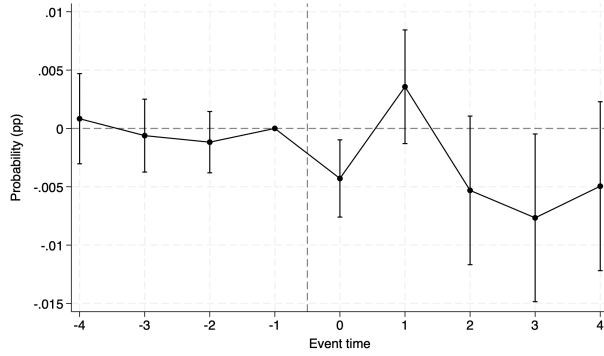
	(1)	(2)
	Log total income	Log income tax
Post-reform $\times$ Post-event	-0.071 (0.012)	-0.093 (0.010)
Constant	12.6 (0.0028)	11.1 (0.0023)
ID fixed effects	Yes	Yes
Observations	319,083	293,014
Number of clusters	30,410	29,742
F-statistic	229	562
Adjusted R-squared	.6	.64

Panel (b): Spillovers onto business partners

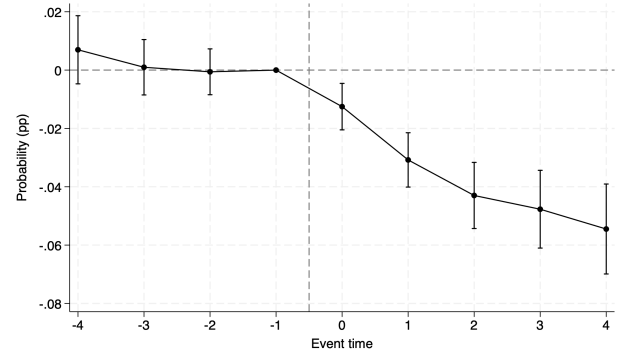
	(1)	(2)
	Log total income	Log income tax
Post-reform $\times$ Post-event	-0.080 (0.018)	-0.066 (0.019)
Constant	13.1 (0.0043)	11.7 (0.0046)
ID fixed effects	Yes	Yes
Observations	78,697	76,895
Number of clusters	7,541	7,522
F-statistic	154	145
Adjusted R-squared	.55	.58

Note: These tables display results 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. This figure is referred to in Section 3.4.

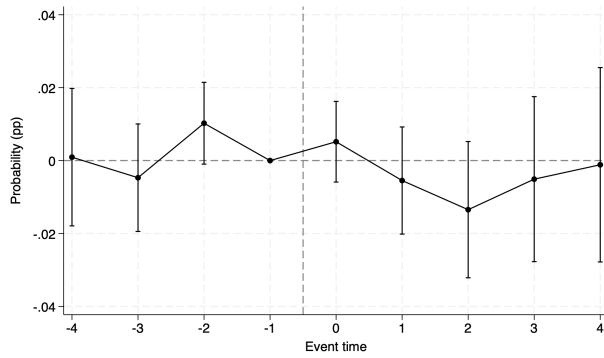
Figure A.11: Auxiliary responses to the decrease in statutory penalty rate



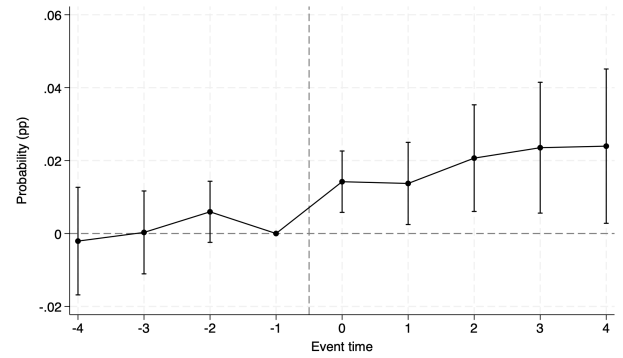
Panel (a): Bankruptcy



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



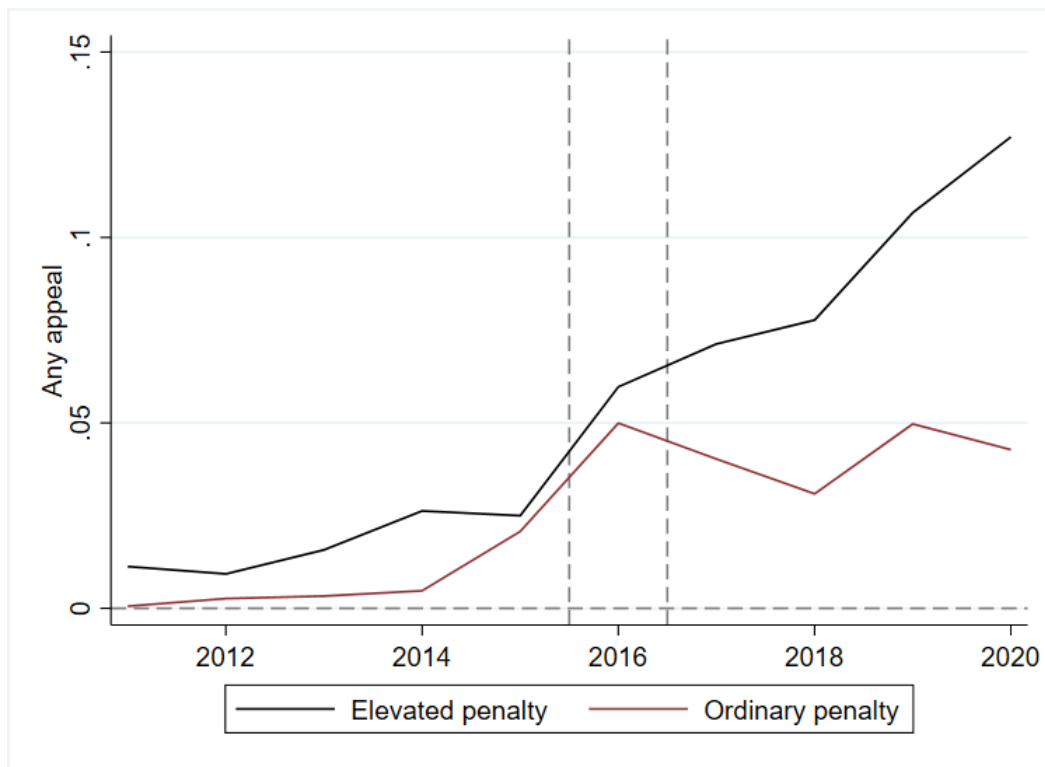
Panel (c): Move within municipality



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 at the taxpayer level. This figure is referred to in Section 3.4.

Figure A.12: Appeals by type over time



This figure plots the evolution of appeals linked to elevated- and ordinary rate penalties over the 2011-2020 period. This figure is referred to in Section 4.

Table A.14: Impact of the penalty rate decrease on appeal probability
Controlling for risk score

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ordinary regime $\times$ Post	-0.038 (0.017)	-0.025 (0.0093)	-0.036 (0.017)	-0.024 (0.0097)	-0.040 (0.017)	-0.025 (0.0096)	-0.037 (0.017)	-0.024 (0.0099)
Ordinary regime	-0.012 (0.0039)	-0.010 (0.0025)	-0.014 (0.0034)	-0.011 (0.0025)	-0.0026 (0.0054)	-0.0016 (0.0040)	-0.0051 (0.0044)	-0.0025 (0.0037)
Post	0.077 (0.014)	0.062 (0.0077)			0.078 (0.014)	0.065 (0.0076)		
Log penalty					0.0051 (0.0018)	0.0063 (0.0023)	0.0052 (0.0017)	0.0064 (0.0023)
Constant	0.017 (0.0037)	-0.067 (0.027)	0.041 (0.0048)	-0.041 (0.027)	-0.038 (0.018)	-0.078 (0.031)	-0.014 (0.018)	-0.052 (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 at the year level are included in parentheses. This table is referred to in Section 4.

Table A.15: 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 (0.0089)	1.062 (0.011)				
Rate of leave-out mean penalty			0.689 (0.0061)	0.684 (0.0061)		
Log rate of leave-out mean penalty					0.991 (0.0025)	1.007 (0.0025)
Constant	0.816 (0.087)	-1.234 (0.11)	0.447 (0.040)	0.501 (0.041)	-0.646 (0.0084)	-0.599 (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 (0.0089)	0.998 (0.0096)				
Leave-out mean penalty rate			0.996 (0.018)	0.995 (0.020)		
Leave-out mean log penalty rate					0.997 (0.014)	0.997 (0.015)
Constant	0.0142 (0.081)	0.0167 (0.087)	0.0312 (0.13)	0.0374 (0.15)	-0.0101 (0.054)	-0.0109 (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 results 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 $g(\hat{\theta}_{-i})$; 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. This table is referred to in Section 4.

Table A.16: IV results: impact of penalty rate on taxpayer-appeal decision (alternate instrument)

	(1)	(2)	(3)	(4)	(5)	(6)
Log penalty	0.0071 (0.0023)	0.014 (0.0039)	0.015 (0.0039)			
Penalty rate				0.000094 (0.000082)	0.0014 (0.00030)	0.0024 (0.00052)
Log gross income			-0.000035 (0.00052)			0.018 (0.0050)
Log gross wealth			0.0021 (0.00084)			0.0019 (0.00086)
Constant	-0.044 (0.021)			0.024 (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 at the year level are included in parentheses. This table is referred to in Section 4.

Table A.17: IV results: Appeal versus penalty size,
By penalty type

	(1)	(2)	(3)	(4)	(5)	(6)
Log penalty	0.0072 (0.0019)	0.0082 (0.0022)	0.020 (0.0064)	0.027 (0.0092)	0.014 (0.0037)	0.018 (0.0052)
Log gross income		-0.0020 (0.0013)		-0.013 (0.0065)		-0.0082 (0.0036)
Log gross wealth		0.0016 (0.00056)		0.0094 (0.0023)		0.0030 (0.00085)
Fixed effects	Year	Year	Year	Year	Penalty type $\times$ Year	Penalty type $\times$ 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 included in parentheses. This table is referred to in Section 4.4.

Table A.18: IV results: impact of penalty rate on taxpayer-appeal decision:
Municipality $\times$ year $\times$ Penalty regime leave-one-out mean penalty instrument

	(1)	(2)	(3)	(4)
Log penalty	0.0067 (0.0017)	0.0063 (0.0021)	0.011 (0.0030)	0.012 (0.0040)
Log implied corrected income		-0.00034 (0.0017)		-0.0042 (0.0029)
Log gross wealth		0.0021 (0.00031)		0.0024 (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 (.367)	-3.42 (.37)	-31.4 (4.83)	-30.5 (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 $\times$ Year $\times$ penalty regime level. The first stage regresses log penalty amount on the log instrument. Standard errors clustered at the municipality $\times$ year $\times$ penalty regime level are included in parentheses. This table is referred to in Section 4.4.

Table A.19: Appeal versus log penalty size
Taxpayer fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
Log penalty	0.0036 (0.0014)	0.0036 (0.0014)	0.0042 (0.0019)	0.0047 (0.0023)	0.0048 (0.0023)	0.0057 (0.0030)
Log gross income			-0.0018 (0.0033)			-0.0030 (0.0036)
Log gross wealth			-0.00021 (0.00069)			-0.00021 (0.00069)
Constant	-0.015 (0.013)	-0.016 (0.013)	0.0062 (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 included in parentheses. This table is referred to in Section 4.4.

Appendix B 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 countervailing 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.

B.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 .²⁰ 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 $\frac{\partial \phi}{\partial z} > 0 \forall x, z$.²¹
- 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.²²
- 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.

B.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 $T'(x) = \xi'_1(z_1) = \xi'_2(z_2)$.

²⁰The 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.

²¹We assume that ϕ does not depend on the manner of underreporting, but rather the overall amount of detected underreporting.

²²One 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.

In the presence of audits, ignoring endogenous appeal effort, the taxpayer optimizes²³

$$\begin{aligned} \underset{z_1, z_2}{argmax} & (1 - \rho_1) \left(y - T(x) - \xi_1(z_1) - \xi_2(z_2) \right) + \\ & \rho_1(1 - \rho_2) \left(y - T(x + z_1) - \phi(z_1; x) - \xi_1(z_1) - \xi_2(z_2) \right) + \\ & \rho_1\rho_2 \left(y - T(y) - \phi(z_1 + z_2; x) - \xi_1(z_1) - \xi_2(z_2) \right) \end{aligned}$$

Such that $y - T(y) - \xi_1(z_1) - \xi_2(z_2) - \phi(z_1 + z_2) \geq 0$.²⁴

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

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

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 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 $z_2^* = 0$

²³For 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.

²⁴We 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.

except in the knife-edge case where $U'(z_1^*) = U'(z_2^*) > 0$.²⁵

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 .²⁶

$$\begin{bmatrix} \frac{\partial z_1^*}{\partial \phi} \\ \frac{\partial z_2^*}{\partial \phi} \end{bmatrix} = - \begin{bmatrix} \frac{\rho_1}{\xi_1''} \\ \frac{\rho_1\rho_2}{\xi_2''} \end{bmatrix}, \quad (15)$$

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 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

²⁵ z_1^* and z_2^* represent a utility-maximizing allocation if they satisfy the second-order conditions:

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

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.

²⁶In 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.

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 increases usage of the second safer vehicle.²⁷

B.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 $T(x + z) - T(x) + \phi(z; x)$.²⁸

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 $r(\alpha, \beta; \theta, z)$.²⁹ The taxpayer ultimately bases her appeal decision and effort (extensive and intensive margins) to maximize payoff:

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

B.3.1 Intensive margin appeal decision

Conditional on appeal, the taxpayer chooses appeal effort to maximize

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

²⁷Generally, 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.

²⁸We assume no false discovery on part of the audit.

²⁹This central specification features legal expenses as not reimbursed by the losing party. Section B.4.1 considers the alternative where the winning party's legal expenses are covered by the losing party.

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:

$$\varepsilon_{r, \alpha^*} = -\frac{\alpha^*}{r(\alpha^*)\Lambda}. \quad (16)$$

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,

$$\alpha^* = -\varepsilon_{\tilde{r}, \alpha^*} \Lambda^* = -r(\alpha^*) \varepsilon_{r, \alpha^*} \Lambda^*, \quad (17)$$

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.³⁰

Applying the implicit function theorem to the first-order condition allows us to gauge

³⁰The second-order condition for this interior solution to produce an optimum is:

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

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

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.

B.3.2 Extensive margin appeal decision

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

$$\begin{aligned} T(x^* + z^*) - T(x^*) + \phi(z^*; x^*) &> F + \alpha + r(\alpha^*, \beta; \theta, z^*, x^*) \left(T(x^* + z^*) - T(x^*) + \phi(z^*; x^*) \right). \\ \implies (1 - r(\alpha^*, \beta; \theta, z^*, x^*)) \Lambda^* &> F + \alpha^*. \end{aligned}$$

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.

B.4 Joint optimization

Section B.5 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 brevity, 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.

B.4.1 Income underreporting, audits, and penalties with two underreporting vehicles and legal expense reimbursement

Consider an identical setup to Section B, 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:

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

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:

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

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 the quantity “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,

$$\alpha^* = \frac{-\varepsilon_{r,\alpha^*}}{1 + \varepsilon_{r,\alpha^*}} \cdot (\Lambda + \beta), \quad (18)$$

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:

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

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):

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

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

B.5 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

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

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:

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

However, in the case where ex-ante, the taxpayer intends on litigating, optimal underre-

porting is characterized by the alternate first-order condition:

$$z_{L=1}^* : T'(y - z_{L=1}^*) = \xi'(z_{L=1}^*) + \rho r(\alpha)(T'(y - z_{L=1}^*) + \phi'(z_{L=1}^*)). \quad (19)$$

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.

Appendix C Additional optimal tax administration model results

This appendix develops results referenced in Section 5, including the first-order conditions for the tax rate and for enforcement instruments that do not themselves raise revenue; further characterizations of the optimal dual-revenue instrument; the optimal compliance and recoup gaps; strategic interactions among instruments; and special cases of the optimal penalty rate under the parametric form used in the calibration.

C.1 Additional first-order conditions

Tax rate first-order condition. Differentiating Equation (8) with respect to the linear tax rate and applying the envelope theorem gives the necessary condition for an interior optimum:

$$W_t = -(z + r_t) + v' \cdot (z + tz_t + r_e e_t + r_t) = 0. \quad (20)$$

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

$$\frac{t}{1-t} = \frac{1}{\varepsilon_{z,1-t}} \cdot \left(\frac{v' - 1}{v'} \cdot (1 + \epsilon_{r,t} \frac{r}{tz}) + \epsilon_{r,e} \varepsilon_{e,t} \frac{r}{tz} \right). \quad (21)$$

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:

$$W_\alpha = -c_\alpha + v'(tz_\alpha + r_e e_\alpha - a_\alpha) = 0 \quad (22)$$

$$\frac{\alpha \cdot (c_\alpha/v') + \alpha a_\alpha}{(t - r_e)z} = \varepsilon_{z,\alpha} := \phi. \quad (23)$$

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 (23) 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

$$\alpha^* = \frac{(t - r_e)z}{(c_\alpha/v') - a_\alpha} \cdot \varepsilon_{z,\alpha}. \quad (24)$$

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).

C.2 Further results for dual-revenue instruments

Equation (10) prescribes that the elasticity of underreporting with respect to ξ equal the welfare-weighted mechanical change in collections relative to the tax gap. Presuming

$\varepsilon_{e,\xi} \leq 0$, the two sides carry opposite signs, which can be rationalized in two ways. In the empirically relevant case $t > r_e$, and since $\epsilon_{r\xi} > 0$ it must hold that $\frac{a_\xi}{r_\xi} > \frac{v'-1}{v'}$: the marginal cost-revenue ratio of the instrument exceeds the relative valuation of public and private consumption.

The alternative is $r_e > t$, under which the expected recovery on underreported income exceeds the income tax rate. A risk-neutral taxpayer who correctly perceived the environment would then not underreport at all, so the case arises only where the marginal administrative cost of the instrument is very low. We note that in this case the planner's apparent preference for underreporting over honest reporting reflects the treatment of collections on detected underreporting as revenue; in the resource accounting of Slemrod and Yitzhaki (1987) such collections are transfers rather than social gains.

We cannot express ξ^* in closed form, as ξ^* appears in the partial derivatives of r . For $\varepsilon_{e,\xi} \neq 0$, $r_e < t$ and $\frac{a_\xi}{r_\xi} \neq \frac{v'-1}{v'}$, one expression of $\xi^* > 0$ is

$$\xi^* = \varepsilon_{z,\xi} \cdot \frac{z(t - r_e)}{r} \cdot \left(\frac{\epsilon_{r\xi}(1 - v'(1 - \frac{a_\xi}{r_\xi}))}{v'} \right)^{-1}. \quad (25)$$

Dual-revenue instruments follow an elasticity rule opposite in sign to that of the optimal income tax, increasing with the responsiveness of taxpayers: a greater elasticity of reported income $\varepsilon_{z,\xi}$ implies greater effectiveness in inducing substitution from underreporting toward honest declaration. Greater marginal costs a_ξ imply a lower ξ^* . With risk-neutral taxpayers who correctly perceive the penalty environment and costs monotonically increasing in ξ , the maximum optimal ξ^* satisfies $r_e(\xi^*) \equiv t$.

Two limiting cases illustrate further properties of ξ^* .

Case 1. $\varepsilon_{z,\xi} = 0$: when taxpayers are unresponsive to the instrument, the first-order condition reduces to the requirement that the marginal cost-revenue ratio equal the relative valuation of public and private consumption:

$$\frac{a_\xi}{r_\xi} = \frac{v' - 1}{v'}. \quad (26)$$

Case 2. $a_\xi = 0$: this benchmark is of interest because changing the penalty rate, unlike changing the audit rate or intensity, incurs zero *nominal* cost (Becker (1968)); Section 4

estimates the real administrative costs it nonetheless generates. Here the first-order condition simplifies to:

$$\frac{v' - 1}{v'} \cdot r_\xi = e_\xi(t - r_e). \quad (27)$$

Since $\varepsilon_{e,\xi} < 0$, the condition implies $r_e > t$, and taxpayers would continue to underreport only under risk-loving preferences or behavioral internalities.

Finally, noting that for a taxpayer at her envelope $e_\alpha = e_\xi \cdot c_{e\alpha}/r_{e\xi}$,³¹ substituting into the first-order condition in α yields a relationship between the optimal levels of the two instruments:

$$\frac{r_\xi/v' + a_\xi - r_\xi}{c_\alpha/v' + a_\alpha} = \frac{r_{e\xi}}{c_{e\alpha}}. \quad (28)$$

The left-hand numerator is a first-order approximation of the marginal welfare cost of increasing ξ and the denominator that of increasing α . At the optimum, the ratio of marginal welfare costs equals the ratio of the curvatures of each instrument's cost function from the taxpayer's perspective.

C.3 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

$$\frac{G}{1-G} = \frac{-\varepsilon_{z,\alpha}}{\varepsilon_{e,\alpha}} = \frac{-\phi}{\varepsilon_{e,\alpha}}, \quad (29)$$

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

³¹In elasticity terms, $\varepsilon_{e,\alpha} = \frac{c_{e\alpha}\alpha}{r_{e\xi}\xi} \cdot \varepsilon_{e,\xi}$.

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.³²

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 9 we have that

$$\frac{r(t, e, \xi^*)}{e} = \frac{r(t, e, \xi^*) - r(t, 0, \xi^*)}{e - 0} \approx r_e(\xi^*) = t + \left(1 - \frac{a_\xi}{r_\xi} - \frac{1}{v'}\right) \cdot \frac{r_\xi}{z_\xi} \quad (30)$$

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).

C.4 Complementarity and substitutability among instruments

To what extent do these instruments operate as strategic complements or substitutes at the optimum? The question is relevant when one or more instruments cannot feasibly be adjusted, perhaps due to political economy constraints. Keen and Slemrod (2017) pose one such question: if α is constrained below its optimum, is it better to raise the statutory tax rate to offset weak administration, or to lower it to expand the base in light of suboptimally high underreporting? We ask this and related questions in our extended environment.

First, it is straightforward to apply the proof in Keen and Slemrod (2017) Equation (14) and their Footnote 23, replacing α with ξ , to show that taxable income elasticities with

³²An identical relationship between z and e holds for instrument ξ , such that an analogous condition arises with nearly identical interpretation:

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

respect to the penalty instrument and the tax rate exhibit the symmetry $\frac{\partial \varepsilon_{z,1-t}}{\partial \ln \xi} = \frac{\partial \varepsilon_{z,\xi}}{\partial \ln(1-t)}$. If an increase in the penalty instrument is known to reduce the elasticity of taxable income, it follows that an increase in the tax rate raises the elasticity of reported income with respect to the penalty instrument.

We address six questions concerning the strategic interactions of the tax rate, administrative enforcement effort, and penalty instruments:

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

Throughout we assume constant v' for simplicity. Questions (3) and (4) are symmetric with (1) and (2) respectively, so we treat the latter pair and then turn to (5) and (6).

C.4.1 If ξ is constrained, (when) should the tax administration raise t ?

Applying the implicit function theorem to the first-order condition in the tax rate (Equation (20)) gives the derivative of the marginal welfare impact of t with respect to ξ . A positive value indicates strategic complements, a negative value substitutes. The sign of the slope of the best response function $t(\xi)$ is given by the sign of

$$W_{t\xi} = \underbrace{(v' - 1) \cdot (r_{t\xi} + z_\xi)}_{>0} + \underbrace{v' \cdot r_{e\xi} \cdot e_t}_{>0} + v' \cdot (t - r_e) \cdot z_{t\xi}. \quad (31)$$

It is sufficient but not necessary for t and ξ to be complements that $(t - r_e)$ and $z_{t\xi}$ share a sign.

The first two terms are positive: an increase in the penalty instrument raises revenue both by discouraging underreporting and by increasing collections on infra-marginal underreporting. If $t < r_e$, however, the administration expects greater collections on marginal

underreporting than on marginal honest reporting, which undermines complementarity when $z_{t\xi} > 0$. We take $z_{t\xi} \geq 0$, meaning the taxable income response to the tax rate attenuates at higher penalty levels. Since we find no empirical settings in which $t < r_e$, we heuristically view t and ξ as strategic complements.

C.4.2 If ξ is constrained, (when) should the tax administration raise α ?

Applying the implicit function theorem to the first-order condition in α (Equation (22)), the slope of the best response function is given by the sign of

$$W_{\alpha\xi} = -(a_{\alpha\xi} - r_{e\xi}e_\alpha) + z_{\alpha\xi}(t - r_e). \quad (32)$$

Unlike the relationship between ξ and t , the first two terms point toward substitutability. The administrative cost function is likely weakly convexly increasing, so $a_{\alpha\xi} \geq 0$, and equals zero if administrative costs are additively separable between ξ and α . The second term reflects reduced collections on underreporting through the enforcement instrument's negative effect on underreporting. The third term is ambiguous, depending on whether $(t - r_e)$ and $z_{\alpha\xi}$ are of the same sign.

It is sufficient but not necessary for substitutability that $t < r_e$. Since $z_{\alpha\xi} \geq 0$, an increase in the penalty instrument reinforces the enforcement instrument, so that under $t < r_e$ the substitution from underreporting toward honest declaration reduces collections. As noted above, however, we intuitively think of $t \geq r_e$, so the sign condition does not deliver substitutability here. The sign instead depends on the relative magnitudes of second cross-derivatives of the administrative cost, collections, and declaration functions, which we cannot determine empirically. Under the assumption that the declaration response to enforcement is constant in the penalty rate, the third term vanishes and the two instruments operate as substitutes.

C.4.3 Is it better to raise funds with t , ξ , or α ?

Should policymakers finance additional public spending by raising the statutory tax rate, by increasing enforcement, or by increasing the penalty instrument? The first comparison

is equivalent to asking whether there is a welfare gain from increasing ξ while reducing t so as to leave government spending unchanged. Evaluating $dW = W_t \frac{dt}{d\xi}|_g + W_\xi$ gives a rule for when it is better to raise funds through ξ than through t :

$$\frac{z_\xi(t - r_e) + (r_\xi - a_\xi)}{z + r_t + (tz_t + r_e e_t)} > \frac{r_\xi}{z + r_t}. \quad (33)$$

As a sufficient but not necessary condition, using $e_t < -z_t$, it is better to raise funds through t than through ξ if

$$\frac{z_\xi(t - r_e) + (r_\xi - a_\xi)}{z_t(t - r_e) + z + r_t} < \frac{r_\xi}{z + r_t}. \quad (34)$$

Turning to α and ξ , we ask whether there is a welfare gain from increasing ξ while reducing α so as to leave public spending unchanged. Evaluating the sign of $dW = W_\alpha \frac{d\alpha}{d\xi}|_g + W_\xi$ yields

$$\frac{(r_\xi - a_\xi) + z_\xi(t - r_e)}{-a_\alpha + z_\alpha(t - r_e)} > \frac{r_\xi}{c_\alpha}, \quad (35)$$

so that the planner should prefer the penalty instrument over enforcement when the net marginal revenue ratio of penalties to enforcement exceeds the ratio of privately borne costs.

C.4.4 Prioritization among dual-revenue instruments

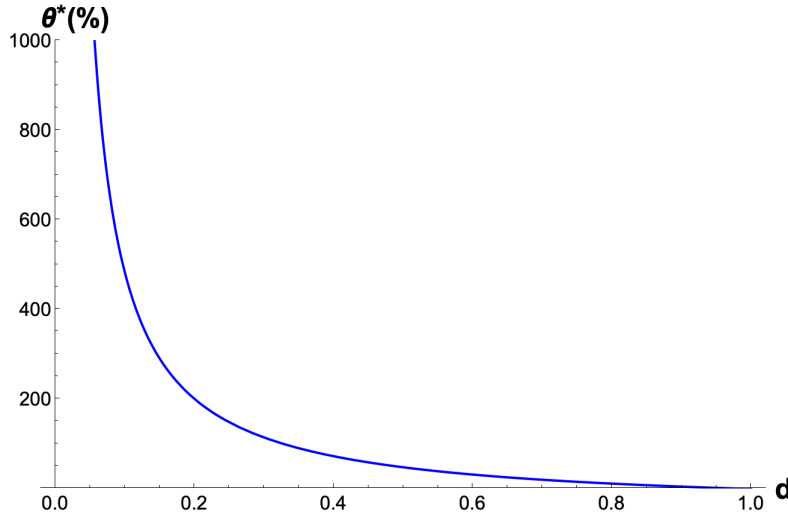
Suppose the administration has multiple dual-revenue instruments, ξ_i and ξ_j , perhaps the audit rate and the penalty rate. If the audit rate is constrained, should the authorities raise the penalty rate? The question differs from those above in that both instruments carry mechanical and behavioral effects on collections. Evaluating the sign of $W_{\xi_i} \frac{\partial \xi_i}{\partial \xi_j}|_g + W_{\xi_j}$ and defining $R_i := r_i + z_i(t - r_e) - a_i$ as the marginal net fiscal gain from increasing ξ_i , when the administration's 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:

$$\frac{R_{\xi_i}}{R_{\xi_j}} = \frac{r_{\xi_i}}{r_{\xi_j}} \quad \forall \xi_i, \xi_j \in \Xi. \quad (36)$$

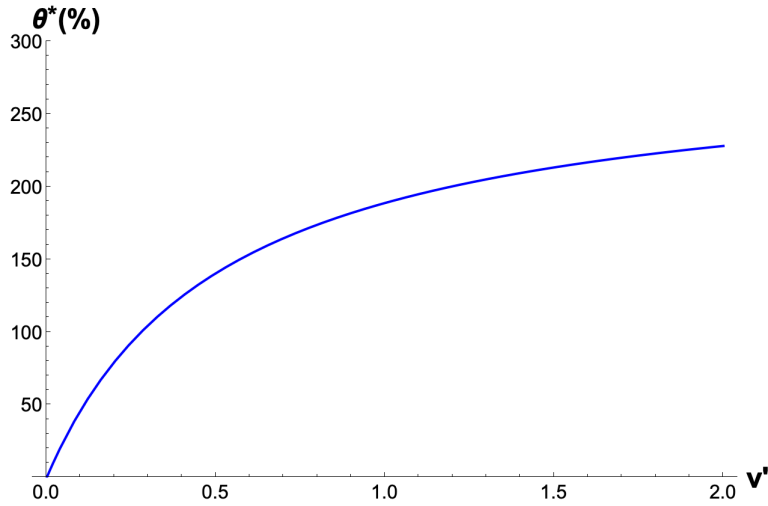
Where equality fails, the planner should increase ξ_i if the ratio of marginal net fiscal gains

exceeds the ratio of privately borne marginal costs. Empirically this depends on the infra-marginal revenues from each instrument as well as on real marginal administrative costs and taxpayer behavioral responses.

Figure C.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 5.2. 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. This figure is referred to in Section 5.2.