Targeting in Tax Behavior: Evidence from Rwandan Firms

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Abstract

The tax behavior of small firms in low income countries shapes government revenues and the welfare of poor entrepreneurs. This paper provides evidence that how these firms respond to tax instruments diverges from traditional models of compliance in ways that have unintended and regressive consequences. Using the universe of administrative filings in Rwanda, I document perverse responses to changes in liability: an income tax reform that standard models would predict should lead all taxpayers to pay lower taxes in fact caused firms to increase tax paid by 75%. To explain this behavior, I establish a new stylized fact: firms consistently target past liability when paying taxes, even when the structure of liability changes. Many firms bunch sharply on their previous amount of tax paid year after year and stick to this level despite changes in their tax rate. Others increase tax paid rather than deviating downward from past levels when changes to the tax schedule remove their ability to pay the same amount as before. Evidence from a survey of filers and a randomized information experiment imply that firms’ uncertainty about own earnings generates reliance on the heuristic of previous liability, while enforcement perceptions and peers influence how firms select target amounts. Ultimately, this behavior produces regressive outcomes: less educated and less profitable entrepreneurs are more likely to overpay relative to their true liability.

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1 Introduction

Small firms comprise the majority of taxpayers in the developing world. Yet how these firms navigate taxation is little understood. The typical entrepreneur is poor, often illiterate and undereducated, characteristics which may predispose them to encountering frictions in navigating incentives. At the same time, non-standard aspects of the underlying tax environment, stemming from limited enforcement capacity within the tax authority, may distort how firms respond to taxes. Tax structures meant to simplify compliance for firms — through lower rates and limited reporting requirements — are becoming increasingly common in these settings. However, in developed countries, similar structures have been shown to produce regressive outcomes when poorer entrepreneurs are more likely to make mistakes in filing (Aghion et al. 2018). How does the tax behavior of firms in the poorest countries shape the impacts and distributional implications of such policies?

I offer an empirical answer to this question by estimating how firms in a low income country respond to a change in tax incentives. Using the universe of administrative filings in Rwanda, I measure responses to an income tax reform. I implement surveys and an information experiment with taxpayers to shed light on the drivers of firms’ responses and to examine distributional consequences.

I begin by documenting perverse responses to a reduction in liability. The reform I consider universally lowers liability for all affected firms, with the average size of the reduction amounting to a 40% tax cut. Pooling all firms, I use an event study approach to estimate the average impact of the reform on tax behavior. Standard models would predict that firms should pay less when liability decreases. Instead, I find that firms on average increase taxable income by 100% and tax paid by 75%, relative to pre-reform levels. This effect persists six years after the reform.

This response cannot be explained by increases in production. I find no evidence of impacts on real production measures like sales and employment, and the effect is several orders of magnitude larger than benchmark estimates of taxable income responses in both developed and developing countries (Chetty 2009; Kleven and Waseem 2013; Bachas and Soto 1). Such firms account for 80–90% of taxpayers in a typical developing country (World Bank 2011). In Rwanda, small firms — earning less than USD 4,000 in annual revenues — are 75% of taxpayers. 2

1 Such firms account for 80–90% of taxpayers in a typical developing country (World Bank 2011). In Rwanda, small firms — earning less than USD $4,000 in annual revenues — are 75% of taxpayers.


3 These can include audit thresholds that concentrate scarce enforcement resources on larger firms but which may distort responses to tax rates, or unequal allocation of enforcement across firm types — e.g., targeting industries with the highest levels of evasion.

4 “Simplification” of tax structures involves reducing the size of the tax burden and the costs of compliance. This can include levying tax rates as low as 0.5%, taxing turnover rather than profits, and imposing taxes on a lump sum basis. Coolidge and Yilmaz (2016) provide an overview of simplified tax regimes.

5 Absent a tax cut stimulating large growth in production, firms may increase taxable income but should pay less when the amount of liability decreases (Allingham and Sandmo 1972).
To understand this behavior, I design and implement a survey of 1,000 taxpayers that I match to administrative declarations. I first show that many firms possess poor information about their own earnings. Only half of firms keep any form of accounts or receipts, of which approximately 90% are handwritten logs of transactions. Only 15% employ accountants, and less than 3% pay quarterly value-added tax, which for payers provides a de facto record of revenues. Such constraints in tracking annual earnings undermine entrepreneurs’ ability to measure the revenues that determine their tax obligations, and to provide proof of those earnings if subjected to audit. Uncertainty about one’s true taxable income generates uncertainty about true liability, presenting scope for reliance on heuristics or rules of thumb.

I then document evidence of a particularly salient heuristic: targeting of previous liability, in which firms seek to pay the same amount of taxes year after year. I present a new stylized fact: that targeting is the predominant mode of compliance among small firms. In approximately half of all administrative income tax filings over a ten year period, the amount paid by a firm is identical to the amount paid the year before.\(^6\) This proportion far exceeds a naïve baseline expectation of year-to-year correspondence given normal growth patterns and annual fluctuations in income.\(^7\) I confirm that this behavior appears across payment amounts, firm sizes, and sectors, and cannot be explained by bunching on salient amounts like round numbers.

Targeting correlates strongly with proxies for uncertainty in taxable income. Survey measures, linked to administrative filings, suggest that firms’ poor information about their own earnings drives targeting behavior. Conditional on income, employing an accountant, keeping business records, and paying value-added taxes are negatively correlated with paying the same amount of taxes year after year. Perceptions of and past contact with enforcement are uncorrelated with this behavior. These patterns imply that entrepreneurs rely on the heuristic of past payments when unsure of their taxable earnings and true liability. Firms may target payment amounts year-to-year in place of taxable income levels because they observe payments directly but cannot measure taxable earnings with certainty.

In a next step, I examine how the targeting heuristic interacts with the reform and whether it can explain firms’ responses. To do so, I decompose the reform’s average effect by heterogeneity in responses to the specific changes in liability that firms encounter. Reforms of the type I consider are becoming increasingly widespread in developing countries.\(^8\) Before the reform, taxpayers face a uniform 4% marginal tax rate on turnover. The reform assigns firms either to remain in the marginal-rated regime and face a reduced 3% rate or to move to a

\(^6\)Figure 3 shows the distribution of the ratio of year-to-year payments.

\(^7\)Average annual growth in GDP in Sub-Saharan Africa over this period is estimated to be between 1 and 5%; annual per capita growth in GDP between 1 and 2% (World Bank 2018)

\(^8\)In Sub-Saharan Africa, Zambia, South Africa, Tanzania, Zimbabwe, Uganda, and Rwanda have recently implemented such policies (Africa Tax Report 2017).
new tax regime composed of a graduated schedule of lump sum payments. The lump sum schedule imposes a reduction in liability of between 25 and 100%, varying with a firm’s location in the taxable income distribution pre-reform. The nature of tax reduction a firm receives depends on its regime assignment and reported pre-reform size.

First, I consider firms remaining in the “linear” (marginal-rated) regime, who receive a uniform 25% cut in rate. Estimating the event study separately on this group, I find that these firms increase taxable income by 30% on average. This parallels the predicted targeting response of 33% to a 25% rate cut: $TI' = TI \times (0.04/0.03)$. Despite the change in rate, the effect on tax paid is zero, and firms bunch sharply on the amount of taxes they paid in the year before the reform. That firms increase taxable income but stick to previous liability implies that payment amounts are the salient target rather than reported earnings.

In contrast, the bulk of firms newly assigned to the lump sum (“Flat Tax”) regime lie within a range of taxable income in which the new schedule exempts them from income tax payment. Using the same approach, I find that these firms move out of the new exemption range to the next tax bracket up, where post-reform liability remains positive and more closely approximates their pre-reform level. Within this new bracket, they bunch sharply at the level of reported earnings closest to their old one. This movement, however, involves increasing reported taxable income by a factor of two on average. Tax paid among these firms increases by more than double, driving the overall average effect.

I can rule out that these firms are motivated by a fear of paying zero. At the time of the reform, other lump sum firms are placed in higher brackets where liability is not reduced to zero. For many, the change in schedule similarly removes their ability to pay the same amount as before. These firms likewise move to brackets even further up, where liability is nearer to their previous level, significantly increasing both reported income and tax paid.

Firms in both regimes avoid paying less than what they paid before — targeting previous liability conditional on not deviating downwards — even when it means paying more. Given uncertainty in taxable income, and the inability to document true earnings, firms err on the side of paying more when presented with the option of moving up or down from previous payment levels. As a result, tax payment ratchets up. A high level of perceived enforcement may drive this asymmetry: in the survey, firms report audit likelihoods of 48% on average,

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9 Figure 1 shows reform detail.
10 Evidence from my survey of taxpayers indicates that regime assignment is not endogenous to factors that may bias estimates of the responses to the reform across regime subgroups. As a rule, firms were assigned to the marginal-rated regime if they kept accounts and to the lump sum regime if they did not. I discuss the assumptions involved in separating firms by regime in Section 3.4.
11 The lump sum payment amount for this range — between 0 and 2 million RwF in revenues — is zero; however, taxpayers are still required to report taxable income despite being exempt. Tax liability increases to a flat amount of 60,000 RwF (USD $69) in the next bracket up in the lump sum schedule.
12 Firms declaring taxable income between 2 and 4 million RwF pre-reform receive a similar-sized tax cut, but the new liability corresponding to their reported pre-reform size is still positive.
and 75% are aware of the existence of penalties for evasion, which include large fines and business closure.

I consider alternative stories for the patterns but find little evidence to support them. I first assess whether responses to the reform can be explained by how the tax authority enforces obligations. Combining audit records with taxpayer reports of enforcement experiences, I find no discontinuities in audits or visits by tax officials that could rationalize the observed behavior.\(^{13}\) I also find no evidence that tax officials, who might be personally incentivized to raise collections, force firms to stick to payment amounts — taxpayers do not interact with officials in the act of payment and only 12% of surveyed firms report being told by officials what to declare. Second, I examine whether low knowledge about the tax structure among taxpayers could by itself generate mistaken responses. If firms make mistaken tax choices immediately post-reform, the average effect on tax paid should weakly decrease over time as some firms update and reoptimize. Instead, I find sticky responses: firms bunch strongly year-to-year on the levels of liability they select after the reform, and, if anything, the effects increase over the post-reform period. Moreover, in the survey, a majority of taxpayers can describe the structure of liability within their regime. Finally, I assess whether entrepreneurs’ tax morale could motivate targeting behavior — if firms derive positive utility from taxes as civic contributions — but show that firms are not more likely to target liability when their access to public goods is greater or they view the state more positively.

I then map responses to the reform to their distributional implications. Comparing reports of revenues in the survey to actual administrative declarations — matched by firm — for the most recent fiscal year, I estimate that approximately 30% of firms are overpaying relative to their true liability.\(^{14}\) I then show that the response to the reform is regressive. Less educated and less profitable taxpayers are more likely to overpay relative to their true tax liability and to counterparts of the same size. This regressivity stems from the assignment of these taxpayers to the lump sum regime. Given firms’ tax behavior, the regressive outcomes are a function of the assignment of more vulnerable firms to the regime that generates a larger increase in tax paid. This result runs counter to the explicit intentions of the reform: to simplify the tax burden and enhance the progressivity of the tax schedule by exempting the poorest firms from tax obligations.\(^{15}\) My findings imply that, in low income settings, how small firms navigate taxation can lead the design of traditional tax incentives to backfire.

Finally, to assess the potential value of information interventions in improving tax choices, I test whether firms’ choices are responsive to new information. As part of the survey, I con-

\(^{13}\) The audit likelihood for small firms is less than 1% and smooth across firm size. Perceived audit probabilities among firms are higher, but are also not discontinuous across levels of taxable income or payment amounts.

\(^{14}\) See Section 7.1 for calculation details. Given that many firms encounter uncertainty in determining true earnings, I assess the robustness of this outcome to under-reporting of taxable income.

\(^{15}\) This finding parallels recent work showing that tax and transfer policies in Sub-Saharan Africa increase rather than reduce poverty and inequality (Inchauste and Lustig 2017).
duct a randomized experiment priming firms with information about audit likelihoods and the choices of local peers. I assess impacts on survey experimental outcomes and administrative declarations and payments. Firms respond only to information that other similar firms are declaring less than they are. This information produces negative effects on actual tax paid and the perceived likelihood of audit. These findings show that firms are responsive to new information about peers, but are consistent with both a channel of reducing uncertainty in the estimation of own earnings — firms updating about the size of their taxable income after learning what firms like them are declaring — and one of reducing perceived risk of enforcement — firms updating on the likelihood they will be audited given that others are declaring less.

This paper relates to several strands of literature. First, it contributes to the growing set of studies estimating responses to tax instruments in developing countries. This work documents how theory and estimation techniques designed for analysis in developed countries must be adjusted to account for differences in enforcement and tax behavior (Kleven and Waseem 2013; Best et al. 2015; Bachas and Soto 2017; Waseem 2018). It adds in particular to work evaluating the simplification of income taxation in these settings (Gordon and Li 2009; Best et al. 2015; Coolidge and Yilmaz 2016), made relevant by the increasing prevalence of these policies (Africa Tax Report 2017).

It also contributes to the literature exploring behavioral responses to incentives in developing countries and their implications for welfare (Kremer et al. 2013; Kaur et al. 2015; Andreoni et al. 2017; Schilbach 2018; Blumenstock et al. 2018). I show that behavioral explanations like targeting can help to understand non-standard responses in the context of taxation, but my findings run counter to recent evidence suggesting that heuristics can improve financial decisions among entrepreneurs in developing countries (Drexler et al. 2014).

Finally, this paper relates to the literature examining the welfare and regressivity implications of taxation. Recent work in developed countries shows that differences in the likelihood of making mistakes in tax decisions affect the incidence and regressiveness of tax instruments: including complex incentives (Abeler and Jäger 2015), non-salient sales taxes (Taubinsky and Rees-Jones 2018) and simplified income tax schedules (Aghion et al. 2018). This paper shows that how governments assign tax instruments across taxpayer types — even with progressive intentions — can make more vulnerable individuals worse off when their behavioral responses are not accounted for.

The rest of the paper proceeds as follows. Section 2 describes the setting and data, Section 3 the empirical strategy, and Section 4 the results. Section 5 discusses explanations for firms’ responses. Section 6 describes distributional implications, and Section 7 the information experiment. Section 8 concludes.
2 Setting and Data

2.1 Small Firms in Rwanda

I briefly highlight characteristics of the tax environment in Rwanda to provide context for the following analysis, which focuses on small firms.

Rwanda is a typical low-income country, where the majority of citizens live on less than USD $2 per day (World Bank 2018). Similar to other poor countries, the majority of the tax base is made up of small firms (World Bank 2011). In Rwanda, small firms — defined as earning less than 50 million RwF (USD $56,500) in annual revenues — comprise 99% of all taxpayers. The mass of firms is concentrated towards the bottom of the taxable income distribution, with 90% of firms earning annual revenues less than 12 million RwF (USD $14,000) and 70% less than 2 million RwF (USD $2,300).

The representative firm in Rwanda is owner-operated and employs less than two workers. The median owner has not graduated secondary school. The majority of firms are in the wholesale, retail, or service sectors and transact entirely in cash. Only half keep some form of business accounts, with the most common form being handwritten records. The average firm has been in the tax base for 3 years and pays only income tax at the federal level. Less than 3% of small firms also pay value-added or payroll taxes, but almost all businesses pay an annual business license fee to their district government.\textsuperscript{16} Entrepreneurs operate on very small margins: average profits for the bottom 70% of taxpayers total USD $1,000 annually or USD $2.8 per day. The average amount of income taxes paid annually is USD $150, or 15% of profits.

Given limited resources, the tax authority concentrates enforcement on the largest firms in the economy, which generate 60% of total revenues.\textsuperscript{17} Small firms, by contrast, receive comparably weaker forms of oversight, including invitations upon registration to workshops explaining tax obligations, text message reminders to pay around deadlines, and brief informal visits by field teams of tax officials.\textsuperscript{18} The likelihood of a small firm experiencing an audit on income tax declarations in a given year is less than 1%, though it is common to receive fines.

\textsuperscript{16} The “patente” is a flat fee levied by the district that firms pay in order to operate on an annual basis. The fee varies by firm size, ranging from USD $69 for small firms to USD $289 for large firms. Depending on the district, firms are also required to pay security and cleaning fees on a monthly basis to public or private entities that provide these local amenities.

\textsuperscript{17} Only 400 firms in Rwanda are classified as large (>1 billion RwF or USD $1,130,000 in annual profits) but contribute more than half of total revenues, mostly through value-added taxes. These firms receive strict oversight in the form of monthly monitoring visits or calls and the ability of the revenue authority to requisition accounting records.

\textsuperscript{18} The revenue authority deploys teams of tax officials around tax deadlines. Their primary function is to do cursory checks of accounts, remind taxpayers of deadlines, and provide information about tax obligations. The teams, consisting of between 6 to 8 officials, typically walk through market areas in urban centers on a quarterly basis to conduct these duties. 70% of small firms report being visited by a tax authority official in the last 6 months.
for late filing or payment.

2.2 Income Tax Schedule and Reform

The Rwandan income tax system divides firms into profit and turnover bases. Large firms pay taxes on annual profits, while small firms pay taxes on annual turnover (revenues).\(^{19}\) The ability to pay taxes on turnover rather than profits, though strictly production inefficient (Diamond and Mirrlees 1972), is intended to lessen the reporting burden on small firms: they need not keep detailed records of costs and can fully comply with declaration requirements by reporting just one number — their total turnover — each year.\(^ {20}\) Eighty-five percent of income taxpayers in Rwanda report in the turnover base.

The reform I consider alters the size of liability and the tax schedule for all firms in the turnover base.\(^ {21}\) It was intended to improve tax compliance among small firms by lowering barriers to accurate reporting. Before the reform, taxpayers in the turnover base faced a uniform marginal tax rate on turnover of 4%. In July 2012, the middle of the fiscal year, the reform introduced a choice between remaining in the marginal-rated regime and facing a reduced 3% rate or opting for a new tax regime composed of a graduated schedule of lump sum payments. The lump sum schedule imposed a reduction in liability of between 25 and 100%, varying with a firm’s location in the taxable income distribution pre-reform. The reform universally reduced liability for all firms paying income taxes on turnover, with the size of the reduction depending on regime assignment and reported firm size pre-reform. The average size of the reduction amounted to a 40% tax cut.

Figure 1 summarizes the changes to the income tax schedule induced by the reform, which include three salient alterations.\(^ {22}\) First, firms remaining in the linear regime received a uniform 25% reduction in the marginal tax rate on annual turnover, across all levels of taxable income.\(^ {23}\) Second, firms assigned post-reform to the lump sum ("Flat Tax") schedule received a conversion of liability to a lump sum amount, with variation in the size of the tax reduction depending on pre-reform income.\(^ {24}\) At the time of the reform, firms were legally permitted

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\(^ {19}\) In the period I study, firms generating less than 50 million RwF (USD $56,500) in annual turnover are permitted to declare in the profit base, but in practice less than 5% of firms below this threshold elect to do so. Before 2018, firms above 50 million RwF in annual turnover were required to report in the profit base; in July 2018, the threshold was reduced to 20 million RwF (USD $22,600).

\(^ {20}\) Recent work shows that this form of taxation can be revenue efficient in settings where opportunities for evasion are high (Best et al. 2015).

\(^ {21}\) Profit taxpayers were unaffected by the reform.

\(^ {22}\) The lump sum schedule in its totality includes five bins of liability (see Appendix Figure 1). I focus on the bottom two bins for the purposes of estimation, as this range accounts for approximately 75% of all taxpayers; bins further out possess sparser samples.

\(^ {23}\) All firms in the turnover base received this tax cut, applying the reduction in rate to firms generating less than 50 million RwF (USD $56,500) in annual turnover.

\(^ {24}\) Only firms earning less than 12 million RwF (USD $14,000) in annual turnover were eligible for the Flat Tax schedule — between 12 and 50 million RwF, firms must report in the “linear” 3% rated regime. However, 90%
to choose a regime. In practice, firms did not actively select schedules: most were automatically assigned by the tax authority to the linear schedule if they kept business accounts and assigned to the lump sum schedule if they did not.\textsuperscript{25} The creation of the lump sum schedule was intended to aid firms without record-keeping technologies to more easily comply with obligations.\textsuperscript{26} Third, as part of the lump sum schedule, the reform exempted firms earning less than 2 million RwF (USD $2,300) from income taxes.\textsuperscript{27} Firms could obtain this exemption only within the lump sum schedule, otherwise they faced a 3% marginal tax rate in the linear regime despite declaring income within the exemption-eligible range.

Though framed as a “simplification,” the reform produced a new tax structure with added complexity. For instance, at the bottom of the distribution, the lump sum regime dominates the linear regime for almost all values of taxable income. Appendix Figure 1 shows that further up the schedule, the reverse is true. This odd aspect underscores the reform’s objective of creating two entirely separate schedules, assigning firms by their level of sophistication to tailored regimes rather than offering a menu with dominated choices. The simplicity lay in reducing the tax burden overall and providing less sophisticated firms with the ability to pay lump sum amounts by selecting a turnover bin, rather than requiring them to estimate taxable income precisely to compute tax liability.

2.3 Data Sources

I use data from three sources: administrative filings and payments, survey data matched to taxpayer declarations, and records of audits.

**Administrative Filings and Payments:** The administrative data comprises the universe of federal tax returns filed and payments made by Rwandan taxpayers between the 2008 and 2017 fiscal years, for all federal taxes: income, value-added, payroll, customs, and excise. I primarily use income tax filings and payments as they capture the total federal tax burden for most firms in my sample. Filings for other tax types are used as measures of firm heterogeneity in total tax burden and to capture spillovers across tax bases. Income tax declarations are made annually and, for the majority of firms, require reporting only turnover as a single line item.\textsuperscript{28} The data also includes quarterly prepayments for income tax, which are manda-
tory payments of 25% of the total income tax paid in the previous year. Registration details of taxpayers upon entry into the income tax base provide firm industry, geographic location, and owner name and phone number, which I use to conduct taxpayer surveys.

**Survey of Taxpayers:** I collected survey data independent of the Revenue Authority between January and March 2018 through phone interviews with firm owners. Firms were randomly sampled from FY2016 income tax declarations among those declaring less than 4 million RwF (USD $4,600) in annual turnover. The sample was selected to be nationally representative — with taxpayers interviewed in 241 of Rwanda’s 416 geographic sectors — and representative across firm industry types. The survey collected measures of firm and owner characteristics, access to public goods and services, tax compliance, knowledge of and experiences with taxation, audit perceptions, and intended income tax declarations for 2017 (due on April 3, 2018, just after the conclusion of the survey period). Finally, the survey contained a randomized information experiment and collected survey-experimental outcomes on audit perceptions and intended declaration amounts, with actual 2017 administrative filings providing a measure of real behavior. Surveys are linked to the full administrative dataset for each taxpayer, including the complete history of filings, payments, and audits.

**Audit Records:** The third dataset comprises administrative records of audits conducted by the revenue authority for two audit types — comprehensive and desk/issue — with the date, reason, and outcome, for the years 2014 to 2017. Comprehensive audits are full audits in which taxpayer records are requisitioned, extensive staff time is dedicated, and detailed reports are produced. Desk and issue audits are ad hoc reviews conducted by an office-based team directed to inspect declarations flagged for low values or deviations from previous trends; they entail a call or brief visit by a tax official to verify filing details. Audit records are linked to the administrative filings through taxpayers’ unique identification numbers.

### 3 Empirical Strategy

I implement an event study design to estimate the impact of changes in liability on roughly 14,000 firms whose tax burden was altered by the income tax reform. The first part of this section describes the sample of firms used in the analysis and provides summary statistics. I then describe the empirical strategy and estimating equations.

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29 I restrict to this range to obtain sufficient samples within narrow ranges of turnover. This range capture approximately 75% of all taxpayers in the economy, while data is sparser above 4 million RwF.

30 A substantial share of Rwanda’s mostly rural, geographic sectors have extremely sparse populations of taxpayers. The survey covers all urban areas in the country, which account for the vast share of taxpayers within the sample of interest. See Appendix A for more detail on the survey design and sampling.

31 See Section 7 for a discussion of the experiment.
3.1 Sample

**Balanced Panel of Firms:** I construct a balanced panel of firms declaring positive taxable income in the turnover base in all years for which administrative data is available. I restrict the sample to firms declaring less than 4 million RwF ($4,600 USD) in 2011, the year prior to the reform. I focus on this subset for two reasons: First, the majority of taxpayers concentrate within this range of pre-reform taxable income, which accounts for 75% of firms in the tax base in 2011. Second, for higher ranges of pre-reform income the data becomes sparser, limiting the ability to estimate precise effects. To eliminate concerns related to switching between profit and turnover bases, I drop the small sample of firms who move between these bases over time, which accounts for less than 1% of all firms. Finally, I exclude firms in the transportation sector, which are subject to an idiosyncratic tax schedule.

**Sample for Estimating Heterogeneous Responses:** The balanced panel pools firms in the turnover base that post-reform are assigned in the linear schedule with those assigned to the new lump sum schedule. The average effect estimated on this sample thus pools responses to varying changes in liability, which depend on a firm’s assignment to schedule and pre-reform taxable income. To decompose responses by the specific changes in tax structure that firms encounter, I break up the universe of filers by levels of pre-reform income and regime assignment post-reform. The assumptions involved in this step are discussed in Section 4.4. In estimating impacts among these subsets, I discard the balanced panel requirement and require only that firms declare positive taxable income at least twice in the data — once before and once after the reform. I do this in order to include the full sample of firms whose behavior may be impacted by reform-induced changes in liability. The inclusion of additional firms permits me to obtain more precise estimates and to better capture the full impact of the reform. I continue to exclude base-switchers and transportation firms.

3.2 Summary Statistics

This section provides summary statistics for firms in the samples described in the previous section.

**Characteristics of Firms in the Balanced Sample:** Column (1) of Table 1 reports summary statistics for the 354 firms in the balanced sample in the year before the reform, 2011. Average taxable income is 1.3 million RwF (USD $1,500) and income tax paid 52,509 RwF (USD $60). Slightly less than 3% of firms pay value-added or payroll taxes in addition to income taxes. Sixty-seven percent are in the wholesale/retail sector, while only 15% and 1% are in the services or construction sectors, respectively. Eighty-four percent are located in the capital city region of Kigali, reflecting the concentration of the tax base in this area in earlier periods, as firms must report taxable income in 2008 to enter this sample.
Characteristics of Firms in Heterogeneity Samples: Columns (2) and (3) of Table 1 report summary statistics for the 11,019 and 7,858 firms assigned post-reform to the linear and lump sum regimes, respectively, in 2011. For linear firms, average taxable income is 1.2 million RwF (USD $1,430) and income tax paid 49,583 RwF (USD $57). Approximately 1% of linear firms pay value-added or payroll taxes. Only 11% are in the wholesale/retail sector, while 24% and 1% are in the services or construction sectors, respectively. Forty-five percent are located in Kigali. For lump sum firms, average taxable income is 1 million RwF (USD $1,150) and income tax paid 43,254 RwF (USD $50). Less than 4% pay value-added taxes, and 1% pay payroll taxes. Sixty-seven percent are in the wholesale/retail sector, while 17% and 2% are in the services and construction sectors, respectively. Forty-four percent are located in Kigali.

3.3 Empirical Approach

I exploit the changes in tax liability induced by the income tax reform described in Section 2.2 to estimate the impact of these changes on taxpayer behavior. The law was unanticipated, and compliance with its alterations to the tax schedule were mandatory. These features of the reform permit the application of an event study design to estimate how firm behavior changes immediately following the reform compared to behavior in preceding years. My data provides four years of pre-reform instances and six years post-reform, which allows me both to establish pre-trend measures and to estimate short- and longer-run impacts. Panel data on firms additionally allows me to control for individual-level variation in firm behavior over time.

I focus on two margins of taxpayer behavior: declared taxable income (turnover) and amount of income tax paid.\footnote{The variable drawn from the administrative data is “income tax payable,” which captures the income tax due for the taxable income the taxpayer reported. I confirm that tax payable matches actual tax paid to the tax authority by comparing receipts of actual tax payments.} Taxable income consists only of turnover, reported on an annual basis, while tax amounts are automatically determined by the declaration platform based on the amount of taxable income reported, taking into account the schedule to which a firm is subject (linear or lump sum). This analysis focuses on the intensive margin of taxpayer behavior. Appendix B discusses extensive margin effects.

3.4 Estimating Equations and Assumptions

I implement an event study design over a ten year period to estimate how shocks to income tax liability affected reported taxable income and tax paid. This approach offers the advantages, first, of assessing graphically the evolution of treatment effects in the years following the reform, and, second, of directly evaluating the validity of the pre-trend assumption. In this section, I describe the estimating equations.
Estimating Equation for Trends in Outcomes: I estimate the effect of changes in liability on reported taxable income and tax paid for the balanced panel of firms using the following event study approach:

\[
y_{ik} = \alpha + \gamma_i + \sum_{k=-4}^{5} \beta_k \times I(\text{period}_k) + \varepsilon_{ik}
\]

where \( y_{ik} \) denotes the outcome \( y \) for firm \( i \) in year \( k = t - 2012 \). I include firm fixed effects, \( \gamma_i \). Event time \( I(\text{period}_k) \) denotes a fiscal year relative to the reform year. The coefficients of interest, \( \beta_k \), capture the effect of reform-induced changes in liability in year \( k = t - 2012 \) and are normalized to zero in the year before the reform, \( k = -1 \) (\( \beta_{-1} = 0 \)). I cluster standard errors at the firm level. To estimate the effects, I use variation in liability within firms over time, comparing outcomes relative to \( k = -1 \). This approach allows the pre-trend assumption to be visually tested by observing coefficients in the pre-reform period.

Estimating Equation for Heterogeneous Effects: To capture the effects of specific changes to liability, I decompose the sample into groups that differ in the changes they encounter through the reform. These changes vary by (1) assignment to tax regime and (2) pre-reform taxable income. I divide the sample by whether firms are assigned to remain in the linear regime or are assigned to the lump sum regime and whether pre-reform taxable income lies within or above the future exemption range below 2 million RwF.\(^{33}\) The nature of changes in liability across these subsets is described in Section 2.2. I estimate the following event study approach separately on each individual subgroup:

\[
y_{ik} = \alpha + \gamma_i + \eta_k + \beta \times I(\text{post-period}_k) + \varepsilon_{ik}
\]

where \( y_{ik} \) again denotes the outcome \( y \) for firm \( i \) in year \( k = t - 2012 \). I include firm fixed effects, \( \gamma_i \), as well as period fixed effects, \( \eta_k \). The indicator, \( I(\text{post-period}_k) \), denotes periods post-reform. The coefficient of interest, \( \beta \), captures the average effect of reform-induced changes in liability in years \( k \geq 0 \) for the relevant sample. I cluster standard errors at the firm-level. The individual samples require that firms declare positive taxable income at least once before and after the reform. Section 4.4 discusses pre-trend assumptions and evidence to support the identification assumption that post-reform regime assignment is exogenous.

\(^{33}\)I consider being above or below the exemption threshold a relevant margin of heterogeneity, as for lump sum firms this determines whether they received a conversion of liability to zero or to a positive, lower lump sum amount. The magnitudes of the reductions in levels for lump sum firms on either side of the threshold are identical — they differ only in how large a reduction they represent relative to pre-reform taxable income. I assess responses among linear firms in each range — which receive a uniform 25% rate cut on either side — as a comparison.
4 Results

My main results, presented below, document perverse responses to changes in income tax liability: the reform should have led all firms to pay lower taxes, yet firms double taxable income and increase the amount of taxes they pay by 75% on average, relative to previous levels. This effect does not appear to be driven by increases in production, and it persists six years later.

The average effects mask substantial heterogeneity: firms remaining in the linear regime increase taxable income by 30% but leave tax paid unchanged, bunching on previous liability. Firms moved to the lump sum schedule in range of the exemption, for whom liability is reduced to zero, more than double both taxable income and tax paid. Firms moved to the lump sum schedule whose pre-reform taxable income places them above the exemption range — who receive an identical reduction in the level of the tax burden but continue to face positive liability — likewise double taxable income and increase tax paid by 60%. Both linear and lump sum firms across ranges of income remain at their new, weakly higher levels of liability going forward.

In this section I first discuss the main effects, then I show results on measures of production and responses by regime and firm size. Finally, I discuss potential threats to identification in detail.

4.1 Effects on Taxable Income and Tax Paid

I first show trends in taxable income around the reform for the balanced sample of firms, pooling those opting for both the linear and lump sum regimes. Figure 2(a) shows that the reform is a shock to declaration behavior. I show the effect on log taxable income in the years post-reform amounts to an average doubling of reported taxable income. In the first declaration period after the change in liability, firms increase taxable income by 75% relative to the previous year’s level. In the following year this effect climbs to 100% and persists five years later. A positive response in taxable income to a tax cut is not surprising: firms may increase reported income due to increases in real production spurred by a lower tax burden or may choose to evade less given the marginal benefit of evasion in terms of avoided liability has declined. However, if taken literally, the size of the response implies a large taxable income elasticity: the observed response to an average 2% increase in the net-of-tax rate yields an elasticity of 50, which is 12 to 25 times those estimated in other developing country settings (Kleven and Waseem 2013; Bachas and Soto 2017) and approximately 33 to 150 times estimates from developed country settings (Chetty 2009), suggesting that standard models of tax behavior are insufficient to account for the response I estimate.

Next, I show that these increases in taxable income correspond to an increase in tax paid.
Figure 2(b) shows that the reform shocks this dimension of behavior. I show the effect on log tax payable for the same sample of firms, documenting an increase of 50% in the declaration period following the reform and an average increase of 75% across post-reform years. The increase in tax paid is sticky, implying that if firms mistakenly increase tax paid post-reform they are not learning to correct this behavior over time.\textsuperscript{34,35}

The effect on tax paid — that firms increase the amount they remit to the tax authority in response to a reduction in their liability — is the central fact of this analysis. The perversity of this response given traditional understandings of tax behavior implies that existing models, developed to explain taxpayer choices in developed country settings, are irreconcilable with those of small firms in low income countries like Rwanda.

4.2 Effects on Production

To estimate whether the reform affects real outcomes like sales and employment, I assess the impact of the income tax reform along these margins. To do so, I use data on value-added and payroll tax payments, which also capture measures of sales and firms’ wage bill. Within the turnover base, less than 3% of firms within the sample of those declaring less than 4 million RwF in 2011 pay VAT and payroll taxes.\textsuperscript{36} However, Appendix Figure 6 shows that payers of these taxes exhibit similar responses to the reform in the income tax base, increasing taxable income and tax paid in response to the reform. Assessing production responses among these firms helps to determine whether the observed response in taxable income and income taxes paid reflects real changes in production.

I estimate a version of equation (1) on these outcomes for the balanced panel of firms described in Section 3.1, including only those that declare and pay either VAT or payroll tax. The outcomes are in levels of RwF, Winsorized at the top 10% of values, to permit zero values, of which there is a high proportion, to enter the regression. Appendix Figure 7 shows that there are no detectible changes along sales or employment simultaneous with the reform. The spillovers on the tax bases associated with these measures, pooling across post-periods,

\textsuperscript{34}I collect actual receipts of payments from the tax authority for 2008–2016 and show using equation (1) that impacts in real tax paid match increases in tax payable. Appendix Figure 3 shows that the impacts estimated on income tax payable (tax owed by firms as a result of reported taxable income) correspond to actual increases in tax paid (payments received by the tax authority).

\textsuperscript{35}I show that the results on taxable income and tax paid are broadly robust to an alternative identification strategy. Using a difference-in-differences design comparing turnover taxpayers (treated by the reform) to profit taxpayers (untreated by the reform), I find similar results. Appendix Figure 4 shows trends for both groups, and Appendix Figure 5 shows outcomes in a balanced panel. The event study approach is preferred as profit taxpayers comprise a distinct group of taxpayers and do not offer a perfect control: these taxpayers are, controlling for taxable income, more likely to pay value-added and payroll taxes, more likely to keep accounts, and are anecdotaly more sophisticated firms than those reporting in the turnover base.

\textsuperscript{36}Only firms earning at least 20 million RwF in annual turnover are required to pay value-added taxes. Firms with formal employees earning more than 30,000 RwF (USD $35) per month are required to pay payroll taxes.
are also non-significant.\textsuperscript{37}

These estimates indicate that the change in taxable income and tax paid embody reporting responses rather than real changes in production, or that if responses in the income tax base reflect real changes they are not drawn from margins that comprise the most observable components of production — sales and employment. Additionally, the estimates rule out the possibility that responses to the reform derive from simultaneous growth spurred by factors unassociated with the reform. GDP growth over this period in Rwanda exhibits no discontinuous jumps that could explain the increase in reported taxable income (World Bank 2018). This interpretation may be restricted to taxpayers subject to VAT and payroll taxes, who may differ in degree of sophistication or of oversight they receive from the tax authority, yet it is consistent with evidence from other settings that responses to changes in income tax incentives are primarily driven by reporting (Kleven and Waseem 2013; Bachas and Soto 2017; Waseem 2018).

4.3 Heterogeneous Responses by Regime

I next assess heterogeneity in effects by distinct changes in liability, which are functions of pre-reform taxable income and regime assignment. Through the reform, these measures imply differing changes in the amount and structure of the tax burden: firms remaining in the linear schedule receive a uniform 25% tax cut, regardless of pre-reform taxable income, while firms moving to the lump sum schedule receive an exemption (100% reduction) if their pre-reform taxable income is less than 2 million RwF and a 25 to 62.5% reduction if pre-reform taxable income locates them in the next bracket up.

Table 2 summarizes the heterogeneous responses in log taxable income and log tax paid. Panel B columns (2) and (5) present effects among firms moving to the lump sum schedule locating pre-reform within the range of the exemption’s future applicability and document the largest point estimates across subgroups: these firms more than double reported taxable income and tax paid when their taxes go to zero. Importantly, this effect does not appear driven by a fear of paying zero: Panel B columns (3) and (6) show that lump sum firms in the next interval who receive a similar-size reduction in the level of the tax burden — but for whom tax liability remains positive conditional on pre-reform turnover — likewise approximately double reported taxable income. Tax paid for this subset increases, though to a lesser degree relative to those receiving the exemption, by 60%.

\textsuperscript{37}The average effect for payroll taxes is driven by a slight increase in taxes remitted two years after the reform, which, in the event study, does not appear to correspond to the change in the income tax schedule.

\textsuperscript{38}The size of the reduction in this range — between 2 and 4 million RwF — depends on the amount of taxable income firms report in 2011: firms declaring 2 million RwF in 2011 receive a 25% reduction while firms declaring 4 million RwF receive a 62.5% reduction, with those in between receiving a reduction somewhere in this range. Compare the dashed red line in Figure 1(a) to the solid blue line in this range in Figure 1(b) for the specific changes in liability by pre-reform taxable income.
Panel C columns (2) and (5) present effects among firms remaining in the linear schedule with pre-reform turnover below the future exemption threshold and show a 30% effect on taxable income and a zero effect on tax paid. The effect on taxable income disappears in the next interval (columns (3) and (6)); however, the small size of this subsample relative to that of other groups means the point estimates are less precisely estimated, and it is not possible to reject equality of coefficients with the sample below the threshold. The effect pooling all linear firms is a 25% increase in taxable income and zero change in tax paid.

These patterns are consistent with differential responses to distinct changes in tax incentives: firms receiving a marginal rate reduction increase taxable income but leave tax paid unchanged, while firms receiving larger reductions in tax burden, and conversion of payments to lower lump sum amounts, massively increase reported income and tax paid.

4.4 Potential Threats to Identification

Here I discuss the identification assumptions for the empirical strategies I implement above.

Assumptions and Potential Threats for Main Effect: The key event study assumption is that changes to liability induced by the income tax reform are exogenous. This implies that firms would have followed existing pre-reform trends in reported taxable income and tax paid in absence of the reform. This assumption can be directly tested by observing pre-period trends in outcomes, which should be flat given no changes in the structure of liability.

Potential threats to identification include anticipation of the reform and contemporaneous shocks. Anticipation of the reform could bias estimates of the effect in either direction depending on the responses of firms in advance of the reform. In Rwanda, tax reforms are not typically announced in advance, and their determination is subject to a parliamentary process which may alter the design in the final stages. Firms do not appear to anticipate changes in liability, exhibiting flat pre-trends in outcomes.

Contemporaneous shocks to tax liability, the tax environment, or the Rwandan economy could bias estimates if they affect responses to changes in liability. If, for example, firms experience sharp growth in the reform year or the tax authority increases enforcement, the estimation approach may conflate responses to these events with those induced by alterations to the tax schedule, biasing estimates of the effect upwards. However, there appear to be no simultaneous changes along margins that are likely to affect the outcomes of interest. The Rwandan economy experiences smooth growth over this period (World Bank 2018), and no

39If firms anticipate increases in outcomes, the effect would be downward-biased; if instead firms underreport in anticipation of increases in post-reform years, the effect would be upward-biased.

40In 2008, there is a negative and significant estimated effect, which disappears in the following year. This reflects the entry of many firms into the tax base in the 2008 — the first year of my data. It is common for firms to declare lower or even zero taxable income in their first tax year as after establishing their business they require time to begin earning. Therefore, earnings in the first year should be significantly lower than earnings in the second year of business.
changes in liability or direct enforcement are implemented alongside the income tax reform. The results in 4.2 suggest no simultaneous growth in production at the firm-level.

**Assumptions for Heterogeneous Effects:** Subgroup analysis requires the additional assumptions that pre-reform income and regime assignment are exogenous. Exogeneity of pre-reform income can be assessed through observation of pre-trends in outcomes, which show flat patterns before the reform, providing strong evidence that firms do not determine taxable income in pre-reform years in expectation of changes in liability.

Assignment to regime, however, is potentially endogenous, given that the decision to declare in the linear or lump sum schedules implies large differences in liability. To be plausibly exogenous, regime assignment must not be endogenous to firm characteristics that affect responses to changes in liability. According to the text of the law, firms are allowed to choose their regime. However, two pieces of evidence support the interpretation of regime assignment as exogenous: First, according to the tax authority, the lump sum regime was explicitly designed for firms that do not keep detailed business accounts. The intention was to permit firms with limited record-keeping capacity to report taxable income within broad ranges with no variation in liability. Anecdotally, revenue authority officials report that firms that do not keep accounts are told by officials to enter this regime, while those that do keep records are told to remain in the linear regime. These administrative guidelines appear to result in regime choices that are not directly determined by the firm. In the survey sample, firms without accounts are much more likely to be in the lump sum regime. Appendix Figure 2 shows the most common reasons provided by respondents for appearing in a particular regime: being automatically enrolled accounts for 60% of responses, while firms making active choices primarily report greater ease of declaration or payment motivating the decision. These responses indicate that the majority of firms (1) do not make an active choice in regime selection and (2) if they do choose, the reasons for their selection are not related to differences in the structure of liability. Taken together, this evidence supports interpreting assignment to regime as one determined with plausible exogeneity, with the decision either being applied exogenously from the perspective of the firm or made for reasons orthogonal to the structure of liability.

Second, in general, despite being informed about their own regime, firms exhibit low levels of knowledge about their counterfactual regime. In the survey, less than one-sixth of turnover taxpayers report being aware of both turnover regimes, while only 7% of taxpayers

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41 The Rwanda Revenue Authority did initiate a new taxpayer awareness campaign beginning in 2012, focusing on disseminating information and reminders about tax obligations; however, for the average small taxpayer, contact with the campaign was limited to text messages and newspaper notices around quarterly tax declaration deadlines. The communication of reminders to declare and pay on time could affect the rate of timely reporting but should not shape the intensive margin of compliance. Restricting analysis to a balanced panel of firms additionally attenuates this concern, as estimation of the effects is limited to firms who are already regularly declaring and paying income tax in the pre-period.
in the Flat Tax regime are aware the linear regime exists. Low levels of awareness about the alternative regime imply that selection of regime is not strongly correlated with awareness of the changes in or amount of liability this choice entails, relative to a firm’s own regime.

5 Explaining Firms’ Responses

In this section, I discuss explanations for firms’ responses to the reform. I first use evidence from the survey of taxpayers, combined with administrative filings, to show that the uncertainty that firms encounter in measuring annual taxable income creates scope for reliance on heuristics in tax compliance. I then document evidence of a particularly salient heuristic — targeting previous liability, or paying the same amount in taxes year after year. I examine how targeting behavior interacts with the reform and assess whether it can explain firm responses. Finally, I discuss potential alternative explanations for responses to the reform.

5.1 Uncertainty in Determining Taxable Income

I first show that many firms possess poor information about their own earnings. Table 3 shows that only half of firms keep any form of accounts or receipts, of which approximately 90% are handwritten logs of transactions. Only 15% employ accountants. Table 1 shows that less than 3% pay quarterly value-added tax, which for payers requires use of an electronic billing machine that provides an automatic digital receipt of the sum of sales, which can then be used to calculate taxable earnings for income tax payments on an annual basis.

Lump sum firms are more likely to lack access to record-keeping technologies. Table 3 shows that lump sum firms are differentially less likely than linear firms to keep accounts or to employ an accountant. Linear firms are 25% more likely to keep some form of accounts or receipt records, and three times more likely to have an accountant that helps with record-keeping.

Constraints in tracking annual earnings may affect tax compliance by undermining entrepreneurs’ ability to measure the revenues that determine their tax obligations, and to provide proof of those earnings if subjected to audit. Uncertainty about one’s true taxable income generates uncertainty about true liability, presenting scope for people to rely on heuristics or rules of thumb in tax compliance.

5.2 Liability Targeting as a Mode of Compliance

I next document the existence of a specific compliance heuristic: targeting previous liability, by which taxpayers pay the same amount in taxes year after year. I establish a new stylized fact: that targeting is the predominant mode of compliance among small firms in Rwanda —
taxpayers rely on the amount of liability they have previously paid in selecting the amount of tax to pay in subsequent declaration periods.

To demonstrate this behavior in the Rwandan context, I construct a measure of targeting in administrative payment records as the share of taxpayers paying precisely the same amount year-to-year in annual income tax:

\[
Targeting_{ik} = \mathbb{1}(z_{ik} = z_{i,k-1})
\]

where \(z_{ik}\) is taxes paid for firm \(i\) in period \(k\), and \(z_{i,k-1}\) is the identical measure in the preceding period \(k - 1\).

In approximately half of all income tax payments over a ten year period, the amount paid by a firm is identical to the amount paid the year before. This proportion far exceeds a naïve baseline expectation of year-to-year correspondence given normal growth patterns and annual fluctuations in income. Figure 3 plots the density of ratios of year-on-year declarations between 2008 and 2017, defining ratios as \(r_i = z_{ik} / z_{i,k-1}\) and excluding the year of the reform: approximately 50% of year-to-year tax payments are identical, with firms paying the same amount, and almost 30% of taxable income declarations follow the same pattern.\(^{42}\)

To confirm that bunching on previous liability is a feature of tax compliance across firm sizes and types — rather than characteristic of a particular subgroup or a result of bunching on round numbers and salient amounts — I measure bunching on the ratio of 1 across levels of taxable income, firm sector, and the “roundness” of taxable income and tax payable in the pre-reform period. I restrict this analysis to the years before the reform in order to exclude any effects the reform may have on this behavior. Figures 4(a) and (b) show that targeting appears consistently across measures of firm size and sector, implying that it is not a select group of firms that exhibit this behavior. Figures 4(c) and (d) show targeting for taxpayers declaring round numbers of taxable income, defined as multiples of 100,000 RwF, and those paying round tax amounts, defined as multiples of 10,000 RwF, compared to those that declare at other values and displays no significant differences. Taxpayers paying round tax amounts appear slightly more likely to bunch on previous liability; however, a substantial mass of taxpayers paying non-round amounts also do so.

5.3 Targeting and Uncertainty in Taxable Income

I next present evidence that targeting behavior is inversely correlated with a firm’s knowledge of its own turnover and the sophistication of a firm’s operations and declaration behav-

\(^{42}\)The difference in the ratios for tax due and taxable income derives from the structure of the lump sum regime after 2012, which makes it easier for firms to pay the same amount in taxes year-to-year as they can alter taxable income within a given range and still face the same liability.
ior. Figure 9 shows correlates of targeting drawn from the administrative data and the survey of taxpayers. I find that firms that keep any form of accounts, report hiring an accountant to help with record-keeping, and that also pay value-added taxes are much less likely to exhibit targeting. These correlations suggest that knowledge of one’s own annual revenues is an important determinant of how firms make year-to-year declaration decisions: value-added taxation requires that firms report quarterly amounts of inputs and sales, which should both make the recording of this information more salient to firms and provide a relatively detailed accounting for cross-checking annual income tax declarations of total revenues. This pattern is consistent with evidence showing that account-keeping impacts firms’ ability to estimate revenues (de Mel et al. 2009). Employing an accountant can provide a mechanism to keep better track of revenues but it may also combine accounting information with advice on tax choices as accountants may lend expertise on strategies for minimizing taxes or evading. Conversely, measures of enforcement intensity are uncorrelated with targeting. Firms that have been audited or visited by a revenue authority official are not more likely to pay identical amounts year-to-year. Likewise, a higher perceived likelihood of audit is unassociated with this behavior.

Sole proprietorships and firms in the wholesale/retail sector are more likely to target. These firms may be predisposed to rely on heuristics like previous payments given sole proprietors lack help in management and wholesalers and retailers must keep track of sales across many different kinds of goods. By contrast, firms in the services and construction sectors are less likely to target, which may result from a greater ability to track revenues given that they produce a more limited range of products or services over the course of the fiscal year.

These correlates imply that targeting behavior is driven by uncertainty in estimating taxable income, rather than enforcement, real or perceived. Measures of information — employing an accountant, keeping business records, and paying monthly value-added taxes — are strongly and negatively correlated with paying the same amount of taxes year after year, while measures of contact with enforcement — past audits, visits by tax officials, and higher perceived enforcement probability — are unassociated with this behavior.

5.4 Targeting and Responses to the Reform

I next explore whether targeting can account for the heterogeneous responses to the reform across regime assignment, separately for linear and lump sum taxpayers.

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43 Administrative correlations are based on the pre-reform period, pooling all years. Correlations from the survey are based only on FY2016 declarations, and whether they match those in FY2015. Appendix Figure 12 shows correlations within regimes.
5.4.1 Linear Taxpayers

Consider a firm remaining in the linear tax regime that seeks to precisely target previous liability in response to a 25% rate cut. This firm must increase taxable income by 33% to precisely match the amount of taxes paid before the change \((TI' = TI \times (0.04/0.03))\). If a firm declared 750,000 RwF in taxable income in the year before the reform, it faced a 4% tax rate and paid 30,000 RwF in income tax. After the reform, this firm faces a 3% tax rate; to continue to pay 30,000 RwF, it must now declare 1 million RwF in taxable income, which involves an increase of \((1,000,000 - 750,000)/750,000 = 1/3\). Figure 5(a) illustrates the predicted movement of this firm along the x-axis: involving an increase in taxable income but no change in tax paid. In the event study, I find effects among linear taxpayers that directly parallel the expected responses: an increase in taxable income of 30% and zero change in tax paid among this group (see Table 2, Panel C).

To assess the direct impact of the reform on targeting among linear taxpayers, I estimate the effect of changes in liability on targeting using the balanced panel of firms declaring in the linear regime before and after the reform using the following event study approach:

\[
Targeting_{ik} = \alpha + \gamma_i + \sum_{k=-4}^{5} \beta_k \times I(\text{period}_k) + \epsilon_{ik}
\]

where \(Targeting_{ik}\) denotes the outcome defined in equation (3) for firm \(i\) in year \(k = t - 2012\). I include firm fixed effects, \(\gamma_i\). Event time \(I(\text{period}_k)\) denotes a fiscal year relative to the reform year. The coefficients of interest, \(\beta_k\), capture the effect of reform-induced changes in liability in year \(k = t - 2012\) and are normalized to zero in the year before the reform, \(k = -1\) \((\beta_{-1} = 0)\). I cluster standard errors at the firm level. To estimate the effects, I use variation in liability within firms over time, comparing outcomes relative to \(k = -1\). Figure 6(a) shows no significant impact on targeting for this group in the years after the reform. As expected, Figure 8(a) shows that firms in the linear regime continue to bunch sharply on previous liability immediately after the change in rate in 2012 and in the years afterwards.

5.4.2 Lump Sum Taxpayers

If firms choosing the lump sum schedule in the exemption range seek to target previous liability, then most are no longer able to do so post-reform, as pre-reform liability amounts within this range are reduced to zero. To approximate previous liability firms would need to move one threshold up to where liability becomes positive, which for the average firm entails a large increase in taxable income and tax paid over previous year’s levels. Consider
the same firm as in the linear case above declaring 750,000 RwF in taxable income and paying 30,000 RwF in income taxes in the year before the reform. If this firm is assigned to the lump sum regime, it may either now pay zero taxes — at its previous level of taxable income — or relocate taxable income to at least 2 million RwF, in order to maintain a positive liability, yet this movement entails a doubling of tax paid to 60,000 RwF. Figure 5(b) shows the predicted movement of this firm along both axes for firms relocating to the higher bracket: a large increase in taxable income and tax paid. I find that the empirical response follows this pattern: these firms increase both taxable income and tax paid by more than double, locating in the range where tax liability remains positive, rather than reducing taxes to zero (Table 2, Panel B).

Assessing the direct impact of the reform on targeting among lump sum taxpayers, I estimate equation (4) on the balanced panel of taxpayers assigned to the lump sum regime post-reform. Figure 6(b) shows that in the year after the reform firms are significantly less likely to target previous income, but on average in post-reform years are much more likely to do so. The impact in 2012 represents the average increase in tax paid relative to the pre-reform period (the distribution of which is displayed in Figure 8(a)); the large positive impacts post-reform show taxpayers are more likely to target, though the strength of this impact in part derives from the relatively greater ease of targeting post-reform due to the structure of the lump sum schedule: firms can alter taxable income within brackets and still face the same income tax liability.

Figure 7 shows that firms in the lump sum regime affected by the introduction of the exemption bunch sharply post-reform on the first amount of taxable income at which liability becomes positive under the new schedule at 2 million RwF. Additionally, excess masses appear above 4 million RwF, capturing the movement of firms within the 2 to 4 million RwF pre-reform to levels of taxable income in the next bracket up, where tax due is nearer to their pre-reform payment levels. This behavior implies that when changes to the tax schedule remove the ability to target previous liability and taxable income precisely, firms move to points that most closely approximate their old values.

**Ruling Out Fear of Paying Zero:** An alternative explanation for the response among firms assigned to the lump sum schedule in range of the exemption, which drives the average increase in tax paid among all firms, is that their behavior derives purely from a fear of remitting zero income taxes to the revenue authority. To address this concern, I compare firms opting for the lump sum schedule one threshold up, which receive a similarly sized reduction in their level of tax liability, but for whom, post-reform, previously-reported taxable income still corresponds to a positive amount of liability. I find that these firms also approximately double taxable income and increase tax paid by 60% (Table 2, Panel B); as such, the response of firms affected by the exemption cannot alone be explained by a fear of remitting
Figure 10 shows changes in tax paid in the year after the reform compared to the year before the reform, for firms declaring between 2 and 4 million RwF in 2011 and assigned to the lump sum regime in 2012. The results in Section 5.4.2 show that many firms within this range relocate to the next highest threshold (above 4 million RwF and below 7,000,001 RwF) post-reform, increasing taxable income by 90% and tax paid by 60% on average. However, the change in liability caused by the reform creates variation within this bracket in the ability of firms to target pre-reform liability within the bracket to which they relocate. At 3 million RwF, tax liability is 120,000 RwF under the old 4% marginal-rated regime, which is identical to new liability in the 4 to 7 million RwF lump sum bracket post-reform. Therefore, firms declaring 3 million RwF in revenues and paying 120,000 RwF in 2011 in income taxes can perfectly target previous liability by moving to the higher bracket in 2012. To the left side of this point, firms increase tax paid by larger margins when they relocate to the higher bracket; to the right side, firms decrease tax payments when they move up one bracket.

Figure 10(a) shows the mechanical consequence of movement to the higher bracket around this point: the further below 3 million RwF firms are pre-reform, the greater increase in tax liability generated by post-reform movement. Figure 10(b) shows, however, that the likelihood of movement is increasing in proximity to 3 million RwF — firms near to 3 million RwF in taxable income are approximately 60% more likely to move to the next highest bracket (which is the only way to increase tax paid in the lump sum regime) than firms declaring 2 million RwF in taxable income in 2011. These patterns imply that, although the reform generated an average increase in tax paid by motivating firms to relocate to higher brackets where post-reform liability more closely approximated their previous levels, firms did not universally relocate. Some firms chose to reduce tax payment. Yet the likelihood of making the move to a higher liability bracket is increasing in the closeness of pre-reform liability to the liability amounts offered by higher brackets. Firms are more likely to increase tax paid the nearer their old liability is to the liability amounts offered by the new lump sum schedule.

5.4.3 Why Not Deviate Downward?

These results imply that many firms avoid paying less than what they paid before — targeting previous liability conditional on not deviating downwards. Given uncertainty in taxable income — and the inability to document true earnings — firms err on the side of paying more when presented with the option of moving up or down from previous payment levels. As a result, tax payment ratchets up. A high level of perceived enforcement may drive this asymmetry: Appendix Figure 11 shows that surveyed firms report audit likelihoods of 48% on average; Table 3 shows that nearly 40% of firms have been punished in the past. Though the majority — 87% — received fines for late payment, the enforcement of such penalties may
generate a perception of high enforcement capacity in the detection of evasion.

Adding to perceived oversight, almost 70% of firms report being visited by tax officials in the last six months — which involve mainly reminders to pay around deadlines — but which may raise firms’ beliefs that they could be audited or their records scrutinized if they are believed to be evading. Seventy-five percent of firms are aware of the penalties for evasion, which include fines and business closure. Though in reality the audit likelihood for small firms is less than 1%, high levels of perceived enforcement may raise the perceived cost of audit relative to the cost of higher tax payments. Recent experimental evidence in a developing country setting shows that firms’ misperceptions of audit probabilities can affect tax payments, and that awareness of audits can drive payment increases by generating irrational fear of enforcement (Bergolo et al. 2017). Firms may believe it is wiser to pay more rather than risk paying less and getting into trouble.

5.4.4 Bunching Estimates

To show the magnitude of bunching on previous liability over time, I estimate bunching on the ratio of 1 for taxable income and tax payable in each year (over last year’s), pooling all firms and by regime.\footnote{Following Best (2014), observations are grouped into bins 0.1\% wide, and the counterfactual is estimated as:}

$$c_j = q \sum_{m=0}^q \beta_m d_j^m + \sum_{r=k^-}^{k^+} \gamma_r \mathbb{1}(j = r) + \mu_j$$

(5)

where $c_j$ is the number of observations in bin $j$, $d_j$ is the distance of bin $j$ to the ratio of 1: $(j - 1)/0.1$, and $q$ is the order of the polynomial. $q = 1$ in Table 4 as the counterfactual mass appears best estimated linearly. Estimations using higher order polynomials yield similar results in the pre-period; however, the size of bunching in the post-period becomes extremely noisy for higher order polynomials because the estimated excess mass is scaled by a counterfactual mass that can be less than 1 or 0. Therefore, I stick to the linear version across all groups. The second term excludes the bins in the region $[\bar{k}^- , k^+]$ around the ratio of 1, and $\mu_j$ is an error term. Standard errors are bootstrapped following the procedure in Chetty et al. (2011).

\footnote{I estimate excess mass at the ratio of 1 as $b = \sum_{r=k^-}^{k^+} (\hat{c}_r - \hat{\epsilon}_r)$, where $\hat{\epsilon}_r$ is the counterfactual mass in bin $r$ predicted using equation (5), excluding the contribution of the dummies in the excluded range. To allow comparisons across estimates, Table 4 shows $B = b/c_0$, a version of the excess mass normalized by the average counterfactual density in the excluded range, $c_0 = \sum_{r=k^-}^{k^+} \hat{c}_r / [(k^+ - k^-)/0.1]$.}
brackets that make it easier to do so.\textsuperscript{46}

In the years following the reform, firms in both the linear and lump sum schedules continue to bunch strongly year-to-year on previous liability: Figure 8(b) shows that more than 50\% of annual income tax payments match the previous year’s in the post-reform years, indicating that the perverse responses to the tax cut are sticky choices rather than mistakes undone by learning over time.

These patterns imply that a large share of small firms in Rwanda navigate tax obligations by consistently targeting a specific amount of liability year after year, and that this behavior can generate perverse responses to familiar tax instruments. This fact is hard to reconcile with traditional models of taxpayer decisions, in which firms optimize with respect to fluctuations in income and changes in liability on the margin (Allingham and Sandmo 1972). Instead, many firms appear averse to paying less than what they paid before, even when it means paying more.

5.5 Alternative Explanations

In this section, I assess potential alternative explanations for the responses estimated in Section 4. I discuss whether enforcement by the tax authority, taxpayer mistakes, and tax morale among firms can reconcile the observed behavior.

5.5.1 Enforcement, Real and Perceived

Revenue-Targeting by Tax Officials: I first consider whether it is the policy of tax officials to force firms to pay the same or minimum amounts of taxes annually. Despite the national government’s intention to reduce the tax burden through the reform, tax officials may still be incentivized to ensure total collections do not decline. For example, local tax agents could be paid based on their ability to hit annual targets in the amount of taxes firms within their domain pay.

However, in Rwanda I do not find support for this behavior, and there are several pieces of evidence to contradict this explanation. First, the majority of firms do not interact with tax officials in the act of payment; in the survey, 79\% of firms report declaring and paying taxes on the phone or computer without interacting with an agent from the tax authority.

\textsuperscript{46}To separate by regime, I classify firms as belonging to the linear regime if they never declare in the lump sum schedule. Firms are classified as lump sum if they declare once in lump sum schedule in the post-period. This is to separate firms that purely participate in the linear regime from those that end up in the lump sum regime eventually, which, according to the assignment rule based on possession of record-keeping technologies, merits their consideration as lump sum firms even if they only start reporting in this regime later (in other words, they should always have been lump sum firms, and I consider them as such). I then project this post-period regime determination on pre-period observations to separate out firms by post-period regime assignment.
Second, only 12% of surveyed firms report being told by a tax official what to declare, indicating that officials do not inform taxpayers of what they should pay in advance of the act of payment. Third, ensuring that the firms target their pre-reform liability would require a significant oversight effort to verify that individual firms did not deviate from their past levels of payment. Given that firms are not all paying the same amount, this task would require substantial time and effort on the part of the tax authority, whereas that effort might be better invested in higher-return activities. Anecdotally, officials from the tax authority report the existence of no such extra-legal policy forcing firms to pay particular amounts. Most enforcement is concentrated on large and medium firms, which generate 80 to 90% of total tax revenues (RRA Report 2016).

Nor does the tax authority appear to audit or impose softer forms of enforcement like visits from tax officials on firms by their observed propensity to target. Figure 9 shows that whether a firm reports being audited or visited by tax officials is uncorrelated with targeting.

**Actual Enforcement:** A change in the structure of enforcement simultaneous to the reform could motivate taxpayers to maintain or increase tax payment. If, for example, the tax authority began heavily auditing taxpayers declaring taxable income in the range of 0 and 1,999,000 RwF or those paying zero at the same time as the reform, it could be rational for taxpayers to relocate to brackets where the enforcement likelihood is lower. I lack data on audits pre-reform, but post-reform there do not appear to be any discontinuities in the number or likelihood of audits for taxpayers in the sample range. Appendix Figure 8 shows that audit counts and audit likelihoods are extremely low in general (less than 1%) for all levels of taxable income and tax paid among taxpayers in the sample. There are more audits of taxpayers who pay zero, which might provide a reason for staying out of the exemption range, but this count belies a low actual probability for those paying zero, as there is a large number of taxpayers paying zero to match the higher number of audits. Appendix Figure 8(d) shows that the audit likelihood is not differentially higher for taxpayers paying zero.

Softer forms of enforcement also do not vary discontinuously with firm size: Appendix Figure 9 shows that visits by tax officials — to provide reminders or do spot checks of inventory or accounts — as reported in the taxpayer survey are smooth in taxable income. Nor does risk aversion: Appendix Figure 10 shows no significant differences across firm size for a measure of owner risk aversion collected in the taxpayer survey.47

**Perceptions of Enforcement:** Though the tax authority appears to implement uniform, extremely low levels of enforcement for firms within the sample, taxpayers may have perceptions that differ from reality. Recent evidence from a developing country shows that taxpayers...

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47Risk aversion is elicited through a series of questions asking respondents (firm owners) to choose between investment opportunities yielding either (1) certain payoffs of lower value or (2) higher payoffs that have a 50% chance of realization but a 50% chance of yielding zero returns. The measure in Appendix Figure 10 reports the average of this measure across payoff comparisons below the version that provides equivalent payoffs.
ers incorrectly perceive audit likelihoods and that these misperceptions affect tax behavior
(Bergolo et al. 2017; Bergolo et al. 2018). To address this possibility, the survey asks taxpayers
whether their perceived probability of being audited varies across hypothetical amounts of
declarations or taxes paid. Appendix Figure 11 shows that there are no significant differences
across amounts or by actual taxable income declared, implying that the perceived audit like-
lihood is high but inelastic to declaration and payment behavior. Nor do these perceived
probabilities vary with the size of the respondent firm.

Firms do appear to overestimate the true likelihood of audit substantially. In reality, the
actual audit likelihood for small firms is less than 1%, with no discontinuities in imposition
(Appendix Figure 8), implying that the average reported audit likelihood in the survey of
48% overestimates the true probability by at least 50 times. Part of this explanation may
derive from the survey elicitation procedure, as respondents may not have understood how
to determine percentage probabilities. However, comparing a categorical variable capturing
perceived audit likelihood, 46% of firms say that is somewhat likely that they will be audited
this year, while 37% say that it is very likely or absolutely certain. The correlation between
the categorical scale and the percentage measure is 0.61.

Finally, Section 7 discusses the results of an information experiment priming firms with
information about audit likelihoods (that they are high or low) and shows no effects on fil-
ings.

5.5.2 Taxpayer Mistakes

Next, I assess whether the reform’s impacts could be consistent with taxpayers making mis-
takes in responding to changes in liability. Linear firms on average return to pre-reform levels
of liability despite receiving a cut in their tax rate. Lump sum firms bunch post-reform on the
higher liability side of the new tax thresholds. Were firms simply not aware of the tax change,
or did they misunderstand how it changed the tax schedule? I assess whether evidence from
the survey and patterns in filing over time aligns with this interpretation.

First, I show that firms appear to understand the structure of the tax schedule relevant
to their regime. Table 3 shows that 63% of respondents correctly describe their own regime.
Knowledge about other regimes — the lump sum schedule in particular — is lower, yet the
share of lump sum firms aware that it is possible to legally pay zero taxes within their regime
(34%) exceeds the share of lump sum firms that locate within the exemption range (approxi-
mately 3%), suggesting that not knowing about the exemption is not the salient barrier keep-
ing firms from taking advantage of it.

Second, the persistence of responses runs counter to the interpretation that taxpayer mis-

48 Neither do discontinuities in risk aversion account for differences across income and tax paid (Appendix
Figure 10).
takes could alone explain the reform’s impacts. Firms relocate to on average higher liability points immediately post-reform and remain there six years afterwards. If the average response is mistaken, the effect on tax paid among lump sum taxpayers should weakly decrease over time as at least a small subset of firms learn and re-optimize in subsequent years. I observe that essentially none of the taxpayers that relocate after the reform to the positive liability side of the exemption range move back down to the exemption range in following years, nor do linear taxpayers end up decreasing tax payments in later periods. In the pooled sample, Figure 2(b) shows, if anything, the effect on tax paid in the balanced panel is increasing in the years following the reform.

5.5.3 Tax Morale

Finally, I consider whether tax morale could drive targeting. Firms may seek to remit a fixed amount to the government each year as a civic duty (Luttmer and Singhal 2013). This behavior could be consistent with a model of reference dependence on a “warm glow” produced by specific levels of taxes, and would imply that entrepreneurs derive positive utility from tax payment (Andreoni 1990). When taxes decline, firms target previous liability to return to their reference contribution.

There are two pieces of evidence to contradict this explanation. First, absent a universal norm in the level of tax payment as civic contribution, taxpayers should exhibit heterogeneity in the amount they pay across variation in the quality of public goods and services they receive, or in their views of the state. Figure 9 shows that firms in the survey that report better access to public goods and services are not more likely to exhibit targeting. Nor does an index of government trust correlate. Instead, measures related to the sophistication of taxpayers — the education of the owner, whether they pay other taxes like value-added, and whether they employ an accountant — are more strongly correlated with targeting. Second, in the survey, 37% of surveyed taxpayers describe taxes as being too high, and 34% describe them as being unfair. The fact that this proportion is greater than the proportion of taxpayers who do not increase liability in response to the reform implies that tax morale cannot account for targeting responses among all taxpayers.

49I also rule out overlapping discontinuities in non-tax incentives like government subsidies that might motivate taxpayers to pay a minimum amount in taxes. There appear to be no other public goods — like access to subsidies or other tax exemptions — that correspond to the income tax exemption threshold discontinuity, nor are there private incentives to pay positive tax — e.g., a tax clearance certificate, which is required for obtaining some forms of bank loans, is available to exempted taxpayers as long as they file a return.
6 Distributional Implications

In this section, I map responses to the reform to their distributional implications. I first describe the reform’s average effect on evasion and overpayment and then examine implications for progressivity by measuring how the effect varies across firm type.

6.1 Are Firms Worse Off?

Firms increase income tax paid on average, and this response appears driven by changes in reporting rather than real growth in production. If firms were previously evading, the effects of the reform could simply represent a reduction in underreporting, which may be a positive outcome from the perspective of the revenue authority. However, I instead find that a substantial share of firms post-reform are overreporting taxable income and overpaying in income taxes relative to their true size and liability.

Figure 11 presents the distributions of evasion in taxable income and tax paid. Evasion is measured by comparing taxable income declared to the tax authority and actual tax paid relative to the same values reported by the firm in the survey, one to three months in advance of making declarations for FY2017. For taxable income, this value is defined as (Self-Reported – Declared) / Declared, where Self-Reported is the survey-reported measure of turnover; for tax paid, it is (Counterfactual – Actual) / Actual, where Counterfactual uses the Self-Reported measure of turnover and a firm’s administratively-coded regime to calculate true liability. Self-reported taxable income for FY2017 was collected directly from firms between January and March 2018 — after the tax period had closed but before declarations were due on April 3, 2018, for FY2017. I Winsorize values at the top and bottom 10% to account for outliers.

The median levels of evasion are –10% for taxable income and 0% for income tax paid. Fifty-six percent of firms are overreporting taxable income and 46% are overpaying relative to true liability; 6% and 11% report zero evasion, while 38% and 43% report positive evasion, respectively. Mean evasion is slightly positive — 18% for taxable income and 33% for tax paid — however, this is partly driven by the inclusion of taxpayers close to zero, for whom the measure of evasion is blown up in a mechanical way, magnifying positive values.

Estimating the Proportion of Overpayers: The fact that a large proportion of firms in the survey is negatively evading is striking. However, as the survey sampled equally from above and below the exemption threshold, it is not possible to directly compare the distributions in Figure 11 to the true population of taxpayers to estimate what share of taxpayers are overpaying. To do so requires an estimate of the counterfactual taxable income distribution in absence of the reform.

I overcome this limitation by simulating the growth in year-to-year income and comparing it to the observed taxable income distribution. Appendix Figure 13 shows that the mass
of taxpayers shifts upward immediately post-reform, concentrating just above the exemption range — by 2017, 35% of all taxpayers are located between 2 and 4 million RwF, compared to 10% in 2011. To measure the share of this response that is attributable to natural income growth, I take the share of filers declaring less than 2 million RwF in 2015 and observe their reported taxable income in 2016. Only 7% of this group declare within the range of 2 to 4 million RwF in 2016. This implies an excess mass of 28% in the interval above the exemption range, where taxes are higher. Figure 11 shows that a random sample of taxpayers within this range are on average overdeclaring and overpaying relative to true liability. This implies that approximately 30% of taxpayers are led to overpay as result of the reform.

6.1.1 Is It Rational to Pay More?

Taxpayers may think the tax schedule too complex that they are willing to avoid active pursuit of information that would allow them to improve decisions. First, the adjustment costs of switching regimes may be perceived to be larger than the reduced tax burden. In actuality, adjustment costs should involve at most approximately one day of foregone earnings — to visit the revenue authority and switch regimes, which is less than the potential savings in tax liability. Additionally, the amount of tax liability is substantial — 18% of profits, on average, for those at 2 million RwF — which suggests that the cost is large enough to outweigh even a much higher perceived adjustment or cognitive costs. Blumenstock et al. (2018) do find evidence of cognitive cost influencing decisions in a default savings experiment in Afghanistan; however, in the Rwandan tax setting, that taxpayers lose money rather than simply fail to shift it to a different location implies that the cognitive cost would need to be very high to justify the observed behavior.

Alternatively, it may be possible that the costs of tracking revenues more precisely — to better estimate changes in taxable income year to year, or produce proof of earnings in the event of audit — exceed the benefits in terms of taxes saved. However, there exist low-cost methods of preparing records and obtaining help with declarations. Accountants and tax advisors that serve small firms around tax time charge approximately 20,000 to 30,000 RwF for their services, which is only 30 to 50% of the savings a lump sum firm could obtain by securing help in obtaining an exemption from income taxes or reduced liability.

6.2 Implications for Progressivity

I next examine how the reform’s impacts on evasion and overpayment vary across taxpayer type. I show first that responses induced by the reform lead to variation in the impacts of the policy across taxpayers, leading those who are less educated and less profitable to be worse off than their counterparts. How the response to the reform varies across taxpayer characteristics, and the share of taxpayers falling into each group, relates to the fiscal and welfare
impacts of the change in liability. Additionally, evidence on how taxpayer type influences responses can inform the welfare implications of policies of this type implemented in similar contexts.

The comparison of interest is between lump sum and linear taxpayers, as assignment to regime is associated with distinct (average) responses in tax paid. Linear taxpayers receive a uniform tax reduction and on average do not change the amount of taxes they pay. Lump sum taxpayers receive a tax reduction that varies depending on their location in the pre-reform taxable income distribution, but on average increase tax paid by 100% in response to the reform. Given that firms target previous liability, how assignment of regime sorts taxpayers links directly to the distributional implications of the reform itself.

Using the survey data, I show that more vulnerable taxpayers were assigned to the lump sum regime. I compare linear to lump sum taxpayers in 2017. Figure 12 shows that taxpayers assigned to the lump sum regime are operated by less educated owners and, controlling for earnings, are less profitable firms. They are also less knowledgeable, older, and more likely female.

Figure 13 shows that lump sum taxpayers evade much less taxable income than linear regime taxpayers (Panel A), and they are more likely to overpay in taxes (Panel B). The magnitude of overpayment is not small: the average size of the additional tax liability for taxpayers in the survey amounts to 18% of annual profits, where the average amount of profits for these firms is approximately USD $1,000 or USD $2.8 per day.

Less educated and less profitable taxpayers are more likely to overpay relative to their true tax liability and to counterparts of the same size. This regressivity stems from the assignment of these taxpayers to the lump sum regime, which, given firms’ tax behavior, generates a larger increase in tax paid. This result is opposite to the explicit intentions of the reform: to simplify the tax burden and enhance the progressivity of the tax schedule by exempting the poorest firms from tax obligations, and implies that firms’ behavior can lead traditional tax incentives to backfire.

**Robustness to Regime Sorting by Ability to Evade:** If linear firms possess a greater ease of evasion — through greater knowledge of enforcement, access to evasion technologies, or general savvy — my results would overestimate the role of the reform in generating differences in evasion. To address this concern, I compare correlates of regime assignment in 2011, the year before the reform. Conditional on income, linear firms are significantly more likely to pay value-added and payroll taxes and to pay withholding taxes on public tenders or imports. These characteristics run counter to the interpretation that linear firms have a greater ease of evasion — being subject to other forms of taxation and withholding should reduce evasion.

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50The differences in evasion are not causal evidence that the reform is evasion-reducing: instead it could be the case that the reform “sorts” firms who are more and less likely to evade to positions below and above the exemption threshold, respectively.
the ability of these firms to evade income tax obligations as they provide a paper trail of revenues. It is true that less than 3% of firms within this range pay value-added or payroll taxes, so this explanation may not account for average behavior. However, the difference in estimated evasion across regimes is large enough that a corresponding difference in ease of evasion would need to be significant to produce the observed results. To assess whether inflating self-reported income changes the results, I next measure their robustness to underestimates of self-reports.

**Robustness to Underestimates of Self-Reports:** One potential concern is that, given uncertainty in estimating own earnings, survey reports of turnover may be inaccurate. I address this concern by considering the possibility that respondent taxpayers underestimate self-reported turnover for FY2017. I inflate self-reported estimates by 10 to 30% before comparing to declared turnover and calculating counterfactual liability⁵¹. Appendix Figure 14 shows the results of this analysis for the evasion comparison across regimes. Lump sum taxpayers are still, on average, negatively evading liability at all levels of inflated self-reports, and the difference between linear and lump sum taxpayers remains significant. Though the significance of the negative result for lump sum taxpayers disappears at values inflated to 20%, the point estimate remains negative. If all surveyed taxpayers or lump sum taxpayers in particular systematically underestimate their turnover in the survey responses, this evidence shows that underestimating own earnings cannot account for the difference in levels of evasion between linear and lump sum taxpayers, and that lump sum taxpayers are still weakly overpaying relative to true liability.

7 Experimental Evidence on Informational Nudges

The response to the reform imply that targeting behavior leads firms to on average increase the amount of taxes they pay, and, in particular, makes more vulnerable entrepreneurs worse off. Information interventions offer a potential method for improving tax choices — informing firms about better options or providing ways for entrepreneurs to more accurately estimate their own earnings.

To assess if such inventions hold potential value, I experimentally test whether firms’ tax choices are responsive to new information. This section describes a randomized information experiment I implement priming firms with information intended to affect knowledge of own earnings — by sharing the declaration choices of similar firms — or perceptions of enforcement — by providing facts about audit likelihoods. I assess responses along two sets of outcomes: (1) in a survey experiment design, I elicit pre-treatment and post-treatment measures of perceived audit likelihood and a firm’s intended amount of declared taxable income

⁵¹ de Mel et al. (2009) estimate that Sri Lankan firms underreport revenues by approximately 30%.
for fiscal year 2017; and (2) from administrative declarations, actual taxable income declared and tax paid for fiscal year 2017.

7.1 Experimental Design

The experimental sample is identical to the survey sample. Firms eligible for selection must have declared less than 4 million RwF (USD $4,600) in annual turnover in 2016 in the turnover base (linear or lump sum regime). Sampling was stratified to select half of firms from below the exemption threshold at 2 million RwF and half from above.

The experiment was implemented between January and March 2018, just before the declaration deadline for 2017 declarations on April 3, 2018. Participants were provided with the treatment information over the phone by enumerators in the course of the survey and received follow-up text messages with the same information two weeks before the declaration deadline.

The information treatment groups are as follows:

0. **Control**: Firms in this group receive a message with reminders of the tax declaration deadline.

1. **Audit Low**: Firms in this group receive a message that the likelihood of audit or review by the tax authority for taxpayers like them is very small.

2. **Audit High**: Firms in this group receive a message that the likelihood of audit or review by the tax authority for taxpayers like them is very high.

3. **Peers Below**: Firms in this group receive a message that 50% of taxpayers like them declared less than 2 million RwF but more than 0 RwF in turnover in their income tax declarations for the 2016 fiscal year.

4. **Peers Above**: Firms in this group receive a message that 50% of taxpayers like them declared exactly 2 million RwF in turnover in their income tax declarations for the 2016 fiscal year.

The audit treatment were intended to shift taxpayers perceptions of enforcement up or down. True information about audit counts was presented in ways to suggest that the audit likelihood for the respondent firm was high or low. The peer information treatments were intended to provide firms with information about taxpayers like them, which could permit updating about a respondent firm’s own earnings (given what firms like them are declaring) or

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52 This message was intended as a placebo, with information orthogonal to the content of declarations (firms were only told it was important to declare on time). Firms in treatment message groups also receive an identical reminder of the tax declaration deadline.

53 This treatment communicates that these taxpayers were paying a positive amount in taxes as this is where liability become positive in the lump sum regime.
about their location in the taxable income distribution relative to similar firms (which could shape audit perceptions depending on where a given firm is located relative to the mass of firms, as communicated by the message).

Treatments are mutually exclusive, so that a firm only received one type of message\textsuperscript{54}. Phone conversations were conducted in almost all cases in Kinyarwanda, the most commonly spoken language in Rwanda; text messages, also in Kinyarwanda, were sent in late March, just before the declaration deadline. Appendix Table 1 shows experimental balance. Appendix A provides more detail on the sampling procedure, experimental design, and treatment messages.

7.2 Estimating Equation

I assess the impacts of the information experiment on survey outcomes and actual declaration behavior using the following approach:

\[
y_i = \alpha + \beta_1 \text{AuditLow}_i + \beta_2 \text{AuditHigh}_i + \beta_3 \text{PeersBelow}_i + \beta_4 \text{PeersAbove}_i + X_i \theta + \varepsilon_i \tag{6}\]

where $y_i$ is the outcome of interest, treatment variables are indicators for belonging to a particular treatment arm, and $X_i$ are baseline controls, including taxable income declared in 2016 and outcome-specific baseline controls.

7.3 Results

Table 5 shows the results of the survey experiment on intended declared taxable income for fiscal year 2017 and perceived likelihood of experiencing an audit on those same declarations. There are no impacts on intended taxable income: only 19\% of respondents change the amount they provide (compared to the amount solicited at baseline), and these switchers and the direction in which they update appears uncorrelated with the treatments. This could reflect actual insensitivity of declarations to new information or the cognitive cost of updating the amount of taxable income one intends to declare in the time provided by the survey. Participants are responsive, however, in their perceived audit likelihoods: though only 15\% of participants update their answers post-treatment (relative to pre-treatment measures), the Peers Above treatment causes respondents to increase reported audit likelihood, while the Audit High treatment leads respondents to be both more likely to increase and more likely to decrease perceived audit likelihood, suggesting heterogeneity in responsiveness. However, the only treatment arm that causes respondents to update negatively the level of perceived

\textsuperscript{54}At the beginning of the study, some firms in the peer treatment groups also received the Control message. Controlling for this fact does not affect the results.
audit likelihood is Peers Below, reducing the perceived audit likelihood by 6%. These results suggest that though the average respondent exhibits inertia in audit perceptions over the course of the survey experiment, some taxpayers are responsive to information communicating that many firms like them are declaring low levels of turnover.

Table 6 shows the outcomes drawn from administrative declarations. I estimate effects on levels rather than logs as there is a substantial number of zero values. No differences appear in amount of taxable income declared, though those in the Peers Below treatment are significantly more likely to declare the same amount as in previous years. In income tax payable, taxpayers in the Peers Below treatment pay significantly less in income taxes compared to the control group, while no other treatment generates significant differences in tax paid. This effect is generated by an increase in the control group, suggesting that the treatment did not lead firms to decrease tax paid, but caused them to be less likely to increase tax paid according to natural income processes operating in absence of the information intervention, as measured by control group behavior. It is important to note that these effects are sensitive to Winsorization — they still appear Winsorizing values at the top 99%, but disappear thereafter, suggesting that the behavior of the largest firms is driving the average effect.

The audit treatments go in the expected directions: those in the Audit Low treatment are less likely to increase (or decrease) tax paid and more likely to pay the same amount as in 2016; those in the Audit High treatment are marginally significantly more likely to increase tax paid and less likely to pay the same amount — however, neither of the audit treatments impact the level of tax paid relative to the control group.

**Heterogeneity by Firm Size:** To understand the composition of the impact of treatments by heterogeneity in taxable income, I interact the treatment arms with 1 million RwF increments of 2016 declared taxable income, using the following specification:

\[
y_i = \alpha + \beta_1 \text{AuditLow}_i + \beta_2 \text{AuditHigh}_i + \beta_3 \text{PeersBelow}_i + \beta_4 \text{PeersAbove}_i \\
+ \beta_5 \text{AuditLow}_i \times y_{i,2016}^D + \beta_6 \text{AuditHigh}_i \times y_{i,2016}^D + \beta_7 \text{PeersBelow}_i \times y_{i,2016}^D \\
+ \beta_8 \text{PeersAbove}_i \times y_{i,2016}^D + X_i \theta + \varepsilon_i
\]  

(7)

where \( \text{Treatment} \times y_{i,2016}^D \) is a term interacting a treatment arm with a taxable income declared by firm \( i \) in 2016. Table 6, Panel B, shows that the negative effect on the Peers Below treatment is increasing in firms size, suggesting that the farther removed firms are from the range of taxable income in which they are told many firms are declaring, the larger the negative effect on tax paid.
7.4 Discussion

These findings show that firms are responsive to peer information. Evidence from filings suggests that the influences of peers extends to the selection of liability targets themselves. Appendix Figure 16 shows that new entrants to the linear tax base post-reform are more likely to bunch at 2 million RwF — a point at which many lump sum taxpayers bunch but meaningless to linear regime tax liability — when a greater proportion of local peers are declaring within the lump sum schedule.

The results of the experiment suggest that peers influence how firms decide what to pay, and that part of this decision involves assessing the enforcement likelihood at different payment amounts. The Peers Below treatment produces a negative effect on tax paid and leads firms to update negatively regarding their perceived probability of audit. However, I am unable to distinguish between a channel of reducing uncertainty in the estimation of own earnings — firms updating about the size of their own taxable income after learning what firms like them are declaring — or of reducing perceived enforcement — firms updating on the likelihood they will be audited given that others are declaring less.

Taken together, the evidence discussed in Section 5 on the motivations behind targeting, the predominant mode of compliance, implies that uncertainty in estimating own earnings drives this behavior. The evidence also suggests, however, that targeting is inseparable from perceptions of enforcement. Taxpayers appear to err on the side of paying more, potentially believing the cost worth being safer from audits or investigations. If targets represent “safe spots” with respect to enforcement, entrepreneurs appear to believe that deviating downward risks punishment, making a degree of fear inherent in targeting. The experimental results appear to confirm this fact — changes in tax paid in response to peer information align with changes in perceptions of audit probability.

8 Conclusion

This paper demonstrates that the tax behavior of firms in a low income country diverges from what standard models of compliance would predict. I show that instead of responding to fluctuations in income and changes in liability on the margin, firms appear to navigate tax obligations by consistently targeting a specific amount of liability year after year. This behavior generates perverse responses to familiar tax instruments, leading firms to overpay relative to true liability and making more vulnerable entrepreneurs worse off. These effects are non-trivial for firms: in my context, tax payments represent almost one-fifth of annual profits. Together, my findings imply that familiar tax instruments can backfire when the tax behavior of firms is not taken into account.

These findings relate directly to recent efforts to simplify taxation in developing countries.
Reforms of the type I consider are increasing in prevalence and typically affect large shares of tax bases. As developing country governments seek to craft tax policy to generate much-needed revenues, understanding how firms respond and the impacts of design choices on production and welfare are essential to designing tax systems that can be sustainable in the long-run. Though I document responses in the context of taxation, it is plausible that my findings could also apply to other policy questions in low income country settings, including how poor individuals navigate government services like social welfare programs, subsidies, and private credit markets.

A promising avenue for future research lies in a more thorough mapping of firms’ tax behavior to understand its implications for tax instruments, and in identifying alternative approaches to taxation that could align behavioral responses more closely with the objectives of tax policy.
References


Figure 1: Income Tax Structure Before and After Reform

Notes: This figure displays tax liability by taxable income for taxpayers in the turnover base before and after the 2012 income tax reform described in Section 2.2. The red line with dashes and dots shows the 4 percent “linear tax” in place between the 2005 and 2011 fiscal years; the blue dashed and solid lines show the present tax structure, consisting of a 3 percent “linear tax” (dashed line) and a lump sum “flat tax” (solid line) under which taxpayers pay zero taxes if they declare 1,999,000 RwF (USD $2,324) or less in annual turnover and 60,000 RwF (USD $69) if they declare 2 million RwF or more. Not displayed is the profit regime which levies a 30 percent marginal tax rate on profits. The structure of liabilities under the profit regime has remained unchanged since 2005. In practice, less than 5 percent of firms reporting less than 4 million RwF (USD $4,648) in annual turnover opt for the profit base.
Figure 2: Event Study Estimates — Taxable Income and Income Tax Payable

(a) Taxable Income

(b) Income Tax Payable

Notes: This figure displays coefficients from equation (1) estimated on a balanced panel of firms, regressing (a) taxable income declared and (b) income tax payable on year-dummies. Both regressions include firm fixed effects and cluster standard errors at the firm-level. The sample consists of firms that declared less than 4 million RwF in taxable income in 2011. Vertical bars represent 95 percent confidence intervals for the coefficient estimates. Vertical dashed line indicates the timing of the tax reform.
Figure 3: Bunching on Previous Year’s Tax Liability (Excluding Reform Year)

(a) Tax Payable

(b) Taxable Income

Notes: This figure displays bunching pooling all years except 2012 (first year post-reform) on previous year’s tax liability and taxable income across 2.5% bins. Observations where previous year’s tax liability is zero are dropped.
Figure 4: Pre-Reform Heterogeneity in Bunching on Previous Year’s Tax Liability

(a) Firm Size (2011 Taxable Income)

(b) Firm Sector (2011)

(c) Roundness of Taxable Income (2011)

(d) Roundness of Tax Payable (2011)

Notes: This figure displays bunching pooling all years between 2008 and 2011 on previous year’s tax liability and taxable income across 2.5% bins. Observations where previous year’s tax liability is zero are dropped. Figure (a) shows bunching across firm size as measured by reported taxable income in 2011. Figure (b) shows bunching based on firm sector in 2011. Figures (c) and (d) show bunching on whether taxable income in 2011 matches to a “round” 100,000 RwF value or tax payable in 2011 matches a “round” 10,000 RwF value.
Figure 5: Predicted Targeting Response by Regime Assignment

(a) Linear Regime

(b) Lump Sum Regime

Notes: This figure displays the predicted targeting response of taxpayers in the (a) linear and (b) lump sum regimes as described in Section 5.
Figure 6: Event Study Estimates — Targeting by Regime Assignment

(a) Linear Regime (Pooling Across Pre-Reform Taxable Income Levels)

(b) Lump Sum Regime (Splitting by Pre-Reform Taxable Income Levels)

Notes: This figure displays coefficients from equation (4) estimated on a balanced panel of firms, regressing an indicator for tax paid in the present year matching tax paid in the previous year on present-year-dummies. Both regressions include firm fixed effects and cluster standard errors at the firm-level. The sample is comprised of firms that declared less than 4 million RwF in taxable income in 2011. Vertical bars represent 95 percent confidence intervals for the coefficient estimates. Figure (a) displays firms assigned to the linear regime post-reform, pooling across pre-reform taxable income levels (as the size of the tax reduction does not vary with firm size in this regime). Figure (b) displays firms assigned to the lump sum regime post-reform, splitting by pre-reform taxable income levels (as the nature of the tax reduction varies with firm size in this regime).
FIGURE 7: TAXABLE INCOME DISTRIBUTION PRE- AND POST-REFORM — LUMP SUM REGIME

Notes: This figure displays the taxable income distribution for lump sum taxpayers before and after the reform for taxpayers in the balanced panel. The figure plots the number of taxpayers declaring within 50,000 RwF bins. The diamonds represent pre-reform values, the circles post-reform values.
**Figure 8: Bunching on Previous Year’s Tax Liability (Reform Year and Post-Reform)**

(a) Reform Year

![Graph showing bunching in reform year](image)

(b) Post-Reform

![Graph showing post-reform tax liability](image)

Notes: These figures display bunching in the year of the reform and pooling across all post-reform years on previous year’s tax liability (by regime assignment in the post-period) across 1% bins. Observations where previous year’s tax liability is zero are dropped.
Notes: This figure displays correlates of targeting by administrative variables and variables drawn from the matched survey. Correlations are estimated by regressing an outcome of targeting on the variable of interest, clustering by firm in the administrative sample where multiple years enter the regression. The label for negative correlations contains an (-), the label for positive correlations contains an (+). The labels on the far-left y-axis display the categories of the variables displayed. The categories with the strongest correlations are highlighted.
FIGURE 10: CHANGES IN TAX PAYMENT AMONG LUMP SUM FIRMS — 2 TO 4 MILLION RWF RANGE (2011)

(a) Ratio of Tax Paid 2012 vs. 2011 (Winsorized 5%)

(b) Increased Tax Paid from 2011 to 2012

Notes: This figure displays (a) changes in tax payment and (b) a dummy for increases in tax payment, comparing taxes paid in 2012 (first year after the reform) to 2011 (year before the reform) for firms declaring between 2 and 4 million RwF in 2011 and moving to the lump sum regime in 2012. The bins group the mean value of the outcome for firms within small bandwidths. The lines estimate linear fits on either side of the threshold indicated by the vertical dashed line at 3 million RwF, the point at which previous liability matches new liability in the higher threshold (above 4 million RwF) for the firms in the sample.
Figure 11: Distribution of Reported Evasion — FY2017

(a) Taxable Income

(b) Income Tax Paid

Notes: Figure (a) displays the distribution of percentage of taxable income evaded — defined as (Self-Reported – Declared) / Declared for 2017 — among taxpayers in the survey sample for taxpayers declare at or below the exemption threshold. Figure (b) displays distribution of percentage of income tax liability evaded — defined as (Counterfactual – Actual) / Actual for 2017 — among taxpayers in the survey sample. Self-reported 2017 taxable income was elicited in surveys between January and March 2018; declared 2017 taxable income and actual 2017 tax liability were collected from income tax declarations submitted to the Rwanda Revenue Authority by April 3, 2018; counterfactual 2017 tax liability is imputed as the statutory tax liability given self-reported turnover. Values are Winsorized for the top and bottom 10 percent on the full sample. The sample is all surveyed firms.
Figure 12: Measures of Vulnerability by Regime Choice

(a) Graduated Secondary School

(b) Profits FY2017

Notes: These figures display the mean value and confidence intervals by regime assignment of survey measures of (a) whether the owner graduated secondary school and (b) profits in FY2017 (self-reported). Vertical bars represent 95 percent confidence intervals for the mean estimate by income bin. The sample is all surveyed firms.
Notes: Figure (a) displays the mean value by regime assignment of taxable income evaded — defined as (Self-Reported – Declared) / Declared for 2017 — among taxpayers in the survey sample. Figure (b) displays the mean value across income bins of percentage of income tax liability evaded — defined as (Counterfactual – Actual) / Actual for 2017 — among taxpayers in the survey sample. Self-reported 2017 taxable income (turnover) was elicited in surveys between January and March 2018; declared 2017 taxable income and actual 2017 tax liability were collected from income tax declarations submitted to the Rwanda Revenue Authority by April 3, 2018; counterfactual 2017 tax liability is imputed as the statutory tax liability given self-reported turnover. Vertical bars represent 95 percent confidence intervals for the mean estimate by income bin. Values are Winsorized for the top and bottom 10 percent on the full sample. The sample is all surveyed firms.
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Turnover</td>
<td>1,319,722</td>
<td>1,239,041</td>
<td>1,082,763</td>
</tr>
<tr>
<td></td>
<td>(943,028)</td>
<td>(822,336)</td>
<td>(826,069)</td>
</tr>
<tr>
<td>Income Tax Paid</td>
<td>52,509</td>
<td>49,583</td>
<td>43,254</td>
</tr>
<tr>
<td></td>
<td>(37,478)</td>
<td>(32,953)</td>
<td>(32,964)</td>
</tr>
<tr>
<td>Value-Added Taxpayer</td>
<td>0.03</td>
<td>0.01</td>
<td>0.04</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
<td>(0.11)</td>
<td>(0.18)</td>
</tr>
<tr>
<td>Payroll Taxpayer</td>
<td>0.03</td>
<td>0.01</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
<td>(0.10)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Wholesale</td>
<td>0.67</td>
<td>0.11</td>
<td>0.67</td>
</tr>
<tr>
<td></td>
<td>(0.47)</td>
<td>(0.32)</td>
<td>(0.47)</td>
</tr>
<tr>
<td>Services</td>
<td>0.15</td>
<td>0.24</td>
<td>0.17</td>
</tr>
<tr>
<td></td>
<td>(0.37)</td>
<td>(0.34)</td>
<td>(0.38)</td>
</tr>
<tr>
<td>Construction</td>
<td>0.01</td>
<td>0.01</td>
<td>0.02</td>
</tr>
<tr>
<td></td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Kigali-Located</td>
<td>0.84</td>
<td>0.45</td>
<td>0.44</td>
</tr>
<tr>
<td></td>
<td>(0.37)</td>
<td>(0.50)</td>
<td>(0.50)</td>
</tr>
<tr>
<td>Observations</td>
<td>354</td>
<td>11,019</td>
<td>7,858</td>
</tr>
</tbody>
</table>

Notes: This table displays taxpayer characteristics drawn from the administrative data in 2011. Column (1) shows summary statistics for the balanced panel of firms described in Section 3.1. Columns (2) and (3) show summary statistics for firms assigned in 2012 to the linear or lump sum regimes, respectively, and correspond to the sample used in heterogeneity analysis described in Section 3.1. Standard deviations are reported in parentheses. Turnover is reported taxable income and income tax paid the amount remitted to the tax authority, in 2011 RwF. Value-added and payroll taxpayer status are indicators for paying these taxes in 2011. Wholesale, services, and construction are indicators for declaring one of these sectors at registration. Kigali-located is an indicator for declaring a location within the capital city regime at registration.
### Table 2: Event Study Estimates — Taxable Income and Income Tax Paid

<table>
<thead>
<tr>
<th>Panel A: Pooled Regimes</th>
<th>Log Taxable Income</th>
<th>Log Income Tax Payable</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\frac{\Delta D}{\Delta D_{2011}} &lt; 4m$</td>
<td>$\frac{\Delta D}{\Delta D_{2011}} &lt; 2m$</td>
</tr>
<tr>
<td>Post 2012</td>
<td>1.026***</td>
<td>1.101***</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>Firm clusters</td>
<td>14,736</td>
<td>12,705</td>
</tr>
<tr>
<td>Observations</td>
<td>57,188</td>
<td>48,012</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Flat Tax (Lump Sum) Regime</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post 2012</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Firm clusters</td>
</tr>
<tr>
<td>Observations</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel C: Linear (Marginal-Rated) Regime</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post 2012</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Firm clusters</td>
</tr>
<tr>
<td>Observations</td>
</tr>
</tbody>
</table>

Notes: This table displays event study estimates from equation (2) split by pre-reform (2011) taxable income and regime election post-reform. Post-2012 captures the impact of the change in tax structure on each group of firm. The sample contains firms that appear at least twice in the pre-period and at least twice in the post-period. All regressions includes firm and year fixed effects and cluster standard errors at the firm level.
<table>
<thead>
<tr>
<th>Administrative Regime in 2016</th>
<th>Pooled (1)</th>
<th>Linear (2)</th>
<th>Lump Sum (3)</th>
<th>Difference (4)</th>
<th>p-value (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Easy to declare</td>
<td>0.19</td>
<td>0.20</td>
<td>0.17</td>
<td>0.03</td>
<td>0.16</td>
</tr>
<tr>
<td>Easy to pay</td>
<td>0.22</td>
<td>0.25</td>
<td>0.19</td>
<td>0.06</td>
<td>0.02</td>
</tr>
<tr>
<td>Easier to keep records</td>
<td>0.03</td>
<td>0.03</td>
<td>0.03</td>
<td>0</td>
<td>0.92</td>
</tr>
<tr>
<td>Less oversight by RRA</td>
<td>0.07</td>
<td>0.06</td>
<td>0.09</td>
<td>-0.03</td>
<td>0.05</td>
</tr>
<tr>
<td>Other businesses do it</td>
<td>0.02</td>
<td>0.03</td>
<td>0.02</td>
<td>0.01</td>
<td>0.43</td>
</tr>
<tr>
<td>Auto-enrolled</td>
<td>0.61</td>
<td>0.57</td>
<td>0.65</td>
<td>-0.08</td>
<td>0.01</td>
</tr>
<tr>
<td>Don’t know</td>
<td>0.04</td>
<td>0.05</td>
<td>0.04</td>
<td>0.01</td>
<td>0.69</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Knowledge</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Don’t know regime</td>
<td>0.32</td>
<td>0.26</td>
<td>0.40</td>
<td>-0.14</td>
<td>0.00</td>
</tr>
<tr>
<td>Know both regimes</td>
<td>0.14</td>
<td>0.19</td>
<td>0.09</td>
<td>0.10</td>
<td>0.00</td>
</tr>
<tr>
<td>Own regime correct</td>
<td>0.63</td>
<td>0.70</td>
<td>0.55</td>
<td>0.14</td>
<td>0.00</td>
</tr>
<tr>
<td>Reported matches admin</td>
<td>0.38</td>
<td>0.24</td>
<td>0.54</td>
<td>-0.29</td>
<td>0.00</td>
</tr>
<tr>
<td>Know Flat Tax structure</td>
<td>0.20</td>
<td>0.23</td>
<td>0.17</td>
<td>0.05</td>
<td>0.03</td>
</tr>
<tr>
<td>Know Flat Tax may pay zero</td>
<td>0.32</td>
<td>0.30</td>
<td>0.34</td>
<td>-0.04</td>
<td>0.34</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel C: Declaration</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Keeps accounts</td>
<td>0.50</td>
<td>0.55</td>
<td>0.44</td>
<td>0.11</td>
<td>0.00</td>
</tr>
<tr>
<td>Accountant</td>
<td>0.16</td>
<td>0.22</td>
<td>0.08</td>
<td>0.14</td>
<td>0.00</td>
</tr>
<tr>
<td>Declare on phone</td>
<td>0.43</td>
<td>0.26</td>
<td>0.65</td>
<td>-0.39</td>
<td>0.00</td>
</tr>
<tr>
<td>Declare on computer</td>
<td>0.36</td>
<td>0.49</td>
<td>0.18</td>
<td>0.31</td>
<td>0.00</td>
</tr>
<tr>
<td>Declare at RRA</td>
<td>0.09</td>
<td>0.08</td>
<td>0.10</td>
<td>-0.02</td>
<td>0.37</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel D: Enforcement Experiences</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Audited/Reviewed in past</td>
<td>0.17</td>
<td>0.16</td>
<td>0.19</td>
<td>-0.03</td>
<td>0.28</td>
</tr>
<tr>
<td>RRA visited last 6 mon.</td>
<td>0.68</td>
<td>0.64</td>
<td>0.74</td>
<td>-0.1</td>
<td>0.00</td>
</tr>
<tr>
<td>Know punishments</td>
<td>0.75</td>
<td>0.76</td>
<td>0.73</td>
<td>0.02</td>
<td>0.43</td>
</tr>
<tr>
<td>Know peers punished</td>
<td>0.38</td>
<td>0.39</td>
<td>0.37</td>
<td>0.02</td>
<td>0.47</td>
</tr>
<tr>
<td>Punished in past</td>
<td>0.38</td>
<td>0.38</td>
<td>0.37</td>
<td>0.01</td>
<td>0.76</td>
</tr>
<tr>
<td>Punished for late pay</td>
<td>0.87</td>
<td>0.89</td>
<td>0.86</td>
<td>0.03</td>
<td>0.83</td>
</tr>
<tr>
<td>Punished for not paying</td>
<td>0.22</td>
<td>0.11</td>
<td>0.29</td>
<td>-0.17</td>
<td>0.34</td>
</tr>
<tr>
<td>Fined in past</td>
<td>0.96</td>
<td>0.95</td>
<td>0.97</td>
<td>-0.01</td>
<td>0.60</td>
</tr>
<tr>
<td>Fine amount</td>
<td>178,256</td>
<td>212,710</td>
<td>138,654</td>
<td>74,056</td>
<td>0.20</td>
</tr>
</tbody>
</table>

Notes: This table displays taxpayer characteristics collected in the taxpayer survey, comparing firms that declared less than 2 million RwF to those in the linear or lump sum regime in 2016. In Panel D, punished for late pay, punished for not paying, fined in past and fine amount are estimated conditional on reporting having been punished. The p-value is calculated through a t-test comparing the values across the two groups. RRA corresponds to the Rwanda Revenue Authority.
**Table 4: Bunching Estimates**

<table>
<thead>
<tr>
<th>Year</th>
<th>Pooled</th>
<th>Linear</th>
<th>Lump Sum</th>
<th>Pooled</th>
<th>Linear</th>
<th>Lump Sum</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>2009</td>
<td>34.4</td>
<td>5.1</td>
<td>30</td>
<td>34.9</td>
<td>5.3</td>
<td>30.2</td>
</tr>
<tr>
<td></td>
<td>(0.6)</td>
<td>(0.3)</td>
<td>(0.4)</td>
<td>(0.5)</td>
<td>(0.3)</td>
<td>(0.4)</td>
</tr>
<tr>
<td>2010</td>
<td>34</td>
<td>4.8</td>
<td>31.4</td>
<td>33.1</td>
<td>4.8</td>
<td>30.5</td>
</tr>
<tr>
<td></td>
<td>(0.6)</td>
<td>(0.5)</td>
<td>(0.8)</td>
<td>(1.2)</td>
<td>(0.5)</td>
<td>(0.8)</td>
</tr>
<tr>
<td>2011</td>
<td>68.5</td>
<td>17.7</td>
<td>58.7</td>
<td>67.9</td>
<td>17.7</td>
<td>58.1</td>
</tr>
<tr>
<td></td>
<td>(1.4)</td>
<td>(0.6)</td>
<td>(1.3)</td>
<td>(1.7)</td>
<td>(0.6)</td>
<td>(1.6)</td>
</tr>
<tr>
<td>2012</td>
<td>10.7</td>
<td>2</td>
<td>9.1</td>
<td>32.6</td>
<td>15.6</td>
<td>22.6</td>
</tr>
<tr>
<td></td>
<td>(2.6)</td>
<td>(1.5)</td>
<td>(1.6)</td>
<td>(1.3)</td>
<td>(1.7)</td>
<td>(6.2)</td>
</tr>
<tr>
<td>2013</td>
<td>297.3</td>
<td>29.5</td>
<td>289.8</td>
<td>965</td>
<td>43.2</td>
<td>1128.1</td>
</tr>
<tr>
<td></td>
<td>(4.2)</td>
<td>(3.5)</td>
<td>(1.9)</td>
<td>(12.9)</td>
<td>(2.3)</td>
<td>(42.5)</td>
</tr>
<tr>
<td>2014</td>
<td>285.3</td>
<td>45.7</td>
<td>262.2</td>
<td>805.2</td>
<td>57.6</td>
<td>685.8</td>
</tr>
<tr>
<td></td>
<td>(2.9)</td>
<td>(2.3)</td>
<td>(1.8)</td>
<td>(93.9)</td>
<td>(13.9)</td>
<td>(5.9)</td>
</tr>
<tr>
<td>2015</td>
<td>348.7</td>
<td>120.7</td>
<td>281.5</td>
<td>862.5</td>
<td>123.7</td>
<td>529.9</td>
</tr>
<tr>
<td></td>
<td>(7.9)</td>
<td>(7.4)</td>
<td>(4.9)</td>
<td>(30.1)</td>
<td>(26)</td>
<td>(35.5)</td>
</tr>
<tr>
<td>2016</td>
<td>508.8</td>
<td>352.2</td>
<td>306.7</td>
<td>661.4</td>
<td>228.3</td>
<td>541.5</td>
</tr>
<tr>
<td></td>
<td>(7.2)</td>
<td>(1.8)</td>
<td>(8.4)</td>
<td>(21.3)</td>
<td>(28.3)</td>
<td>(61.9)</td>
</tr>
<tr>
<td>2017</td>
<td>859.5</td>
<td>445.2</td>
<td>579.1</td>
<td>430.3</td>
<td>269.6</td>
<td>245.7</td>
</tr>
<tr>
<td></td>
<td>(8.8)</td>
<td>(1.1)</td>
<td>(10.9)</td>
<td>(25.4)</td>
<td>(37.5)</td>
<td>(37.5)</td>
</tr>
</tbody>
</table>

Notes: This table displays bunching estimates from the procedure outlined in Section 5.4.4 at the ratio of 1 on taxable income and tax payable by year and regime. Boostrapped standard errors are in parentheses.
### Table 5: Information Experiment Results — Survey Experiment Outcomes

<table>
<thead>
<tr>
<th>Panel A: Pooled Sample</th>
<th>Intended Taxable Income 2017 Compared to Baseline Level</th>
<th>Reported Audit Likelihood for 2017 Declarations Compared to Baseline Level</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Same (1) Increase (2) Decrease (3) Endline Level (4)</td>
<td>Same (5) Increase (6) Decrease (7) Endline Level (8)</td>
</tr>
<tr>
<td><strong>Audit Low</strong></td>
<td>0.018 0.030 -0.048 -264.024 (0.038) (0.024) (0.033) (172.238)</td>
<td>-0.055 0.043 0.012 0.459 (0.038) (0.027) (0.029) (1.374)</td>
</tr>
<tr>
<td><strong>Audit High</strong></td>
<td>-0.054 0.003 0.051 -256.149 (0.039) (0.024) (0.034) (174.716)</td>
<td>-0.138*** 0.060** 0.078** -0.570 (0.039) (0.028) (0.030) (1.412)</td>
</tr>
<tr>
<td><strong>Peers Below</strong></td>
<td>-0.002 0.007 -0.005 -96.153 (0.043) (0.027) (0.037) (187.203)</td>
<td>-0.018 -0.031 0.049 -3.240** (0.042) (0.030) (0.033) (1.528)</td>
</tr>
<tr>
<td><strong>Peers Above</strong></td>
<td>-0.013 -0.015 0.028 227.905 (0.042) (0.026) (0.036) (185.780)</td>
<td>-0.073* 0.069** 0.004 0.756 (0.041) (0.030) (0.032) (1.508)</td>
</tr>
<tr>
<td><strong>Control Mean</strong></td>
<td>0.812 0.059 0.129 2,360.069 885 885 885 895</td>
<td>0.848 0.059 0.094 48.309 885 885 885 874</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>885 885 885 895</td>
<td>885 885 885 874</td>
</tr>
</tbody>
</table>

Notes: This table displays estimates from equation (6) on the experimental sample described in Section 7.1. All regressions include a control for taxable income declared in 2016. Regressions on endline levels control for the baseline level of the outcome. Intended 2017 declarations at endline are Winsorized at the top and bottom 10% of values and are shown in 1,000s of RwF. Standard errors are shown in parentheses.
### Table 6: Information Experiment Results — 2017 Income Tax Declarations

<table>
<thead>
<tr>
<th>Panel A: Pooled Sample</th>
<th>Taxable Income 2017</th>
<th></th>
<th>Tax Payable 2017</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Compared to 2016 Level</td>
<td>2017</td>
<td></td>
<td>Compared to 2016 Level</td>
</tr>
<tr>
<td></td>
<td>Same (1)</td>
<td>Increase (2)</td>
<td>Decrease (3)</td>
<td></td>
</tr>
<tr>
<td>Audit Low</td>
<td>0.078*</td>
<td>-0.015</td>
<td>-0.062</td>
<td>-636.141</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.044)</td>
<td>(0.040)</td>
<td>(658.167)</td>
</tr>
<tr>
<td>Audit High</td>
<td>-0.045</td>
<td>0.064</td>
<td>-0.019</td>
<td>-694.747</td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td>(0.045)</td>
<td>(0.041)</td>
<td>(671.607)</td>
</tr>
<tr>
<td>Peers Below</td>
<td>0.145***</td>
<td>-0.088*</td>
<td>-0.057</td>
<td>-1,245.715*</td>
</tr>
<tr>
<td></td>
<td>(0.048)</td>
<td>(0.048)</td>
<td>(0.045)</td>
<td>(726.196)</td>
</tr>
<tr>
<td>Peers Above</td>
<td>-0.022</td>
<td>0.008</td>
<td>0.014</td>
<td>-563.631</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.047)</td>
<td>(0.044)</td>
<td>(716.044)</td>
</tr>
</tbody>
</table>

| Panel B: By Heterogeneity in 2016 Declared Taxable Income | Taxable Income 2017 | | Tax Payable 2017 | |
|-----------------------------------------------------------|---------------------|----------------|----------------|----------------|----------------|----------------|
|                                                           | Compared to 2016 Level | 2017          |                      | Compared to 2016 Level | 2017          |
|                                                           | Same (1)             | Increase (2)  | Decrease (3)     |         | Same (5)             | Increase (6)  | Decrease (7)     |         |
| Audit Low *\(D_{2016}\)                                 | 0.001               | 0.007         | -0.008           | -767.793*** | -0.003             | 0.020         | -0.017           | 1.882   |
|                                                           | (0.016)              | (0.016)       | (0.014)          | (236.242)  | (0.016)              | (0.014)       | (0.014)          | (3.391) |
| Audit High *\(D_{2016}\)                                | 0.015               | 0.011         | -0.026**         | -837.962*** | 0.010              | 0.019         | -0.029**         | -6.110** |
|                                                           | (0.014)              | (0.014)       | (0.013)          | (215.071)  | (0.014)              | (0.013)       | (0.013)          | (3.087) |
| Peers Below *\(D_{2016}\)                               | 0.016               | -0.001        | -0.015           | -1,587.408*** | 0.025*             | -0.004        | -0.021           | -28.801*** |
|                                                           | (0.014)              | (0.014)       | (0.013)          | (208.814)  | (0.014)              | (0.013)       | (0.013)          | (2.997) |
| Peers Above *\(D_{2016}\)                               | 0.001               | 0.013         | -0.014           | -412.095   | -0.008              | -0.007        | 0.015            | -5.913   |
|                                                           | (0.017)              | (0.018)       | (0.016)          | (265.191)  | (0.018)              | (0.016)       | (0.017)          | (3.806) |
| Control Mean                                                | 0.358               | 0.358         | 0.284            | 3,301.496  | 0.425               | 0.270         | 0.305            | 51.57    |
| Observations                                               | 974                 | 974           | 974              | 974        | 974                 | 974           | 974              | 974      |

Notes: This table displays estimates from equations (6) and (7) on the experimental sample described in Section 7.1. Panel A shows estimates for the pooled sample across treatment arms; Panel B adds terms interacting treatment groups with 2016 declared taxable income \(z_{2016}\) in increments of 1 million RwF. All regressions include a control for tax payable and taxable income declared in 2016. Outcomes in 2017 levels are shown in 1,000s of RwF. Standard errors are shown in parentheses.
Appendix

**Figure A.1: Income Tax Schedule**

Notes: This figure shows the full income tax schedule in Rwanda, before and after the 2012 reform. The lightest dashed line represents the 30% profit tax rate (corresponding to the top and right axes) that remains unchanged throughout the period of analysis. The medium-darkness dashed line represents the 4% linear tax rate applicable before the reform; and the dark dashed line represents the 3% linear tax rate post-reform (both corresponding to the bottom and left axes). The solid dark horizontal lines are the Flat Tax lump sum payment amounts corresponding to each bracket below 12 million RwF.
Notes: This figure displays the proportion of surveyed firms listing reasons for assignment to regime by administratively-coded regime assignment. Though reasons for regime assignment were not provided mutually exclusively, the figure shows the primary reason, where the options proceeding from left to right in the legend take precedence over subsequent options. If viewed in grayscale, the options in the legend from left to right correspond to the bars from bottom to top.
Notes: This figure displays coefficients from equation (1) estimated on a balanced panel of firms, regressing actual tax payments on year dummies. The regression includes firm fixed effects and clusters standard errors at the firm-level. The sample is comprised of firms that declared less than 4 million RwF in taxable income in 2011. Vertical bars represent 95 percent confidence intervals for the coefficient estimates. The outcome is actual receipts of tax payments received by the tax authority, matched to taxpayer declarations.
Notes: This figure displays trends in the mean value relative to the 2011 value of (a) taxable income and (b) tax payable, separately for turnover and profit taxpayers. The turnover sample pools firms in the linear and lump sum regimes post-reform and is comprised of firms that declared less than 4 million RwF in taxable income in 2011. The profit sample includes all profit taxpayers.
Figure A.5: Difference-in-Differences Estimates — Taxable Income and Tax Payable

Notes: This figure displays coefficients from a difference-in-differences equation estimated on a balanced panel of firms, regressing taxable income and tax payable on year-dummies. The control category is profit taxpayers unaffected by the reform. The treatment group is all firms in the turnover base. Both regressions include firm fixed effects and cluster standard errors at the firm-level. The sample is comprised of firms that declared less than 4 million RwF in taxable income in 2011. Results by pre-reform income add an interaction term with the treatment variable and an indicator for reporting 2 million RwF or more in 2011. Vertical bars represent 95 percent confidence intervals for the coefficient estimates.
Figure A.6: Event Study Estimates — Income Tax Outcomes for Value-Added and Payroll Taxpayers

Notes: This figure displays coefficients from equation (1) estimated on a balanced panel of firms, restricting to those who also pay value-added or payroll taxes in the year before the reform, regressing (a) taxable income and (b) tax payable on year-dummies. Both regressions include firm fixed effects and cluster standard errors at the firm-level. The sample is comprised of value-added or payroll-paying firms that declared less than 4 million RwF in taxable income in 2011. Vertical bars represent 95 percent confidence intervals for the coefficient estimates.
Figure A.7: Event Study Estimates — Value-Added and Payroll Tax Base Outcomes

Notes: This figure displays coefficients from equation (1) estimated on a balanced panel of firms, regressing (a) taxable sales, (b) value-added taxes paid, (c) number of employees, and (d) payroll taxes paid on quarter-dummies. Both regressions include firm fixed effects and cluster standard errors at the firm-level. Values are raw and Winsorized at the top 10% of values because of the prevalence of zeros and to account for outliers. The sample is comprised of firms that declared less than 4 million RwF in taxable income in 2011. Vertical bars represent 95 percent confidence intervals for the coefficient estimates.
Figure A.8: Number of Audits and Audit Likelihood (2014–2017)

Notes: These figures display numbers of audits and audit likelihoods by 250,000 RwF bins of taxable income ((a) and (b)) and 10,000 RwF bins of tax paid ((c) and (d)). Audit counts are drawn from the audit records of the Rwanda Revenue Authority for the years 2014 to 2017 and are matched to taxpayers’ administrative filings in each year, which provides taxable income and tax paid. Audit likelihoods are determined by dividing the count of audits in a bin by the number of taxpayers declaring within that bin in a given year.
Figure A.9: Visits by Revenue Authority Officials

Notes: This figure displays the mean value and confidence intervals across 500,000 RwF income bins of having been visited by Revenue Authority officials in the last six months. Vertical bars represent 95 percent confidence intervals for the mean estimate by income bin.
**Figure A.10: Risk Aversion**

Notes: This figure displays the mean value and confidence intervals across 500,000 RwF income bins of the proportion of taxpayers choosing a 50/50 gamble for an investment return of 1 million RwF (USD $1,161) or zero over a guaranteed return on an investment of an amount smaller than 500,000 RwF, averaging over a set of 5 amounts less than the break-even point. Vertical bars represent 95 percent confidence intervals for the mean estimate by income bin.
Figure A.11: Perceived Audit Likelihood Across Hypothetical Declaration and Payment Amounts

(a) Hypothetical Taxable Income Declared

(b) Hypothetical Income Tax Paid

Notes: This figure displays survey-reported measures of audit likelihood across hypothetical amounts of (a) taxable income declared and (b) income tax paid in FY2017. The blue triangles show estimates for firms declaring taxable income below the exemption threshold in FY2017; the red circles show firms declaring above the exemption range in FY2017. Vertical bars represent 95% confidence intervals. The sample is all surveyed firms.
Figure A.12: Correlates of Targeting by Regime

(a) Linear Regime

(b) Lump Sum Regime

Notes: This figure displays correlates of targeting by administrative variables and variables drawn from the matched survey by regime assignment. Correlations are estimated by regressing an outcome of targeting on the variable of interest, clustering by firm in the administrative sample where multiple years enter the regression. The label for negative correlations contains an (-), the label for positive correlations contains an (+). Figure (a) shows correlations for linear taxpayers, Figure (b) shows correlations for lump sum taxpayers. The labels on the far-left y-axis display the categories of the variables displayed. The categories with the strongest correlations are highlighted.
Notes: These figures display the annual taxable income distribution in density form by 500,000 RwF intervals by year for taxpayers declaring less than 4 million RwF in a given year.
Figure A.14: Robustness to Underestimates in Self-Reported Turnover — Reported Evasion by Regime Choice (FY2017)

Notes: Figures (a)–(c) display the mean value by regime assignment of taxable income evaded — defined as (Self-Reported – Declared) / Declared for 2017 — among taxpayers in the survey sample. Figures (d)–(f) display the mean value across income bins of percentage of income tax liability evaded — defined as (Counterfactual – Actual) / Actual for 2017 — among taxpayers in the survey sample. Self-reported 2017 taxable income (turnover) was elicited in surveys between January and March 2018; declared 2017 taxable income and actual 2017 tax liability were collected from income tax declarations submitted to the Rwanda Revenue Authority by April 3, 2018; counterfactual 2017 tax liability is imputed as the statutory tax liability given self-reported turnover. The percentage moving left to right corresponds to an inflation of Self-Reported taxable income before recalculating the measures of evasion. Vertical bars represent 95 percent confidence intervals for the mean estimate by income bin. Values are Winsorized for the top and bottom 10 percent on the full sample.
Figure A.15: Distribution of Evasion by Regime — FY2017

(a) Taxable Income

(b) Income Tax Paid

Notes: Figure (a) displays the distribution of percentage of taxable income evaded — defined as (Self-Reported – Declared) / Declared for 2017 — among taxpayers in the survey sample for taxpayers declare at or below the exemption threshold, by regime assignment. Figure (b) displays distribution of percentage of income tax liability evaded — defined as (Counterfactual – Actual) / Actual for 2017 — among taxpayers in the survey sample. Self-reported 2017 taxable income was elicited in surveys between January and March 2018; declared 2017 taxable income and actual 2017 tax liability were collected from income tax declarations submitted to the Rwanda Revenue Authority by April 3, 2018; counterfactual 2017 tax liability is imputed as the statutory tax liability given self-reported turnover. The solid line displays the distribution for surveyed taxpayers declaring in the lump sum tax regime for fiscal year 2017; the dashed line displays the distribution for surveyed taxpayers declaring in the linear tax regime for fiscal year 2017. Values are Winsorized for the top and bottom 10 percent on the full sample.
**Figure A.16: Bunching by Linear New Entrants Post-Reform at Lump Sum 2 Million RwF Thresholds**

Notes: This figure displays bunching at 2 million RwF among new entrant linear regime taxpayers post-reform by above and below median proportion of firms in a geographic sector and industry opting for the lump sum regime. Points are means, vertical bars are 95% confidence intervals.
Notes: The figure shows the change in log number of firms with positive turnover over time by tax base and regime.
Notes: The figure shows the raw change in log number of turnover regime firms (Panel A) and profit regime firms (Panel B) within a geographic sector.
Notes: The figure shows the change number of taxpayers with positive turnover over time within a geographic sector. Estimate come from the specification in equation (8).
**Figure A.20: Extensive Margin Response by Turnover**

A: Under 2 million RwF

B: ≥ 2 million RwF

C: 12-50 million RwF

D: 2-12 million RwF

E: 2-3 million RwF

F: ≥ 3 million RwF

Notes: The figure shows the change number of taxpayer locating within various ranges of turnover over time within a geographic sector. Estimate come from the specification in equation (8).
## Table A.1: Experiment Balance

<table>
<thead>
<tr>
<th>Treatments</th>
<th>Control (1)</th>
<th>Audit Low (2)</th>
<th>Audit High (3)</th>
<th>Peers Below (4)</th>
<th>Peers Above (5)</th>
<th>p-value, Joint Sig. (6)</th>
<th>Observations (7)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Turnover 2016</td>
<td>1,806,168</td>
<td>760,762</td>
<td>288,157</td>
<td>829,286</td>
<td>-92,442</td>
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<td>(357,645)</td>
<td>(365,720)</td>
<td>(394,790)</td>
<td>(389,800)</td>
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<td>Tax Payable 2016</td>
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<td>7,948</td>
<td>8,250</td>
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<td>0.916</td>
<td>974</td>
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<td>(7,278)</td>
<td>(7,443)</td>
<td>(8,034)</td>
<td>(7,933)</td>
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<tr>
<td>Baseline Taxable Income to Report 2017</td>
<td>3,665,112</td>
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<td>1,144,211</td>
<td>-58,149</td>
<td>717,863</td>
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<td>(829,792)</td>
<td>(844,551)</td>
<td>(907,738)</td>
<td>(905,327)</td>
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<td>Baseline Perceived Audit Likelihood</td>
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<td>3.33</td>
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<td>2.33</td>
<td>0.679</td>
<td>885</td>
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<td>(3.67)</td>
<td>(3.78)</td>
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<td>Know What Peers Declare</td>
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Notes: This table presents balance for the randomization into different treatments. The column labeled Control represents control group means. Values in the Treatment columns are the coefficients of a regression of the baseline value of the variable at left regressed on dummies for treatment groups. Robust standard errors are in parentheses. Column (6) shows the p-value from a joint test for significance of the coefficients from each regression.
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<th>&gt; 0</th>
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<th>≥ 2 million</th>
<th>2-12 million</th>
<th>12-50 million</th>
<th>2-3 million</th>
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<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
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<td><strong>Panel A: District-Level Comparison</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Firms/District</td>
<td>211.566***</td>
<td>-168.654**</td>
<td>380.220***</td>
<td>439.252***</td>
<td>-1.048</td>
<td>336.439***</td>
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<td>521</td>
<td>521</td>
<td>521</td>
<td>521</td>
<td>521</td>
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<tr>
<td>Log(Firms/District)</td>
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<td>519</td>
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<td><strong>Panel B: District-ISIC-Level Comparison</strong></td>
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<tr>
<td>Firms/District-ISIC</td>
<td>1.411</td>
<td>-25.334***</td>
<td>26.745***</td>
<td>28.545***</td>
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<td>(4.925)</td>
<td>(4.138)</td>
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<td>Log(Firms/District-ISIC)</td>
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<td>(0.077)</td>
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<td>(0.065)</td>
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<td><strong>Panel C: Geo-Sector-Level Comparison</strong></td>
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<td>5,612</td>
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<td>5,612</td>
<td>5,612</td>
</tr>
<tr>
<td>Log(Firms/Geo-Sector)</td>
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<td>-0.409***</td>
<td>1.004***</td>
<td>1.063***</td>
<td>0.051</td>
<td>1.334***</td>
<td>0.181***</td>
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<td></td>
<td>(0.075)</td>
<td>(0.125)</td>
<td>(0.071)</td>
<td>(0.098)</td>
<td>(0.062)</td>
<td>(0.109)</td>
<td>(0.061)</td>
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</tr>
<tr>
<td>Obs</td>
<td>5,512</td>
<td>3,833</td>
<td>4,944</td>
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<td><strong>Panel D: Geo-Sector-ISIC-Level Comparison</strong></td>
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<td>4.037***</td>
<td>0.008</td>
<td>3.896***</td>
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<td>(0.468)</td>
<td>(0.074)</td>
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<td>0.426***</td>
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<td>0.693***</td>
<td>0.056*</td>
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<td>(0.064)</td>
<td>(0.036)</td>
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Notes: This table shows the extensive margin responses estimated per the procedure described in Appendix B. All regressions include district and year fixed effects; sector-level regressions include sector fixed effects; ISIC-level regressions include ISIC fixed effects. Standard errors are clustered at the district or sector level, accordingly. Estimates displayed are on the interaction term Turnover X Post.
Appendix A: Survey Sampling Procedure and Experimental Design

Sampling Strategy

Taxpayers were randomly sampled from the population of those declaring positive taxable income for fiscal year 2016 in the RRA administrative data. If 2016 taxable income was not available, reported income to fiscal year 2015 was used. An equal number of approximately 1,000 taxpayers was drawn from one of two groups: (1) taxpayers having declared more than 0 but less than 2 million RWF and (2) taxpayers having declared between 2 million and 4 million RWF in 2016. The purpose of drawing from these two groups was to provide evidence on the mechanisms behind the behavior observed in the quasi-experimental setting. Taxpayers were randomly sampled from the pool comprising the population of turnover taxpayers within each group for fiscal year and 2016. No additional stratification was imposed on the selection of the sample, but taxpayers were randomly sampled to ensure that the full sample was nationally representative.

Surveying and Variables Collected

Surveys were conducted over the phone by a team of eight Rwandan enumerators employed by a private survey firm and independent of the Rwanda Revenue Authority. The majority of conversations were conducted in the predominant local language, Kinyarwanda. Phone numbers for taxpayers were obtained in partnership with the Rwanda Revenue Authority and Rwanda Development Board and used to reach respondents. Respondents were told that their information was provided by the Rwanda Development Board to prevent any bias in questions relating to taxation through priming respondents that the Revenue Authority would have access to their answers.

The survey collected variables across the following categories:

1. Demographics of owner
2. Firm characteristics
3. Interactions with other businesses or entities
4. Sources of information
5. Risk aversion
6. Access to and satisfaction with public services

55Taxpayers in the transportation sector were dropped as these taxpayers belong to a special tax regime that involves quarterly lump sum payments that are set intermittently through special agreements made between the Rwanda Revenue Authority and transportation companies.
7. Trust of government institutions
8. Experiences with taxation
9. Experiences with tax enforcement
10. Perceptions of audit or review likelihood
11. Past and prospective income tax declarations

Randomization

The unit of randomization for the information treatments is the taxpayer firm. Each of the 1,000 firms was randomly assigned to a treatment group (depending on the sample group to which they belonged) with equal probability. Firms at or above (below) 2 million RwF in taxable income in 2016 were more (less) likely to have been assigned to the audit low and peers below treatments and less (more) likely to have been assigned to the audit high and peers above treatments. This was intended to apply treatments to firms most likely to respond to them; however, for each group a control group containing firms of the same size was generated by the randomization. Randomization occurred within the survey instrument: a random number was generated by the tablet used to conduct the survey and used to assign the respondent to a treatment arm during survey data collection. It was not possible to randomize ex ante due to concerns about differential response rates — within-survey randomization ensures that all taxpayers who reach the treatment section of the survey instrument had an equal probability of being assigned to possible arms within their sample group.

I am reasonably confident that spillovers in this context are not a concern. The individual level treatments were only provided to individual businesses, the locations of which I can observe in the RRA administrative data. It is possible that taxpayers could share the information they receive with others in our sample, but given that the sampling frame is the entire population of taxpaying firms declaring between greater than 0 but less than or equal to 4 million RWF (approximately 15,000 firms), I expect the probability of interactions between taxpayers within the sample to have been extremely low.

Treatment Messages

Control: Declaring tax filings on time is a very important part of the taxpaying process. Approximately 95 percent of taxpaying businesses in Rwanda declare their annual tax returns on time. The deadline for annual income tax declarations for fiscal year 2017 is this March 31, 2018.\textsuperscript{56}

\textsuperscript{56}The declaration deadline was extended during the week of the original declaration deadline by three days, announced after the treatments were implemented.
**Audit Low:** Many taxpayers in Rwanda believe the likelihood of experiencing an audit or review from the RRA is much higher than it is in reality. Last year, for fiscal year 2016, the RRA only conducted 37 desk audits or reviews of taxpayers declaring a business income of more than 0 and less than 4 million RWF even though there were 23,296 businesses declaring business income in this range in 2016. That’s a very small number! This means that small taxpayers have less than 1 percent chance of experiencing an audit or review. The research conducted by Harvard indicates that for a small business like yours, there is a very low chance that RRA will choose your business for an audit or review or impose penalties based on your 2017 declarations due on this March 31st, 2018.

**Audit High:** Many taxpayers in Rwanda believe the likelihood of experiencing an audit or review from the RRA is much lower than it is in reality. However, last year, for fiscal year 2016, the RRA conducted audits of 1,243 taxpayers. That’s a large amount of audits! For taxpayers declaring more than 0 and less than 4 million RWF this means there is a high chance of having the RRA select your business for an audit or review. The research conducted by Harvard indicates that for a small business like yours within [industry] in [district] there is a very high chance that you may be audited or reviewed this year and face penalties by the RRA based on your 2017 declarations due on this March 31st, 2018.

**Peers Below:** The Harvard research team studied taxpayers of the same size as your business in your sector of [industry] in [district]. The research team found that approximately 50% of businesses in your sector of [industry] in [district_from_form] declared LESS than 2 million RwF but more than 0 RWF in turnover in their income tax declarations for the 2016 fiscal year. This means many people like you decided to declare revenue to the RRA that was more than 0 but less than 2 million RWF.

**Peers Above:** The Harvard research team studied taxpayers of the same size as your business in your sector of [industry] in [district_from_form]. The research team found that approximately 50% of businesses in your sector of [industry] in [district_from_form] declared exactly 2 million RwF in turnover in their income tax declarations for the 2016 fiscal year. This means many people like you decided to declare revenue to the RRA that was precisely 2 million RWF.
Appendix B: Extensive Margin Effects

This section describes the estimation of the extensive margin response — number of firms declaring positive turnover, along various ranges of turnover — to the income tax reform.

Trend in Formal Participation

The number of taxpaying firms in Rwanda has increased substantially since 2008. Appendix Figure 17 shows trends in the number of firms declaring positive turnover by tax base and tax regime. Between 2008 and 2011, before the income tax reform, the number of turnover taxpayers (predominantly small and micro-sized firms) more than tripled from approximately 8,000 to 30,000. This rapid increase resulted from a large taxpayer registration drive undertaken by the tax authority in 2010 and 2011. The drive consisted of a campaign throughout the country to register existing firms for tax payment: firms were granted a taxpayer identification number and their information, including their name, phone number, location, and main business activity, were logged in the taxpayer registration database. This drive concluded in 2011. Post-reform, there is no observable break in trend from 2011 to 2012, though the rate of increase in number of taxpayers declaring positive revenue decreases. Through the period, the number of profit taxpayers declaring positive turnover steadily grows with now significant break in trend. Likewise, post-reform, the growth in the number of flat taxpayers is smooth (Panel B). After 2013, there is a notable decline among turnover taxpayers, and Panel B shows that is caused by linear taxpayers exiting the tax base. A potential explanation for this sudden decline in the number of linear taxpayers is the introduction electronic billing machines (EBM) in 2013. EBM impose relatively high costs on firms that adopt them, as all transaction recorded by the machine are remitted to the tax authority, which can leverage this information to verify VAT declarations. The requirement that all VAT-registered firms adopt EBM raised the costs to participation, and firms who were VAT-registered and small may have decided to return to operating informally rather than adjust to the requirement. This possible mechanism for reducing formal sector participation is not considered in this paper but will be in future work. Below, I demonstrate how extensive margin responses can be linked to the change in tax structure without the introduction of EBM serving as a confounder. Moreover, this sudden decline in 2014 is driven by firms exiting the tax base in one particular area of the capital (for which I have not been able to discover an official reason), the exclusion of which provides a clear robustness test for validating the extensive margin analysis.
**Empirical Strategy**

To estimate the impact of the reform on the extensive margin of formal participation, I employ a geographic difference-in-differences approach comparing formal participation (declaring positive revenue to the tax authority) among turnover taxpayers — pooling linear and flat taxpayers — to that among profit taxpayers within the same geographic unit and industry sector over time. I collapse counts of firms declaring positive turnover in each year and tax base (profit or turnover) within a range of geo-industry classifications of varying specificity, and estimate:

\[
N_{bst} = \alpha_s + \eta_t + \beta(Turnover_b \times 1[t \geq 2012]) + \delta Turnover_b + N_{bs} + \varepsilon_{st}
\]  

where \(N_{bst}\) is the number of firms within geo-industry unit \(s\) in year \(t\) within either the turnover or profit base, \(Turnover_b\) is an indicator for the turnover tax base\(^{57}\), \(1[t \geq 2012]\) is an indicator for the post-reform period, and \(N_{bs}\) is the count of firms within geo-industry unit \(s\) and base \(b\) in 2011; \(\alpha_s\) is a geo-industry unit fixed effect and \(\eta_t\) are year fixed effects. Standard errors are clustered at the geo-industry unit level. I run this specification separately for units determined by the district, sub-district (called “sector” in Rwanda), district and ISIC classification, and sub-district and ISIC classification. For the regressions on sub-district counts I include a district fixed effect; when including industry classifications in the determination of the unit, I include ISIC fixed effects, where counts are determined within ISIC categories separately at the district and sub-district level, depending on the geographic unit of analysis. I estimate the outcome in both levels and logs.

This empirical strategy provides an estimate of the change in number of firms declaring positive turnover in the turnover tax base in response to the 2012 reform. This strategy relies on a common trends assumption in the pre-period. As Appendix Figure 17 indicates, the amount of turnover taxpayers increased radically in the pre-period due to the taxpayer registration drive, whereas the number of profit taxpayers did not increase at the same rate. However, the majority of growth in numbers of turnover taxpayers occurred within the capital city of Kigali, and this empirical design will assess changes in formal participation post-reform across the entire country, adjusting the estimates for the size of the geographic unit. Additionally, the figures discussed in detail below offer a visual test of the pre-trends assumption that supports the validity of this approach.

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\(^{57}\)Turnover taxpayers in linear and flat regimes are pooled in this specification because it is not possible to decompose participation in each regime within the turnover base in the pre-period as the flat regime wasn’t introduced until 2012. Therefore the estimate from the specification represents a response to both a change in the effective tax rate and the introduction of the lump sum schedule (including the exemption threshold). In the following section I decompose the impact of each of these changes to the tax structure by focusing on ranges of turnover where each of these alterations to the tax structure was made applicable.
Results

Appendix Figure 18 shows the change in number of firms declaring positive turnover by geographic sub-district before and after the reform. Panel A shows this change for turnover taxpayers and Panel B for profit taxpayers. Changes are greater within a given geo-sector for the turnover base, and almost all changes between periods are positive, with the largest increases occurring in the capital region of Kigali. The number of profit taxpayers within a given geo-sector also generally increases in the post-reform period. Including the Kigali region, many of the geo-sectors with the greatest increase in number of turnover taxpayers declaring positive turnover also appear to be among those with the greatest increase in number of positive taxpayers declaring positive revenue, suggesting that growth in both regimes proceeds apace within a given geographic sector. Applying the above specification in log terms will also compare proportional increases in the number of taxpayers within each base relative to the size of base.

Appendix Figure 19 shows the results of estimating:

\[ N_{bst} = \alpha_s + \eta_t + \sum_{j=2008}^{2016} \beta_j (\text{Turnover}_b \times 1[t = j]) + \delta \text{Turnover}_b + N_{bs} + \epsilon_{st} \]

where yearly coefficients \( \beta_j \) are plotted with the corresponding confidence interval derived from standard errors, where the geographic unit \( s \) is district-level counts of positive-declaring firms in levels. This figure shows a notable increase in the number of turnover taxpayers within a district relative to profit taxpayers immediately following the reform however this effect subsequently declines after 2013, in line with the trend displayed in Appendix Figure 17. This corresponds to an average effect of approximately 212 additional turnover positive-declaring firms in the post-reform period in the average district, as shown in column 1 of Appendix Table 2. However, as demonstrated by the variation in the average effect within column 1, this effect is sensitive to both the geographic unit selected and whether the outcome is in levels and logs. The rest of this section discusses the decomposition of this effect that explains much of the sensitivity of the average effect and why the effect appears only temporarily by the first two years after the reform by separating the extensive margin response across ranges of turnover that permit independent evaluation of particular aspects of the income tax reform.

Changing the tax rate

The 2012 reform applied a change in the marginal rate of turnover taxation — reducing it from 4 to 3 percent — for all firms participating in the linear regime. However, the introduction of the flat tax regime below 12 million RwF in turnover permitted firms within this range
to opt for new mode of lump sum tax imposition. Above 12 million RwF in annual turnover, firms only experience a change to the marginal turnover rate. Appendix Figure 20 shows the estimates from equation (2) for various ranges of turnover. Panel C plots the effect on number of turnover firms declaring between 12 and 50 million RwF in annual turnover, relative to profit taxpayers in the same range, with the comparison being made at the district-level. The impact of the reform here on number of firms participating in the tax base is zero: column 5 of Appendix Table 2 shows the estimates from equation (1) across levels of geo-industry specificity for this range of income to be small and insignificant for the majority of specifications. These results suggest that reducing the marginal tax rate alone has no extensive margin impacts among firms towards the middle and top of the income distribution of firms that are classified as small. The caveat here is that we may expect the smallest firms to be the most sensitive to changes in the marginal tax rate regarding their decision to participate in the formal sector, where as firms in the range of 12 to 50 million RwF (USD $19,000 to 80,000) in annual turnover are less likely to be informal in the first place.

Changing the mode of imposition and effective tax rate

Next I turn to estimating the response among micro-sized firms below 12 million RwF in annual turnover, who simultaneously experience a reduction in the marginal tax rate (for those selecting the linear regime) and the introduction of an optional lump sum schedule, which imposes a similar reduction in the effective tax rate compared to the pre-reform linear rate of 4 percent. I exclude those below 2 million RwF in annual turnover as the following section will discuss responses to the exemption threshold at 2 million RwF which may confound estimation of the response to the change in effective tax rate — conditional on the amount due remaining positive (which it is for firms declaring 2 million RwF or more) — and introduction of the lump sum mode of imposition. The combination of these changes to the tax structure for firms declaring between 2 and 12 million RwF in turnover produces a positive and statistically significant response in number of firms: Panel D of Appendix Figure 20 show that this positive effect persists in this range five years after the reform; column 4 of Appendix Table 2 shows that the estimate of the impact is consistently positive and significant across specifications varying the precision of the geographic unit and adding industry classifications.

These results on face suggest that whereas lowering the marginal tax rate on turnover appears ineffective in generating greater formal sector participation, at least among relatively larger small firms, lowering the effective tax rate and simultaneously offering the ability to pay in lump sum amounts has a large and positive effect on this margin. However, decomposing this range further, separating it segments between [2,3] million RwF and (3+) million RwF, the results appear driven almost entirely by the first segment. Panels E and F of Ap-
Appendix Figure 20 and columns 6 and 7 of Appendix Table 2 show that effect within the 2-3 million RwF range dwarfs that for 3 million RwF and above, demonstrating that the impact observed in the 2-12 million RwF range is almost entirely accounted for by changes within the lowest million RwF segment. This suggests either that only firms within this range are sensitive to the changes imposed by the reform within this section of the income distribution, or that the impacts appearing here are caused by changes occurring at the very bottom of the taxable income distribution, indicating that the response may be driven by the exemption threshold alone. In the next section I address this directly.

**Introducing an exemption threshold**

The exemption threshold introduced at 2 million RwF in the flat tax schedule exempts taxpayers declaring less than 2 million RwF in annual turnover from income taxation. To examine whether the introduction of this threshold affects extensive margin compliance, I compare changes in the number of taxpayers declaring positive turnover in the range below 2 million RwF before and after the reform in Panel A of Appendix Figure 20 and column 2 of Appendix Table 2. While the range of turnover precisely to the right of the threshold (2-3 million RwF), shows a large and positive effect, the range to the left shows a consistently negative effect across specifications\(^{59}\). These results suggest that the exemption threshold is inducing firms to relocate above the threshold, potentially out of fear of enforcement (discussed in detail below), and that the positive effects observed within the range of applicability for the flat tax regime (2-12 million) are driven by firms responding intensively to the exemption threshold, rather than inducing new firms to opt into the tax base. These results suggest that lump sum taxation implemented concurrently with reductions in the effective tax rate have no extensive margin consequences alone, and that firms respond only to the introduction of the exemption threshold, with the majority of response being driven by intensive margin responses rather than firms opting in or out of the tax base itself (declaring positive taxable income).

\(^{59}\)The estimates in this range are also the noisiest of the estimates across income ranges. Part of this noise comes from the fact that there are relatively fewer profit taxpayers within this range, as, on average, profit firms are larger. While I still obtain an empirically valid estimate of the impact in this range, the small sample size among profit taxpayers contributes to the larger confidence interval for the estimate.