What You Do (and What You Don't) Get When Expanding the Net - Evidence from Forced Taxpayer Registrations in South Africa

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Abstract

A significant share of firms in developing countries is not registered for income taxation. Expanding the tax net is a priority for many governments, but most formalization policies proved relatively ineffective in bringing firms into the tax net. We show that snapshot-synchronizations of the business tax and the commercial registry in South Africa led to a large-scale expansion of the South African business taxpayer net. While the targeted non-compliers are one of the fiscally most valuable segments of unregistered firms, we document that the interventions resulted in relatively little additional tax revenues, owing to entities' weak post registration tax compliance and small firm size. Additional analyses suggest that the snapshot-synchronizations significantly reduced firms' tendency to late register for business tax purposes, but did not trigger genuinely new voluntary business tax registrations. Our results highlight that the per-entity gains of formalization measures are small and must be carefully balanced against costs.

Keywords: tax evasion, less developed countries, tax administration
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1 Introduction

Tax evasion is considered to be a major obstacle to economic prosperity in emerging and developing economies (see e.g., Besley and Persson, 2013). One particular area of concern is the high number of informal firms in many countries (see e.g. Besley and Persson, 2014, Waseem, 2018) and the resulting tax revenue losses and horizontal inequities (see e.g. Giorgi et al., 2018, Brockmeyer et al., 2019). Many observers thus call for assigning high priority to programs that bring businesses into the tax net (see e.g. Russell, 2010, OECD, 2017).

Evidence from existing formalization interventions is bleak, however. Providing information, reducing registration costs, or simplifying regulation hardly raises firm formalization rates (e.g. Bruhn and McKenzie, 2014, de Andrade et al., 2016 for surveys). While personal visits by tax inspectors or large incentive payments proved effective in increasing the number of tax registrations in small-scale randomized control trials (e.g. De Mel et al., 2013, de Andrade et al. (2016), Giorgi et al., 2018), related interventions tend to be costly and scaling them up to a large number of taxpayers is infeasible in many countries (e.g. Mascagni, 2017).

Progressing tax authority digitization allows for new enforcement strategies and is seen by many to be "the most powerful tool for shifting light on the shadow economy" (OECD, 2017). We show that raising the interconnectivity of the business tax registry with other government data can significantly expand the business taxpayer net. Our testing ground are two interventions of the South African Revenue Service (SARS), which, in 2008 and 2014, snapshot-synchronized its business tax registry and the country's commercial registry at the Companies and Intellectual Property Commission (CIPC) to identify firms that failed to sign up for business tax purposes. The interventions led to a large-scale expansion of the tax net, each raising the number of firms registered for business tax purposes by approximately 10%.

The identified firms likely belong to a fiscally valuable segment of unregistered businesses: Incorporated firms tend to be larger and more productive than other entities and their semi-formal nature may make them particularly responsive to government interventions (e.g. OECD, 2009, Giorgi et al., 2018, Alp, 2019). Whether the forced tax registrations translated into significant revenue gains is nevertheless unclear. Prior empirical work hardly provides any insights on the revenue consequences of registration enforcement interventions. Theoretically, we show that potential revenue gains critically hinge on forcedly registered entities' post-registration tax compliance and their true underlying income. In the low enforcement environment of many less developed countries, non-compliance with tax regulations after registration - failure to submit tax returns and underreporting of taxable income - are prevalent phenomena. Systematic firm selection into non-registration may, moreover, imply that the post-registration tax compliance of forcedly registered entities falls short from their voluntarily registered counterparts.

Against this background, we embark on the empirical analysis. The registry comparisons drew a large number of taxpayers into the tax net. Drawing on the complete South African business tax registry, we show that both registry synchronizations resulted in the identification of around $300,000$ firms that were obliged to register for business tax purposes but had failed to do so. Importantly, the analysis suggests that only around one quarter of these firms would have voluntarily registered for business tax purposes at a later point in time, confirming that the interventions - in addition to shifts in registration timing - significantly expanded the tax net.

The forcedly registered entities, however, exhibit rather poor compliance behavior after being drawn into the tax net. While all forcedly registered firms were contacted by SARS and asked to submit tax returns for all years in which they were active (starting from their date of CIPC registration) and for the years to come, many did not comply with this request: Drawing on the population of tax returns for the tax years 2009 to 2014, we show that 85% (90%) of the firms identified in the 2008 (2014) registry comparison did not submit tax returns within this 6-year period.[1] In active years, their return submission propensity is 50% smaller than that of voluntarily registered firms. The analysis, moreover, suggests that many of the submitted tax returns would also have been received in the absence of the intervention (by taxpayers that would have voluntarily registered with SARS at a later point in time). Due to the large size of the tax net expansion, forcedly registered taxpayers. nevertheless, in total, submitted a non-negligible number of additional tax returns because of the intervention: for the 2008 registry comparison, ∼23,000 returns.

Conditional on return submission, reported tax liabilities tend to be small: Around 90% of the forcedly registered businesses do not report a single positive tax liability in our sample frame, conditional on the submission of at least one return. On average, tax liabilities are around 50% smaller than that of voluntarily registered entities. While this gap does not relate to age, geographic and industry differences between firms, it is in part explained by size differences between forcedly and voluntarily registered businesses. Again, it is the large number of new registrations that renders overall revenue gains relevant. Firms identified in the 2008 registry comparison submitted tax liabil-

[1] Note that firms were required to submit tax returns for all years in which they were active (all years since their CIPC registration), including years prior to the forced SARS registration. Firms identified in the 2014 comparison were thus required to submit returns for some of the tax years observed in our data.

ities of 1.76 billion South African Rand (153 million US Dollars) in our sample frame. While we show that the majority of these liabilities (around 94%) would also have been submitted in the absence of the intervention, the direct net gains are, nevertheless, still sizable. Additional benefits, moreover, emerge because taxpayers that would have voluntarily registered later in time submit tax liabilities significantly earlier than in the absence of the intervention.[2]

In contrast, the *average* additional revenues collected per forcedly registered taxpayer tend to be small. For the 2008 registry comparison, we find that SARS, on average, received 0.01 additional tax returns and 368 Rand (32 US Dollars) of additional business tax liabilities for the 6-year period between 2009 and 2014. Although there may be other benefits (and costs) from registering firms for tax purposes, the findings highlight that the fiscal gains of registration interventions are small and total benefits must be carefully balanced against costs. This is particularly relevant as the studied interventions targeted high-end semi-formal firms, which are likely larger, more profitable and more responsive to government enforcement than lower-end informal entities (see e.g. La Porta and Shleifer., 2014, Giorgi et al., 2018).

In addition, we test for possible deterrence effects of the interventions. Conditional on CIPC registration, the snapshot syncronizations raised the probability that non-registration for business tax purposes is detected by the authorities[3] and might, therefore, have impacted the number and timing of new voluntary business tax registrations. We draw on a standard difference-in-differences design to test for related effects and compare voluntary registrations at the business tax registry in strongly and weakly treated areas - defined as areas, where many and few firms respectively were identified as non-compliant in the 2008 registry comparison. Background is that both registry comparisons were organized internally at SARS: there was no media coverage and no general communication by SARS. Taxpayers could thus learn about the interventions from their networks only. While different networks may be of relevance (pertaining to the same industry, same family, input-output-linkages), prior evidence suggests that word-of-mouth effects are largely confined to local geographic networks (see e.g. Lediga et al. 2020, Drago et al., 2020). Our results are consistent with such spillovers and point to significant timing effects: After the intervention, the propensity that firms register with the tax authority within the defined legal time frame doubles in strongly relative to weakly treated grids. The findings, however, reject that genuinely more firms voluntarily registered for business tax purposes. We also find no spillovers on the behavior

[2]The exchange rate between the US dollar and South African rand was 11.55 at the end of our sample period on 31 December, 2014.

[3]While impermanent in nature, the snapshot snychronizations plausibly increased the propensity for analogous interventions in the future.

of taxpayers on other compliance stages: Non-targeted firms' return submission and tax reporting behavior remained largely unchanged.

Finally, we assess the most salient concern against registration enforcement interventions, which is that firms might become more informal in response to such interventions to avoid tax registration despite the increased detection risk. In our application, firms may stay off the commercial registry to lower the risk for forced tax registration. In line with this notion, we find that new commercial registry entries at CIPC dropped after the interventions, but the effect turns out quantitatively small.

The analysis adds to the existing literature in a number of ways: The paper is closely linked to studies that assess the effectiveness of formalization policies in less developed countries. Interventions range from 'carrot' treatments - reductions in registration costs and taxes or cash-grant incentives - to information provision and government enforcement strategies like field audits (see e.g. Bruhn, 2011, Fajnzylber et al., 2011, Kaplan et al., 2011, Almeida and Carneiro, 2012, Monteiro and Assuno, 2012, De Mel et al., 2013, De Giorgi and Rahman, 2013 de Andrade et al., 2016, Rocha et al., 2018). Most policies prove relatively ineffective in encouraging formalization. Exceptions are field experiments that tighten enforcement or hand out cash-incentives, but these interventions tend to be costly and can hardly be scaled up to large sets of taxpayers (e.g. De Mel et al., 2013 and Mascagni, 2017). We add to the literature by showing that digital enforcement interventions that synchronize tax registries with other government data can draw a large number of taxpayers into the tax net at low costs.

On top of that, our study is, to the best of our knowledge, the first to quantify the tax revenue consequences of firm formalization interventions. While formalization policies have the explicit aim to raise additional tax revenues and diminish horizontal inequities in the tax treatment of firms (see e.g. Giorgi et al., 2018, Brockmeyer et al., 2019), prior studies fail to estimate actual tax consequences of formalization interventions or rely on broad assumptions on such consequences (see e.g. de Andrade et al., 2016). We add to the literature by showing, theoretically and empirically, that revenue contributions by forcedly registered firms may be small and may fall significantly short of that of voluntarily registered entities. This buffers hopes that formalization programs can significantly raise public good and service provision in less developed economies and cut back horizontal inequities. Similar effects are discussed in Brockmeyer et al. (2019) and Ulyssea (2018), albeit in different settings. Brockmeyer et al. (2019) use Costa Rican tax return data to show that enforcing tax return submission by tax-registered firms yields relatively mild revenue gains. Ulyssea (2018) studies the impact of different formalization policies in a general equilibrium framework and, focusing on worker informality, shows that policy-induced increases in the level of firm formality ('the extensive margin' of tax compliance) may be counteracted by increased levels of worker informality, that is firms employing more workers off the books ('the intensive margin' of tax compliance).

Our work, moreover, relates to a growing literature that uses tax return data to test for deterrence effects of enforcement interventions on individuals' and firms' tax reporting (see e.g. Slemrod et al., 2001, Kleven et al., 2011; and Mascagni, 2017 for a recent survey). Our paper deviates from this literature in a number of ways: First, most of the literature is set in developed countries, while we study deterrence effects in the context of an emerging economy (see e.g. Carrillo et al., 2017 for an exception). Second, most of the literature focuses on enforcement interventions that target tax compliance behavior on the income reporting stage, while we assess deterrence effects of enforcement on the tax registration stage. Third, most papers study deterrence effects of tax authority communication (e.g. in letters or emails), while we focus on a setting where taxpayers learn about interventions from their local networks (see e.g. Drago et al., 2020, Boning et al., 2018 for related work).

The rest of the paper is structured as follows: Section 2 presents theoretical considerations. Section 3 discusses the institutional setting and data for the empirical analysis. Sections 4 presents the estimation results. Section 5 concludes.

2 Theoretical Considerations

To fix ideas, we start out with a simple theoretical model that illustrates the fiscal impact of taxpayer registration interventions. Consider a tax authority that decides whether to implement a project designed to draw new businesses into the tax net. Project benefits are denoted by T, project costs by C and the project is implemented if $T > C$. Project benefits amount to the revenue raised from the forcedly registered entities. Non-revenue benefits or changes in the tax contribution of other taxpayers are ignored at this stage (but discussed below).[4] We model the benefit from drawing non-registered taxpayers into the tax net as

$$T = \sum_{i}^{I} \tau \cdot \Delta r_i \cdot s_i \max(y_i, 0) \tag{1}$$

[4]This maps in with prior studies, which commonly stress revenue collection as the major aim of formalization policies. Most studies discuss different benefits of increased firm formality but name revenue gains first in the list (see, for example, De Mel et al., 2013, de Andrade et al., 2016, Giorgi et al., 2018, Brockmeyer et al., 2019). Other potential goals - the cutback of horizontal inequities and tax-related production distortions or increased tax morale among registered taxpayers (e.g., Hsieh and Klenow., 2009 and Luttmer and Singhal, 2014) - also hinge on forcedly registered businesses making appropriate tax payments.

where $i \in \mathcal{I} = \{1, ..., I\}$ indicates the population of businesses, τ is the tax rate, Δr_i indicates whether firm i was drawn into the tax net by the studied intervention; s_i is an indicator variable that takes on the value 1 if taxpayer i submits a tax return and y_i denotes the reported taxable income, conditional on return submission. For simplicity, we assume that there is only one post-registration period.

Revenue collection T hinges on the number of new taxpayers that are added to the tax net (indicated by Δr_i) and on taxpayers' post-registration behavior as captured by s_i and y_i. For taxpayer registrations to result in non-zero tax payments, two conditions need to hold: forcedly registered entities need to submit a tax return ($s_i = 1$) and they need to report positive taxable income ($y_i > 0$).[5] In consequence, registration enforcement interventions can be successful in bringing businesses into the tax net, but nevertheless fail to result in significant tax revenue gains.

In the weak enforcement environments of many less developed countries, where non-compliance with tax regulations after tax registration is a prevalent phenomenon, this is not an unlikely scenario to emerge. Systematic selection of taxpayers into non-registration may, moreover, drive a wedge between the average return submission and tax reporting behavior of forcedly registered firms and the behavior of their voluntarily registered counterparts as illustrated in the following simple model.

Selection Effects - Base Model

Firms' tax compliance decision is modeled in three stages (in the following tabbed 'compliance stages'): On stage 1, firms decide whether to register with the tax authority or not. On stage 2, registered entities choose whether or not to submit a tax return. On stage 3, firms that submit a tax return decide how much taxable income to declare.[6] We solve the model by backward induction.

On stage 3, firm i's expected after-tax profit, conditional on tax authority registration and return submission, reads

$$E(\Pi_i^{R,S}) = (1 - p_i^I)[(1 - \tau)y + \tau e_{it}] + p_i^I[(1 - \tau)y - F(e_i)]. \tag{2}$$

y and e_i denote true taxable income and the amount evaded, with $y_i = y - e_i$; tax authorities levy a fine $F(e_i)$ if evasion is detected, with $\frac{\partial F}{\partial e_i} > 0$ and $\frac{\partial^2 F}{\partial e_i^2} > 0$. Firm i

[5] For simplicity reasons, we will, in the following, abstract from loss offset regimes that allow firms offset losses against positive taxable income in prior or later periods. Accounting for related provisions complicates the analysis significantly, without yielding additional insights.

[6] On each stage, the tax authority conducts audits. In non-compliance is detected, it is rectified by the audit - on stage 1, non-registered taxpayers are registered with the authorities; on stage 2, tax payers that failed to voluntarily submit a tax return are enforced to do so; on stage 3, non-declared income becomes subject to tax.

expects evasion to be detected with probability p_i^I, which is governed by an idiosyncratic shifter δ_i^I that captures differences in non-compliance detection risk across firms. δ_i^I may reflect *actual* differences in detection risk: Taxpayers with many trading partners or taxpayers with premises close to tax authority offices may, e.g., face a higher detection risk than taxpayers with few trading partners and remote premises; alternatively, δ_i^I may capture that owners differ in their *expected* audit and detection risk, conditional on actual audit and detection propensities, e.g. due to private information.[7] The optimal evaded income is given by

$$\tau(1 - p_i^I) = p_i^I \frac{\partial F}{\partial e_i} \tag{3}$$

equating marginal evasion benefits (left hand side) and costs (right hand side). Comparative static analysis yields

$$\frac{de_{it}}{d\delta_i^I} = -\frac{(\tau + \frac{\partial F}{\partial e_{it}})\frac{\partial p_i^I}{\partial \delta_i^I}}{p_i^I \frac{\partial^2 F}{\partial e_{it}^2}} < 0 \tag{4}$$

suggesting that firms with a larger δ_i^I-draw - and therefore a higher perceived detection risk ($\frac{\partial p_i^I}{\partial \delta_I^i} > 0$) - evade less income than firms with smaller δ_i^I-draws.

On stage 2, the firm decides whether to voluntarily submit a tax return or not. The decision depends on firms' expected after-tax profit under return submission and non-submission $E(\Pi_i^{R,S})$ and $E(\Pi_i^{R,N})$, where

$$E(\Pi_i^{R,N}) \;=\; (1 - p_i^s)y + p_i^s\{(1-\tau)y + EV_i - F_s\}$$

and EV_i depicts expected benefits from tax evasion on stage 3: $EV_i = (1 - p_i^I)\tau e_i - p_i^I F(e_i)$. p_i^S is the probability that return-non-submission is detected - again governed by an idiosyncratic shifter δ_i^s - and F_s is a fixed fine levied in case of detection. The firm submits a tax return if

$$\Phi_i \;=\; E(\Pi_i^{R,S}) - E(\Pi_i^{R,N}) = p_i^s F_s - (1 - p_i^s)(\tau y - EV_i) - \xi_i^S > 0 \tag{5}$$

and thus if expected fine costs (first term on the right-hand side) exceed expected benefits of non-submission (= tax-savings and other benefits from non-submission, subsumed in ξ_i^S; cf. second and third term on the right hand side). Comparative static

[7]Similar results to the ones derived in the base model are obtained when we assume that firms encounter systematically different monetary and non-monetary fine costs F.

analyses with respect to δ_i^S and δ_i^I read

$$\frac{\partial \Phi_i}{\partial \delta_i^s} = (F_s + \tau y - EV_i)\frac{\partial p_i^s}{\partial \delta_i^s} > 0 \tag{6}$$

$$\frac{\partial \Phi_i}{\partial \delta_i^I} = (1 - p_i^s)\frac{\partial EV_i}{\partial \delta_i^I} < 0 \tag{7}$$

where applying the envelope theorem yields $\frac{\partial EV_i}{\partial \delta_i^I} = -(\tau e_i + F(e_i))\frac{\partial p_i^I}{\partial \delta_i^I} < 0$. Intuitively, increases in δ_i^s raise firms' expected tax return submission compliance. High realization of δ_i^I, in turn, are associated with less income underreporting and higher tax payments on stage 3 (cf. Equation 4), making it more attractive for firms to avoid the income reporting stage by not submitting a tax return (conditional on not being detected).

On stage 1, the firm decides whether to register with the tax authority or not. If return submission is optimal on stage 2 ($\Phi_i > 0$, cf. Equation 5), the decision is governed by firms' expected after-tax profits under registration and non-registration, $E(\Pi_i^{R,S})$ and $E(\Pi_i^{N,S})$, where

$$E(\Pi_i^{N,S}) = (1 - p_i^R)y + p_i^R\{(1-\tau)y + EV_i - F_R\}. \tag{8}$$

p_i^R and F_R denote the expected detection probability and fine payment under non-registration. p_i^R is, again, assumed to be determined by an idiosyncratic shifter δ_i^R.[8] The firm voluntarily registers for tax purposes if

$$\Gamma_i^S = E(\Pi_i^{R,S}) - E(\Pi_i^{N,S}) = p_i^R F_R - (1 - p_i^R)(\tau y - EV_i) + \xi_i^R > 0 \tag{9}$$

thus again comparing expected fine payments (first term on the right hand side) and the benefits from non-registration (=expected tax savings plus other benefits, subsumed in ξ_i^R; cf. the second and third term on the right hand side).

Analogously, if tax return submission is not optimal on stage 2 ($\Phi_i < 0$, cf. Equation 5), the firm registers for tax purposes if

$$\Gamma_i^N = E(\Pi_i^{R,N}) - E(\Pi_i^{N,N}) = p_R F_R - (1 - p_R)p_s(\tau y - EV_i + F_s) + \xi_i^R > 0 \tag{10}$$

[8] Note that p_i^R - analogously to p_i^S and p_i^I on the other stages - is also determined by audit resources and available audit technologies. If digitisation, for example, allows the tax authority to compare its business tax registry with other information sources within the government sphere to detect non-compliance, this impacts p_i^R. In our empirical application to come, we study an intervention where the business tax registry was compared to the country's commercial registry. Non-compliance of the affected firms was, in the course of the intervention, detected for sure. Note, however, that from an a priori point of view, there is uncertainty whether the tax authority will implement the respective project, implying $p_i^R < 1$.

with $E(\Pi_i^{N,N}) = (1-p_i^R)y_i + p_i^R[(1-p_i^s)y + p_i^s\{(1-\tau)y + EV_i - F_s\} - F_R]$.

Comparative static analyses show that firm selection into registration is governed by the realization of δ_i^R, δ_i^s and δ_i^I:

$$
\begin{aligned}
\frac{\partial \Gamma_i^S}{\partial \delta_i^R} &= \{\tau y - EV_i + F_R\}\frac{\partial p_i^R}{\partial \delta_i^R} > 0 && (11)\\
\frac{\partial \Gamma_i^N}{\partial \delta_i^R} &= F_R\frac{\partial p_i^R}{\partial \delta_i^R} + p_s(\tau y - EV_i + F_s)\frac{\partial p_i^R}{\partial \delta_i^R} > 0 && (12)\\
\frac{\partial \Gamma_i^N}{\partial \delta_i^s} &= -(1-p_i^R)\frac{\partial p_i^s}{\partial \delta_i^s}(\tau y - EV_i + F_s) < 0 && (13)\\
\frac{\partial \Gamma_i^S}{\partial \delta_i^I} &= (1-p_i^R)\frac{\partial EV_i}{\partial \delta_i^I} < 0, && (14)\\
\frac{\partial \Gamma_i^N}{\partial \delta_i^I} &= (1-p_i^R)p_i^s\frac{\partial EV_i}{\partial \delta_i^I} < 0 && (15)
\end{aligned}
$$

with $\frac{\partial EV_i}{\partial \delta_i^I} < 0$, see above. Higher realizations of δ_i^R raise the propensity for tax registration (cf. Equations (11) and (12)), while higher realizations of δ_i^S or δ_i^I lower it (cf. Equations (13) to (15)). Intuitively, higher values of δ_i^S and δ_i^I come with elevated tax compliance and tax payments after registration, which increases firms' incentives to stay off the business registry.

If firms with high realizations of δ_i^S fail to register for business tax purposes and are drawn into the tax net, they show superior tax compliance at the return submission stage. If firms with a high realization of δ_i^I fail to register for business tax purposes and are drawn into the tax net, they show superior tax compliance at the income reporting stage but inferior tax compliance at the return submission stage (cf. Equation (7)). If the draws of δ_i^R, δ_i^s and δ_i^I are independent, forcedly registered firms thus show elevated compliance behavior on the income reporting stage, conditional on return submission; their return submission compliance may, in turn, positively or negatively deviate from that of voluntarily registered firms (among others depending on the distribution of δ_i^I and δ_i^S draws and their impact on expected detection probabilities).

Selection Effects - Correlation of Detection Risk Across Compliance Stages

In real world settings, the realizations of δ_i^I, δ_i^S and δ_i^R for a given taxpayer may positively correlate, however. Businesses may have characteristics that drive their detection risk on all compliance stages. Large firms with many trading partners or firms located close to tax authority premises may face a high risk to become subject to tax authority's enforcement efforts on the registration, return submission and income reporting stage. Moreover, firm owners' expectation on their non-compliance detection risk may systematically deviate upwards or downwards from actual risk on all compliance stages. Appendix A presents a model version, where δ_i^R, δ_i^s and δ_i^I are a function of a firm-specific common shifter δ_i.

The model shows that forcedly registered firms may behave more or less tax compliantly than voluntarily registered entities, both on the return submission stage and on the income reporting stage. On the one hand, lower realizations of δ_i now reduce firms' expected non-compliance detection risk on all compliance stages, making non-compliance more attractive on all stages. Firms with a low δ_i-realization are thus less likely to register for tax purposes and also behave systematically less compliantly than voluntarily registered firms on later compliance stages when drawn into the tax net. Analogously to the prior section, firms with a low realization of δ_i, on the other hand, care less about ending up on the return submission and income reporting stage as their low δ_i-realization allows for non-compliance on these stages. In consequence, they register for tax purposes at elevated rates, implying that forcedly registered firms - if drawn into the tax net - behave more compliantly than voluntarily registered entities. Theoretically, it is thus unclear whether the compliance behavior of forcedly registered firms deviates upward or downward from that of voluntarily registered firms.

Selection Effects - Firm Size and Non-Registration

Firm selection may not only drive a wedge between the post-registration compliance behavior of forcedly registered and voluntarily registered firms, it may also imply that the two groups of taxpayers systematically differ in firm size and true underlying income. This is obviously of relevance for the revenue consequences of registration enforcement interventions: If large firms with high underlying income are drawn into the tax net, the revenue gains are larger than if the identified non-compliers tend to be small. The appendix presents a model version, where we allow for size differences across firms. In line with prior research, a key model ingredient is that non-compliance detection risk is assumed to increase in firm size (see e.g. Ulyssea, 2018, Kumler et al., 2020).

The findings suggest that it is theoretically unclear whether small or large firms select into tax non-registration. On the one hand, a larger firm size increases the risk that non-compliant behavior is detected by the tax authority, thus raising the propensity that firms register for tax purposes. On the other hand, larger firms earn higher underlying income and, if tax compliant, therefore owe larger tax liabilities. This diminishes their incentive to register for tax purposes. On top of that, effects as described above apply: firm size raises non-compliance detection risk on later stages and thereby diminishes incentives to register for tax purposes.[9] While empirical research suggests that firms in

[9] Analogous incentives apply for the decision to submit a tax return or not.

the informal sector are systematically smaller than firms in the formal sector (implying that the first effect dominates, see e.g. La Porta and Shleifer., 2008, La Porta and Shleifer., 2014), it is unclear whether the same holds true for the semi-formal (i.e. incorporated) firms versus fully formal (i.e. incorporated and tax-registered) firms in our empirical application.

Treatment Effects on Other Taxpayers

So far, the analysis focused on revenue gains related to tax liabilities of businesses drawn into the tax net by the registration enforcement intervention (cf. Equation (1)). But the behavior of other firms - those that voluntarily registered for tax purposes or those that are founded at a later point in time - may also be affected by the treatment. Business owners may interpret the enforcement intervention as a signal of increased enforcement activity at the registration stage. Formally, this corresponds to a rise in δ_i^R and p_i^R. Firms may thus voluntarily register for tax purposes at increased rates after the intervention (cf. Equations (11) and (12)). But the inverse may also apply. With fixed tax authority resources, administration and compliance assessment of forcedly registered firms binds authority resources that would have otherwise been devoted to other taxpayers, reducing the risk of each individual firms that non-compliance at the registration stage is detected. Formally, this corresponds to a decrease in δ_i^R and p_i^R and a decline in firms' propensity to voluntarily register for tax purposes (cf. Equations (11) and (12)).

There may, moreover, be spillovers on other compliance stages. If existing taxpayers interpret the interventions as a signal for a general increased enforcement capacity, δ_i^S and δ_i^I rise. If they expect audit resources to be diverted from other compliance stages to registration enforcement activities or if they focus on congestion effects, δ_i^S and δ_i^I decline. The effect on submission behavior and income reporting is thus theoretically unclear.

Against this theoretical background, we embark on the empirical analysis. Testing ground are two registration enforcement interventions of the South African tax authority that snapshot synchronized its business tax and commercial registry. In the following, we will determine 1) how many taxpayers were drawn into the tax net by the interventions; 2) how much additional revenue was raised by the forcedly registered taxpayers (see Equation (1)) and 3) whether other taxpayers responded to the intervention (did they register for tax purposes at adjusted rates? did they adjust their return submission behavior and income reporting?).

3 Institutional Background and Data

Testing Ground: South Africa

Our empirical testing ground is South Africa. The country is an upper-middle-income economy with a gross domestic product per capita of $5,744$ US dollars in 2015. Its tax-to-GDP ratio exceeds that of other less developed countries (29.0% in 2015 relative to a 19.1% average for the African continent), but still falls short of developed-country levels (OECD average: 34.0%). Similar to other less developed economies, corporate taxes are an important revenue source, as indicated by a corporate-tax-to-GDP ratio of 4.7% relative to a 2.7%-average in the OECD.[10]

Firms in South Africa are subject to business taxes levied under the Income Tax Act 58 of 1962. Business income is taxed at a proportional company tax rate of 28%. Small Business Corporations (SBCs) - among others, characterized by gross income of less than 20 million rand - are subject to a progressive corporate tax scheme with lower tax rates.[11] Business taxes are levied on incorporated firms only; owners of non-incorporated firms are obliged to include business income in their personal tax return. To incorporate, firms need to register with the commercial registry at the Companies and Intellectual Property Commission (CIPC). Incorporation offers several benefits, including limited liability of owners, facilitated access to external capital and allowing for additional transactions with other formal businesses.[12] There are no minimum capital requirements. All firms on the CIPC registry are subject to business taxation and are required by South African tax law to register with the South African Revenue Service for business tax purposes within 21 days from the CIPC registration, irrespective of their size or taxable income. Businesses then have to submit tax returns for every tax year in which they are active. After the close of the tax year (commonly at the end of February), returns are required to be submitted with SARS within 365 days.

Tax compliance is enforced by audits and fines on all compliance stages. Non-registration and late submission of returns is subject to monthly fines of 250-16,000 rand, depending on taxpayers' income. Misreporting of taxable income, conditional on return submission, triggers understatement penalties, where additional tax up to 200% may be imposed, depending on the severity of the case.

[10]Information on GDP per capita was obtained from the World Bank, and information on tax-to-GDP ratios from OECD statistics.

[11]Small firms can also opt to be taxed as micro businesses, in which case they face special tax dispensation in the form of a turnover tax. To be eligible for turnover taxation, firms' gross income must not exceed 1 million South African rand and total assets must not exceed 5 million South African rand. The scheme is perceived as unattractive, however; within our analyzed sample frame, a negligible number of firms opted for turnover taxation.

[12]Procurement policies, for example, regulate that only CIPC registered firms can compete for business contracts of certain size.

SARS Interventions

Our empirical analysis will assess two administrative interventions where SARS compared its business tax registry with South Africa's commercial register. The commercial registry comparisons were conducted in early 2008 and early 2014, and were organised internally, i.e. there was no media coverage prior or after the intervention.

All firms in the commercial register that were identified as non-compliant with their obligation to sign up with SARS for business tax purposes were added to the business tax registry in April 2008 and February 2014. They received a letter informing them that they were registered with SARS for business tax purposes based on their commercial register entry and that they were required to submit tax returns for all tax years from their date of incorporation onwards and for subsequent years to come.

Data for the Empirical Analysis

To assess effects of the interventions, we rely on SARS's current and historic business tax registry which includes information on firms' registration and deregistration dates with SARS, coupled with data on the registration date with CIPC. The business tax registry, on top, includes further baseline data, most importantly on the tax office responsible for the taxpayer and on firms' industry.[13]

In addition, these data are augmented by information on the population of business tax returns for the tax years 2009 to 2014, comprising information on reported taxable income, firms' tax payments as well as sales, costs and assets.

4 Empirical Analysis

The empirical analysis consists of two parts. Part 1 investigates the direct effect of the registry comparison and determines how many firms are drawn into the tax net by the interventions (Section 4.1.1), how many tax returns these firms submitted after their registration (Section 4.1.2) and how much taxes they owe (Section 4.1.3). Part 2 tests for indirect effects on other taxpayers and quantifies the impact of the intervention on *voluntary* tax registrations at SARS (Section 4.2.1) and on the tax return submissions and taxable income reporting of non-targeted firms (Section 4.2.2).

[13]The industry information is only available for firms that submitted a tax return at least once.

4.1 Direct Effects

4.1.1 Tax Registrations

The registry comparisons in April 2008 and February 2014 drew 274,822 and 311,378 firms into the business tax net that had registered with CIPC but not with SARS (within the given legal time frame of 21 days).[14] This corresponds to an expansion of the tax net by 11% and 8% at the time of the interventions. Figure 1 illustrates the evolution of new registrations and the total number of firms on SARS's business tax registry between 2007 and 2014, showing that the comparisons caused, by far, the most significant registration spikes within the data frame.[15]

The costs for bringing the additional firms into the tax net were small: Tax authority outlay largely related to contacting the forcedly registered taxpayers and informing them about their registration for business tax purposes and requesting the submission of tax returns for all tax years since CIPC registration.[16] In the course of both registry comparisons, SARS sent out letters to all forcedly registered entities, at a cost of approximately 5 Rand each. The intervention's direct revenue gains amount to the tax liabilities submitted by the identified firms (cf. Equation (1)) - at least in cases where the firms would have never voluntarily registered with SARS for business tax purposes. Even if they had, revenue gains are not necessarily zero: Forcedly registered firms might submit tax liabilities earlier than in the absence of the intervention, they might submit more returns or might adjust reported tax liabilities.

Drawing on observed voluntary registration behavior, we determine what fraction of the identified registration non-compliers would have voluntarily registered for business tax purposes in the absence of the intervention and when. In doing so, we focus on the registry comparison in April 2008. Voluntary registration behavior is modeled from firms' observed registrations at CIPC and SARS between January 2000 and March 2008. For each month between January 2001 and March 2008, we observe the number of new firm registrations with CIPC. For the period between January 2007 and March 2008, we additionally observe all voluntary registrations for business tax purposes at

[14] For the 2008 (2014) registry comparison, we disregard firms that had registered with CIPC in March/April 2008 (January 2014) when calculating the number of firms drawn into the tax net (as well as their tax return submissions and tax liabilities), given that they had not yet missed the deadline for on-time registration with SARS at the time of the intervention.

[15] Note that there is a smaller spike in registration numbers in July 2009. This likely relates to changes in the tax treatment of venture capital shares in July 2009. Precisely, from that month onwards, investors were allowed to claim amounts incurred on acquiring venture capital shares as a deduction from their taxable income (see SARS's External Guide for Venture Capital Companies).

[16] Costs for incorporating the commercial registry entries into the business tax registry cannot be pinned down precisely but were arguably small. Note, moreover, that it is common practice to ignore fixed setup costs in cost-benefit-analyses (see e.g. Brockmeyer et al., 2019).

SARS, with information on firms' date of registration with SARS's business tax registry and their date of registration with CIPC.

This allows us to obtain an estimate for the propensity of firms to register with SARS with a given lag from their CIPC registration. For each month between January 2007 and March 2008, we determine the propensity to register with SARS with a lag of ℓ months from CIPC registration as the ratio of firms that registered with SARS for business tax purposes in that month having registered with CIPC ℓ months before over the total number of firms that registered with CIPC ℓ month ago.[17] The propensity to register with SARS with an ℓ-month lag $\hat{\alpha}_\ell$ is then determined as the unweighted average of these ratios: $\hat{\alpha}_\ell = \frac{1}{M}\sum_{m=1}^{M} \alpha_{m,\ell}$ with m indicating the months between January 2007 and March 2008 and ℓ indicating the registration gap, where $M = 15$ and $\ell \in \{2, 99\}$.[18] The results suggest that 49.9% of firms register with SARS on time (in the month of CIPC registration or the month thereafter). 24.3% register with a gap of at least two months, 8.7% with a gap of at least 12 months. 25.8% never register with SARS. Figure 3 graphically depicts $\hat{\alpha}_\ell$ (for $\ell > 1$), showing that the propensity to register with SARS sharply declines in ℓ and quickly converges to zero.

As a direct implication, forcedly registered firms that tend to be old at the time of the intervention (measured by their date of CIPC registration) are predicted to have a negligible propensity to have voluntarily registered with SARS at a later point in time. Inversely, firms that tend to be young would have voluntarily registered at non-negligible rates. Figure 2 depicts the actual age distribution of identified non-compliers. We calculate the number of forcedly registered entities that would have voluntarily registered with SARS in the absence of the intervention for each post-intervention month. For May 2008, the predicted voluntary registrations are $\hat{R}_{5/2008} = \sum_\ell \hat{\alpha}_\ell C_{5/2008-\ell}$, where $\hat{\alpha}_\ell$ is the propensity to register with SARS ℓ month late (see above) and $C_{5/2008-\ell}$ is the number of firms that registered ℓ month prior to May 2008 with CIPC. For the whole considered time period, the total number of firms that would have voluntarily registered with SARS in the absence of 2008 intervention reads $\hat{R}_{ALL} = \sum_m \sum_\ell \hat{\alpha}_\ell C_{m-\ell}$, with m indicating the months between April 2008 (the month of the 2008 registry comparison) and January 2014 (the month before the 2014 registry comparison), $m \in \{04/2008, 05/2008, 06/2008....01/2014\}$. [19]

[17] In January 2007, e.g., 792 firms registered with SARS for business tax purposes that had registered with CIPC in October 2006 and hence, on average, with a three month lag. In total, 24,683 firms had registered with CIPC in October 2006. The propensity for CIPC registered firms to register with SARS with a 3-month-lag is therefore 3.2%. Analogously, the propensity to register with a 3-months-lag can be calculated as the ratio of SARS business tax registrations in any other month between February 2007 and March 2008, related to CIPC registrations three months before.

[18] Note that 99 months is the maximum registration lag observed in our data.

[19] Note that expanding the time frame beyond January 2014 does not significantly change $\hat{R}_{ALL}$ (as the propensity to voluntarily register with SARS several years after the date of CIPC registration

The cumulative distribution of the identified non-compliers that would have voluntarily registered with SARS is plotted in Figure 4. The red line indicates the actual number of business taxpayers registered with SARS in the 2008 registry comparisons. The black solid line indicates the cumulative number of identified non-compliers that would have voluntarily registered with SARS at a later point in time in the absence of the intervention. Confidence intervals are bootstrapped. The figure shows that around one quarter of the identified non-compliers would have voluntarily registered with SARS later in time, most of them between 2008 and 2011. The intervention thus drew a significant number of taxpayers into the tax net. For the remaining firms, registration timing was shifted forward, in some cases by several years.

One potential caveat of this analysis is that we approximate the propensity to have voluntarily registered with SARS after 2008 by observed registration behavior prior to the 2008 intervention. If registration behavior changes over time, this poses a threat to the strategy. While many determinants of tax compliance in general and tax registration behavior in particular hardly vary within moderate time frames - including e.g. social norms and institutional settings - economic conditions and business environments may change and may alter registration rates. Figure A1 in the appendix shows that South Africa experienced a moderate economic downswing in the years after the 2008 registry comparison with moderately rising unemployment rates. If less favorable business environments are associated with lower business tax registration rates, the voluntary counterfactual business tax registrations depicted in Figure 4 are an upper bound to the true effect. To account for this possibility, we use our data to estimate the link between business conditions and registrations for business tax purposes in Appendix A. This is used to obtain an adjusted estimate for the number of firms that would have voluntarily registered for business tax purposes in the absence of the intervention. See Appendix A for details. The results are depicted in Figure A2 and show results comparable to the baseline estimate.

Complementarily, we assess the importance of time-varying changes in business taxpayer registration rates by drawing on information on voluntary registration behavior of firms that registered with CIPC after the 2008 intervention. To dampen concerns that these estimates might be affected by the 2008 intervention itself, we focus on areas which were weakly treated by the 2008 intervention. See Appendix A for details. The resulting cumulative number of voluntary business taxpayer registrations in the absence of the 2008 intervention is depicted in Figure A3. Consistent with the analysis before, it suggests that a moderate number of firms would have voluntarily registered

is negligible, see Figure 3). The calculation of R_{ALL}, furthermore, only accounts for registration gaps smaller than 99 months; as for large ℓ, registration propensities turn out to be negligible anyway, we consider this to be a mild assumption.

with SARS after the intervention.

4.1.2 Return Submissions

Next, we study firms' post-intervention behavior and revenue contributions. As sketched in Equation (1), the revenue consequences of enforcement interventions directly hinge on the tax return submission behavior of forcedly registered firms. Business tax returns are submitted at low rates in many countries (cf. Figure 1 in Brockmeyer et al., 2019) and South Africa is no exception. In our data for the tax years 2009 to 2014, the propensity that voluntarily registered firms submit a tax return in an active tax year is 27.3%. While our definition of 'active tax year' accounts for deregistrations from the business tax registry, observed deregistration rates tend to be low, suggesting that frictions may prevent some firms that go out of business from deregistering with SARS. Tax non-compliance, nevertheless, appears to be the main driver of the small return submission rate: When focusing on voluntarily registered firms' first liability year after SARS registration - where firms are active for sure - the average return submission propensity remains a small 35.7%.[20] In line with Carrillo et al., 2017, this highlights that knowledge about tax non-compliance - in our case, failed return submissions - is not sufficient for authorities to rectify it. When authority resources are scarce, enforcement may still be prohibitively costly. This contrast developed economies, where (third-party) information is seen as a key element of modern tax enforcement (see e.g. Kleven et al., 2011). In any case, low levels of return submission compliance reduce the revenue effectiveness of registration enforcement interventions.

As sketched in Section 2, systematic selection of firms into non-registration may, moreover, drive a wedge between the average post-registration compliance of forcedly and voluntarily registered firms, with the sign being theoretically unclear. We find that forcedly registered firms submit tax returns at even lower rates than voluntarily registered entities. The return submission propensity in active years is 9.9% (8.15%) for firms identified in the 2008 (2014) registry comparison, thus being low in tax years *after* firms' SARS registration (as evident from the submission behavior of firms identified in the 2008 registry comparison) and in tax years *before* firms' SARS registration (as evident from the submission behavior of firms identified in the 2014 registry comparison, which are obliged to submit returns for tax years since their CIPC registration).

Comparing forcedly and voluntarily registered firms that signed up with CIPC within the same time frame (2001-2008 for the registry comparison in 2008 and 2009-2013

[20] The reported descriptive statistics are derived for all firms that registered with CIPC after 2007, apart from those identified in the registry comparisons in 2008 and 2014. The tax return years considered are 2009 to 2014.

for the registry comparison in 2014) yields a statistically significant gap in return submission propensities of 15 (20) percentage points - or 61% (72%) - for the registry comparisons in 2008 (2014), cf. Figure 5. This gap does not root in observed firm characteristics: Controlling for firms' year of CIPC registration, the tax return year and the host region leaves the estimate largely unchanged (cf. Tables B1 and B2 in the appendix). The estimated gap also remains unaffected when the sample is restricted to firms that were active at the time of the registry comparison and in the considered tax year (cf. Specification (3) in Tables B1 and B2; for the 2014 registry comparison, the analysis is restricted to firms that registered with CIPC just before the intervention (between February and December 2013) and compares registration rates of voluntarily and forcedly registered firms for in their first tax liability year, where firms were thus active for sure; analogously for the 2008 registry comparison).[21]

As suggested in the prior section, some of the forcedly registered firms may have voluntarily signed up with SARS at a later point in time and may have submitted tax returns. Some of the returns received by forcedly registered taxpayers - in total, 157,946 for the 2008 registry comparison and 64,406 for the 2014 registry comparison, across all tax years 2009 to 2014 - would have also been received in the absence of the intervention. Moreover, within the group of forcedly registered firms, return submission rates may deviate between firms that would and would not have signed up for business tax purposes voluntarily at a later point in time. We draw on observed return submission behavior to model return submissions of the forcedly registered firms in the absence of the intervention. The analysis again focuses on the 2008 registry comparison.

Formally, the number of returns submitted by taxpayers in the absence of the intervention reads: $\hat{S}_{ALL} = \sum_m \sum_\ell \hat{\alpha}_\ell C_{m-\ell} \sum_t \hat{p}^s_{\ell,\tilde{t}}$, where the definition of $\hat{\alpha}_\ell$ and $C_{m-\ell}$ follows the prior section, t indicates the tax years $t \in \{2009, 2010, ...2014\}$ and $\tilde{t} = m - t$ the position of the tax year relative to the year of SARS registration. $\hat{p}^s_{\ell,\tilde{t}}$ is the propensity that firms, which registered late with SARS by ℓ months, submit a tax return for the liability year $\tilde{t}$.[22]

$\hat{p}^s_{\ell,\tilde{t}}$ is calculated from observed return submission behavior of firms that *voluntarily* registered with SARS between January 2007 and January 2014.[23] The base analysis

[21] This ensures that firms are active in the considered tax year and at the time of the registry comparison (thus receiving SARS's letter and the request for tax return submissions). Note that, while restricting the sample to active firms in Specification (3) of Tables B1 and B2, does not affect the gap in registration rates between voluntarily and forcedly registered firms, it increases the return submission rates in both groups.

[22] The latter allows the propensity to submit a tax return for the first liability year after SARS registration to differ from the propensity to submit a tax return in the third year of SARS registration - or in case firms are registered sufficiently late - in the liability years prior to SARS registration).

[23] The analysis hence disregards firms that were forcedly registered with SARS in the course of the 2008 registry comparisons. Furthermore note that January 2014 is the month prior to the second

determines $\hat{p}^s_{\ell,\tilde{t}}$ as the average propensity of firms with a given gap between CIPC and SARS registration to submit a tax return in a tax year with a position $\tilde{t}$ relative to SARS registration. We account for $\tilde{t} \in [-5,\ 6]$: $\tilde{t}$ is largest for firms that would have voluntarily registered with SARS right after the intervention and are still active in 2014 ($\tilde{t} = 6$); and it is smallest for firms that would have voluntarily registered with SARS in 2014 in the first observed tax return year 2009 ($\tilde{t} = -5$). To increase power, $\hat{p}^s_{\ell,\tilde{t}}$ is calculated for groups of taxpayers with a CIPC-SARS-registration gap of less than 6 months, 6-18 months, 18-24 months and so on (in half year steps).

Figure 6 presents estimates of the return submission propensity $\hat{p}^s_{\ell,\tilde{t}}$ for the first liability year after SARS registration for firms with different registration lags as defined above. The graph suggests that the propensity of late registered firms to submit a tax return is substantial and does not fall short of firms that registered with CIPC on time.[24] The propensity to submit a return does also not systematically vary across firms with different gaps between SARS and CIPC registration. Firms that register late with SARS by as much as 4 or 5 years have a comparable propensity to submit a tax return in their first liability year after SARS registration as firms which registered on time. Analogous patterns emerge for other liability years. [25]

Figure 7 plots the cumulative number of returns submitted by the non-compliers identified in the 2008 registry comparison for the tax years 2009 to 2014 (red line) and the cumulative number of returns that would have been submitted by these firms in the absence of the registry comparison (black line). Confidence intervals are bootstrapped. The figure suggests that a significant number of the received tax returns would have also been submitted without the intervention: 135,396 out of the actually received 157,946 tax returns. The increase in return submissions is, nevertheless, statistically significant, albeit moderate in size.[26]

registry comparison in February 2014.

[24]The latter firms' propensity to submit a return in the first year after SARS registration is 35.7%.

[25]Registration propensities are thus largely unaffected by the registration gap ℓ; they tend to decline (increase) in $\tilde{t}$ for $\tilde{t} > 0$ ($\tilde{t} < 0$, however). Note that the analysis relies on all firms that voluntarily registered with SARS and are observed in our data - that is, voluntary registrations between January 2007 and January 2014. Firms that registered with SARS between January 2007 and March 2008 may thereby have very long registration gaps to their CIPC registration, going back as fas as the year 2000 (see the analysis in the previous subsection); for them, we observe the return submission behavior for the first or second post-SARS registration year onwards. For firms that voluntarily registered with SARS between May 2008 and January 2014, registration gaps to CIPC registration are naturally shorter (reflecting that all firms registered with CIPC but not with SARS were forcedly registered in April 2008), but the gap can amount to as much as 68 months. Depending on the time of SARS registration, we observe return submission behavior of these entities in tax years pre and post SARS registration.

[26]Put differently, this suggests that taxpayers who were registered with SARS through the 2008 registry comparisons and would have not voluntarily registered for tax purposes in the absence of the intervention have tax return submission propensities that fall significantly short from the return submission propensity of all non-compliers identified in the 2008 intervention. See also Figure B1.

One concern might be that the return submission behavior of identified non-compliers in the absence of the intervention is estimated from observed behavior of voluntarily late registered firms after the intervention. The obvious threat to this strategy is that return submission behavior of the latter firms might be affected by the intervention. Even non-targeted taxpayers may respond to the intervention and revise expected audit propensities and therefore behavior (see Section 2). Our data suggests that this is not a material threat: We test for related effects below and find no significant response of non-targeted taxpayers' tax return submission behavior or tax reporting to the intervention. Complementarily, we follow the previous section and determine $\hat{p}^s_{\ell,\tilde{t}}$ from firms in weakly treated areas, i.e. areas where not many firms were identified to be non-compliant with their SARS registration requirement by the 2008 intervention. This leaves results largely unchanged, see Figure 8. Figure 8, moreover, presents the results of robustness checks where $\hat{p}^s_{\ell,\tilde{t}}$ is determined in models which explicitly account for year specific differences in return submission behavior. [27]

Note, moreover, that it follows that those forcedly registered firms, which would have never voluntarily signed up with SARS in the absence of the intervention, on average, exhibit lower return submission propensities than all forcedly registered firms (reflecting that the average return submission propensity of the whole group is a weighted sum of the return submission propensities of firms that would and would have not voluntarily signed up with SARS and the former entities' return submission propensity is quite high, cf. Figure 3). The analysis yields that forcedly registered firms that were drawn into the tax net and would have never voluntarily registered submit returns in around 5% of active tax years, see Figure B1, relative to return submission rates of around 9% for the whole group of forcedly registered enterprises.

4.1.3 Tax Revenues

Next we assess the tax revenue impact of the reform. For the tax years 2009 to 2014, SARS received tax liabilities of 1.76 billion South African Rand from firms identified as non-compliant in the 2008 registry comparison and 98.6 million South African Rand from firms identified as non-compliant in the 2014 registry comparison. This corresponds to about 0.21% of the overall business tax revenue during that time period. The

[27] Note that time-specific changes may change the return submission behavior of a given firm but may also change the firms that are registered with SARS and liable for submitting a tax return (see first part of the analysis). If economic conditions deteriorate, less less firms may register for tax purposes. If these adjustment change the composition of the registered firms that may impact the constructed return submission rates. Note, however, that analogous effects would have arisen for the firms identified as non-compliant in the 2008 treatment in the absence of the intervention. It is thus appropriate to them in the construction of the submission propensities.

average tax payment per firm and tax year (conditional on return submission) was 10,634.1 South African Rand for taxpayers identified as non-compliant in the 2008 registry comparison and 1443.7 South African Rand for taxpayers identified in the 2014 registry comparison.[28] Revenues were received from a limited number of taxpayers, however: Out of all non-compliers identified in the 2008 (2014) intervention, only 2.9% (0.5%) reported positive tax liabilities during our sample period.[29] Even conditional on reporting non-zero taxes, tax payments are strongly concentrated: The 10% largest non-compliers - as measured by the sum of tax liabilities across all observed tax years - account for 86% (79.1%) of the revenue raised from all non-compliers identified in the 2008 (2014) registry comparison.

The average tax payments of non-compliers identified in the registry comparisons, moreover, falls significantly short from tax payments of voluntarily registered firms. When comparing non-compliers identified in the 2008 intervention to broadly similar other firms - entities, which equally registered with SARS between 2001 and 2008 - their tax payments are by 80% smaller (cf. Figure 9 and Specification (1) of Table C1 in the appendix). Conditioning on the year of CIPC registration reduces this gap to 50% (cf. Specification (2) of Table C1). Accounting for further observed characteristics - the tax return year, the tax authority district and industry (cf. Specification (3) of Table C1) - does not significantly alter the estimate, suggesting that these characteristics do not add to the observed tax liability gap.

A significant part of the gap is, in turn, explained by size differences across firms (cf. Specification (4) of Table C1). Qualitatively and quantitatively similar results are, moreover, derived in PPML models and when assessing differences in the propensity for positive tax payable. Also note that taxpayers identified as non-compliant in the 2014 registry comparison report significantly less tax payable than other voluntarily registered firms. See Table C3 in the Appendix, As the prior section suggested that many tax returns would have been submitted even in the absence of the interventions (by firms that would have registered late with SARS), we would expect that the behavior of forcedly registered firms correlates with the behavior of voluntarily late registered firms. This is confirmed in Figure C1 in the appendix.

Given that the majority of returns would have also been submitted in the absence of the intervention (see the prior subsection), the same may hold true for the tax revenue received. We again focus on the 2008 registry comparison to disentangle liabilities that

[28]The difference in tax payments of non-compliers identified in 2008 and 2014 likely roots in the fact that the latter firms are younger and may, therefore, be smaller, less profitable and potentially less compliant on average.

[29]The fraction of firms with positive tax liabilities is smaller for the 2014 comparison, likely reflecting that the latter entities are younger.

would have been submitted anyway from additional liabilities received because of the intervention. Formally, the tax liabilities that SARS would have received from the identified non-compliers in the absence of treatment read $\hat{T}_{ALL} = \sum_m \sum_\ell \hat{\alpha}_\ell C_{m-\ell} \sum_t \hat{p}^s_{\ell,\tilde{t}} \hat{T}_{\ell,\tilde{t}}$, where the variable definition corresponds to the prior sections and $\hat{T}_{\ell,\tilde{t}}$ is the tax liability in year $\tilde{t}$ of firms that registered ℓ months late. To determine $\hat{T}_{\ell,\tilde{t}}$, we again turn to observed behavior by voluntarily registered taxpayers after their registration with SARS. In the base analysis, we determine this propensity separately for firms that are late registered with SARS by a given number of months (again grouping firms that are late registered by less than 6 months, 6-12 months, 12-18 months etc.) and the position of the liability year relative to the year of SARS registration. To mitigate the effect of outliers, we winsorize the taxable income variable at the 5% level.

Figure 10 depicts the cumulative tax revenue received from identified non-compliers in the 2008 intervention by tax return year. The left panel indicates the number of tax returns with non-zero tax liabilities, the right panel the actual revenue received. In both figures, the red line marks the actual number and value of positive tax liabilities (in the latter case calculated based on the winsorized tax payable variable). The black line shows the predicted number and value of non-zero tax liabilities received from voluntary return submissions in the absence of treatment. Standard errors are bootstrapped. The analysis indicates that a large fraction of the positive tax liability returns would have also been received in the absence of the intervention (84.2%). The intervention-driven increase in the number of positive liability returns is nevertheless statistically significant, amounting to 2758 additional positive liability returns received. Analogously, the right hand panel shows that a significant fraction of the received tax liabilities would have also been submitted in the absence of the intervention (94.4%). The additional revenues received because of the intervention amount to 98.5 million South African rand (=1760 rand $\cdot$ 0.056; in US dollars: 8.5 million) and turn out marginally statistically significant. [30]

Analogously to the previous sections, we, moreover, run several robustness checks. We first account for potential variation in return submission propensities and tax liabilities over time (in addition to the CIPC registration gap and the position of the liability year relative to firms' SARS registration) when calculating $\hat{p}^s_{\ell,\tilde{t}}$ and $\hat{T}_{\ell,\tilde{t}}$. Moreover, we calculate $\hat{p}^s_{\ell,\tilde{t}}$ and $\hat{T}_{\ell,\tilde{t}}$ only using taxpayers in weakly treated areas. The results are presented in Figure 11 (again showing the number of positive tax liabilities in the left hand panel and the value of tax revenue submitted in the right panel). In both

[30] When determining which fraction of tax revenues would have been received in the absence of the intervention, we mitigate the role of outliers by winsorizing firms' tax liabilities at the 5% level, both when calculating the actual tax revenue received (red line in Figure 10) and when determining the tax payments of identified non-compliers in the absence of the intervention (black line).

cases, the determined counterfactual number and value of positive tax liabilities in the absence of the intervention is somewhat smaller than in the base estimate; and statistically different from the actual number and revenue received.

But increased tax revenues are not the only benefits from the intervention. In addition, forcedly registered taxpayers that would have voluntarily registered at a later point in time might respond to the interventions by submitting tax returns and tax liabilities earlier than in the absence of the interventions. Given the high interest rate on South African government bonds, earlier receipt of tax revenues comes with non-negligible advantages for the public sector.

To capture timing effects, we calculate the net present value of the actual tax revenues received by SARS from non-compliers identified in the 2008 intervention. To do so, we exploit that we observe the exact day of submission of tax returns[31]; assuming an interest rate of 9% - which corresponds to the average yield on 10-year South African government bonds during our sample period -, tax revenues are discounted to September 2008, corresponding to the first month, in which non-compliers identified in the 2008 intervention submitted tax returns. To mitigate the effect of outliers, tax liabilities are, again, winsorized at the 5% level.

We, moreover, follow the prior analyses and determine the timing of tax return submissions of non-compliers identified in the 2008 registry comparison in the absence of the intervention by the behavior of voluntarily late registered firms during our sample period. In doing so, we again differentiate between firms with different registration gaps (in half year steps) and account for the position of the considered tax year relative to SARS registration. The results suggest that the discounted sum of actual tax liabilities is by 22% larger than the discounted value of the sum of tax liabilities that would have been received in the absence of the 2008 intervention (with the difference being statistically significant at the 5% level).

Forcedly registered firms, which would have voluntarily registered at a later point in time, thus indeed submit returns earlier than in the absence of the intervention: This, first, follows from the fact that the difference in the discounted actual and counterfactual revenue receipts is 2.5 times larger than the undiscounted difference in actual and counterfactual revenue receipts - suggesting that the gap in actual and counterfactual discounted revenues cannot fully be explained by the additional tax liabilities received, but in part reflects adjustments in the timing of payments that would have also been received in the absence of the intervention. This interpretation is corroborated when comparing the return submission timing of forcedly registered firms with that of firms,

[31]Unfortunately, we do not observe when firms actually made tax payments to SARS. In the following, we will assume that the date of return submission corresponds to the date when tax payments are made.

which voluntarily registered on time and those which registered late, see Figure 12.[32] The figure shows the kernel density of the day gap between the submission date of tax returns and the day due for forcedly registered entities in 2008 to 2014 relative to voluntarily registered firms that had signed up with CIPC in the same time frame (2001-2008 for the 2008 intervention and 2009-2013 for the 2014 intervention). The graph indicates that non-compliers identified in the registry comparisons submit tax returns later than firms which registered on time with SARS. The former group exhibits significantly less (more) probability mass on negative (positive) values for the difference between the due date of the return and the actual submission date. Interestingly, this also holds true for the 2008 comparison: While late submission of returns of non-compliers identified in the 2014 intervention (where our tax return sample reflects tax years prior to SARS registration) may be a direct consequence of late registration, this is not true for non-compliers identified in the 2008 intervention. For them, the registration with SARS is about two years prior to the due date of the first return observed. This therefore suggest that they choose to submit returns later than other entities, even if already registered. Additional analyses, moreover, suggest that forcedly registered firms submit tax returns systematically earlier than *late* registered firms, which is in line with the notion that they responded to the reform by submitting tax liabilities earlier than in the absence of the intervention. [33]

4.2 Indirect Effects

The prior section assessed the direct effect of the interventions. Several insights emerged. A large number of new taxpayers were added to the business tax registry in the course of the registry comparisons (most of whom would *not* have registered voluntarily with SARS later in time). The identified non-compliers, however, only submit a moderate number of tax returns, many of which would have also been submitted in the absence of the intervention (by firms that would have voluntarily registered with SARS at a later point in time). Only a few of the identified non-compliers make positive tax

[32]Specifically, for each tax year, we determine the date on which the submission of a tax return is due with SARS. For the large majority of firms (around 85%), the tax year runs from March 1 to February 28 in the following year. Return submissions are due within one year. That is, the returns for the tax year 2009/2010, denoted by 2010 in our analysis, are due on February 28, 2011. For all firms and returns received by SARS for the tax years 2009 to 2014, we calculate the difference (in days) between this due date and the actual date when the return was received by SARS.

[33]Note that these post-intervention differences may root in selection of taxpayers into registration and non-registration with SARS but may also reflect effects of the treatment on voluntarily registered firms and forcedly registered entities. Additional analyses below reject significant effects on voluntarily registered entities, suggesting that the observed gap in behavior is driven by selection and treatment effects on forcedly registered firms. Treatment effects on forcedly registered firms are likely positive, pointing to earlier tax return submissions, suggesting that the observed late submission pattern likely roots in selection effects.

payments and the overall revenue impact of the intervention is moderate. There is, in turn, evidence for significant timing responses and benefits related to earlier tax return submissions by the identified non-compliers than in the absence of the intervention.

Note, however, that our analysis disregards a number of other potential effects of the intervention. First of all, we do not observe business tax returns prior to the tax year 2009 or after the tax year 2014. Related tax payments, therefore, remain uncaptured. There may also be spillovers on other tax bases (non-compliers identified in the intervention may, e.g., register for and pay value added tax). And there may be non-monetary benefits to society (firm owners who pay taxes might, e.g., hold politicians accountable). Our data does not allow us to capture such effects - which are likely positive and add to the benefits of the interventions.

We can, however, assess whether the interventions exert indirect effects on non-targeted business taxpayers. The latter might change their compliance behavior after learning about the registry comparisons. The direction of the adjustment is a priori unclear. On the one hand, drawing a large number of new taxpayers into the tax net and administering them binds administrative capacity. With fixed authority resources, audit and detection risk of other taxpayers may decrease. On the other hand, agents may perceive the intervention as a signal of increased enforcement capacity and expect higher audit and detection risk. See Section 2. Moreover, learning about the interventions makes tax enforcement actions salient and, for this reason, may trigger more compliant taxpayer behavior by non-targeted firms.

In the following, we will test for related effects. The empirical identification strategy exploits that the treatment intensity varies across space, with some areas hosting many firms that were identified as non-compliant in the registry comparisons, while others host little. In areas with many treated firms, non-targeted agents are arguably more likely to learn about the registry comparisons from communication with affected neighbors than in areas with little affected firms as documented by a growing literature (Drago et al. (2020)).[34]

4.2.1 Spillovers on other Taxpayers: SARS Registrations

In a first step, we study effects of the interventions on voluntary business tax registrations at the South African revenue authorities. For this purpose, the territory covered by South Africa is divided in 300 meter-to-300 meter grids and firms are linked to these

[34]Taxpayers may also learn about the interventions through communication by SARS or media coverage. Note that media coverage of the events was scarce. Even in the presence of common effects (affecting all taxpayers in South Africa), taxpayer-to-taxpayer communication in local communities plausibly increases the awareness and salience of the intervention.

grids. For each grid, we determine the treatment intensity by the 2008 registry comparison as the ratio of the number of identified non-compliers in the grid over the number of firms in the grid which voluntarily registered with SARS in 2007. We then compare the monthly evolution of *voluntary* business tax registrations at SARS in strongly and weakly treated grids drawing on a standard difference in differences design. The base specification reads:

$$ONTIME_{gt} = \beta_1 \, TR_{gt} + \beta_2 X_{gt} + \delta_g + \gamma_t + \epsilon_{gt} \tag{16}$$

where $ONTIME_{gt}$ captures the fraction of firms in grid g that registered with CIPC in month t and complied with the legal requirement to register for business tax purposes within 21 days from CIPC registration; TR_{gt} is a dummy variable indicating the period after the 2008 intervention in grids that are strongly affected by the intervention. In the base analysis, we compare grids where the ratio of non-compliers identified in the 2008 registry comparison over all voluntary business tax registrations in 2007 is in the lowest and highest quartile of the distribution. δ_g and γ_t depict full sets of grid and month fixed effects. In robustness checks, we augment the set of regressors by grid size-month fixed effects (where grid size is measured by the number of firms in the grid) - and province-month-fixed effects, subsumed in X_{gt}; this absorbs size-specific and province-specific trends in voluntary registration behavior. β_1 captures the treatment effect of interest. Standard errors allow for clustering at the grid level.

The results are presented in Table 1 and Figure 13. In Specification (1), we regress the fraction of timely registered firms on TR_{gt} plus grid and month fixed effects. To avoid mechanical effects related to the 2008 intervention, we disregard CIPC registrations between February 2008 and April 2008. The results point to sizeable changes in registration behavior: After the intervention, the propensity that a firm registers on time for business tax purposes increases by 23 percentage points, on average, or 53% evaluated at the sample mean. The estimated effect is robust to adding grid-size-month-fixed effects (Specification (2)) and province-month-fixed effects (Specification (3)) and to adjusting the definition of weakly and strongly treated grids (Specification (4) compares grids where the 'treatment variable' - the ratio of non-compliers identified in the 2008 registry comparison over all voluntary business tax registrations in 2007 - is above and below the median; Specification (5) compares grids where the treatment variable is in the lowest vs. highest decile; Specification (5) interacts the post-dummy with the continuous treatment variable; Specification (6) adjusts the definition of the treatment variable and only accounts for non-compliers with long registration gaps (firms which registered with CIPC prior to 2007)).[35] Figure 13 presents differences

[35]Note that the relative size of the coefficients also matches with intuition. In particular, the

in time trends in on-time registrations between between strongly and weakly treated grids. Two insights emerge: first, there is a sharp and permanent jump in on-time registration rates at the time of the intervention; second, despite some variation, there is no significant difference in pre-trends. Even if the somewhat increased registration rates in strongly relative to weakly treated grids in early 2008 reflect anticipation effects of the 2008 intervention - which is unlikely given that there was no public information about the intervention - our estimated treatment effect is a lower bound to the true effect. Reestimating Specification (3) without pre-reform months in 2008, yields qualitatively and quantitatively comparable effects to the baseline model (coefficient estimate: 0.230, statistically significant at the 1%-level).

Note that the documented effect on registration behavior may reflect timing responses or level effects: Taxpayers may register with SARS earlier than before or they may decide to register at all. Timing responses can be assessed by studying changes in the ratio of firms that register with SARS early and late. In the following, we look at the relation of on-time registrations with SARS in grid g and month t and SARS registrations in the same grid and month that occur within 180 days from CIPC registration. To avoid picking up mechanical effects of the 2008 intervention on voluntary SARS registrations, we disregard registrations between April 2008 to October 2008.[36] The results are presented in Specification (1)-(3) of Table 2 (with the vector of regressors comprising TR_{gt}, grid and firm-month as well as province-month-fixed effects). In Specification (1), the dependent variable is the ratio of on-time registrations with SARS over registrations within 180 days from CIPC register entry. The estimate points to significant timing effects. After the intervention, on-time registrations - as a fraction of firms that register within 180 days - increase by 11.5% evaluated at the sample mean or by 19.8% of a standard deviation. Moreover, similar effects emerge in a specification where the dependent variable is the ratio of SARS registrations within 21 days over SARS registration within 365 days from CIPC registration (Specification (2)) and the ratio of SARS registrations within 180 over SARS registrations within 365 days from CIPC registration (Specification (3)).[37] In quantitative terms, the effect

estimated treatment effect becomes larger (weaker) when we compare grids in the first and tenth decile of the treatment intensity distribution (grids above and below the median).

[36] Intuitively, firms that were forcedly registered with SARS cannot voluntarily register at a later point in time. If registration gaps of up to 180 days days are considered, this may affect the defined ratio up to October 2008. Note that, contrary to Equation (16), the observational unit in this part of the analysis the grid-$SARS$ registration month. Thus, we compare early and late registrations with SARS in a given month. Similar results, however, emerge when we take the perspective of the CIPC registration month and, for the firms located in grid g that registered with CIPC in month t, determine the relation of firms that registered with SARS on-time and firms that registered within 180 days. The disadvantage of this specification relative to the one presented in the main text is that mechanical effects affect the pre-intervention period, which is considerably shorter than the post-intervention period in our analysis.

[37] To avoid mechanical effects, Specification (3) discards the registration months between April 2008

turns out smaller in the latter specifications: Here the intervention raises the propensity that firms register with SARS within half a year, conditional on registering within a year, by 2.6% evaluated at the sample mean or by 13.7% of a standard deviation. This suggests that timing responses tend to be larger among firms with smaller registration gaps (and tend to result in on time SARS registration).

We, furthermore, assess whether, next to timing effects, genuinely more firms sign up for business tax purposes after the intervention. To do so, we test for effects on registration numbers. The dependent variable is the inverse hyperbolic sine of registrations to avoid losing grids and months with zero SARS business tax entries. Pure timing responses imply that the number of early business tax registrations increases in strongly relative to weakly treated grids, while no significant effect shows up when late business tax registrations are also considered. Level effects, in turn, predict positive effects also in the latter case. This is tested in Table 3. Specification (1) shows that early, namely on-time business tax registrations strongly increase after the intervention (up by 11.9% in strongly relative to weakly treated grids); when the dependent variable captures the number of SARS registrations in a given grid and month within 180 days/365 days/730 days from CIPC registration, the effect is significantly more moderate (up by 3.9%/1.9%/1.1% after the intervention in strongly relative to weakly treated grids respectively, in the latter case not statistically significant at conventional significance levels), cf. Specifications (2)-(4).[38] Specification (5), moreover, assesses the possibility that less firms may register with *CIPC* in response to the intervention. Specifically, while CIPC registration involves benefits (e.g. related to limited liability, easier access to funding and increased opportunities to do business with other registered firms), it comes with the drawback that firms enter the government sphere. Tax non-compliance may thus be detected with a higher probability if tax authorities - as done in the context of SARS tax registry comparisons - make use of this information to enforce registration for tax purposes. After the snapshot registry comparison in 2008, firms may assign a higher probability of follow-up comparisons in the future (as know-how and infrastructure may have been set up at SARS, reducing the costs of further interventions). This may increase incentives of firms not to register with CIPC. To assess this possibility, we compare the number of CIPC registrations per months in strongly and weakly affected grids. The results, presented in Column (5) of Table 3, point to a negative and significant, albeit qualitatively moderate effect, suggesting that the 2008 intervention lowered CIPC registrations by 0.7%.

and April 2009 from the analysis. See also the previous footnote.

[38] Analogously to the previous specification, we avoid mechanical effects by dropping the SARS registration months April and May 2008 in the Specification (1), April 2008 to October 2008 in Specification (2) and April 2008 to April 2009 in Specification (3) and April 2008 to April 2010 in Specification (4).

Concluding, the analysis points to a positive effect of the 2008 registry comparison on on-time registrations. The analysis, moreover, suggests that the effect is largely driving by timing responses - taxpayers registering with SARS earlier than before - rather than genuinely new registrations. This is consistent with the second registry comparison in 2014 again uncovering a large number of non-compliant firms that had registered with CIPC but not with SARS.[39]

4.2.2 Spillovers on other Taxpayers II: Return Submission and Income Reporting

Furthermore, we assess whether the interventions also impacted *non-targeted* firms' compliance behavior on other compliance stages, namely on the tax return submission stage and taxable income reporting stage. As sketched in Section 2, the effect is theoretically ambiguous and compliance behavior may have decreased or increased. The sub-analysis draws on the 2014 registry comparison as we observe tax returns in the post-intervention period but not in the pre-intervention period of the 2008 intervention only. For the 2014 intervention, tax returns before and after the intervention are observed: the data spans returns submitted between mid 2009 to early 2016. We thus follow the empirical identification strategy in the prior sub-section and estimate a difference-in-differences model, which compares tax payable and return submission timing of non-targeted firms located in strongly and weakly treated grids - again defined as grids where the ratio of non-compliers identified in the 2014 intervention over voluntary SARS registrations in the pre-treatment year is in the lowest and highest quartile of the distribution respectively. Main regressor is an indicator for strongly treated grids interacted with an post-treatment indicator, indicating months after February 2014 (the month of the registry comparison). The model is estimated at the firm level to increase efficiency by including firm level control variables (firm assets and age). Clustering is at the grid-level. The set of regressors also includes firm fixed effects (nesting grid fixed effects) and fixed effects indicating the months when tax returns are submitted at SARS.

One caveat to note is that information on grid assignment is available to us only for firms, which registered with CIPC prior to 2012. The definition of treatment intensity in this sub-section therefore relies on firms identified as non-compliant in the 2014 registry comparison that registered with CIPC prior to 2012 (and are therefore, at the

[39]The fact that the number of identified non-compliers in 2014 is even larger than with the 2008 registry, may partly relate to the more adverse economic setting prior to the 2014 intervention (which negatively affects registration compliance as suggested by the analysis in Appendix A) . Also note that grids' treatment intensity by the 2008 and 2014 intervention (as defined in the text) are uncorrelated (correlation coefficient: 0.00, p-value:0.99).

time of the intervention, characterised by a time gap to CIPC registration), normalized on firms that voluntarily registered with SARS in 2011. We consider this to be a minor limitation only: For the 2008 intervention, we find similar results when grids' treatment intensity is calculated based on all non-compliers vs. based on late non-compliers with a significant registration gap only (cf. Specification (7) of Table 1).

The results are presented in Tables 4-5 and Figure 14. Table 4 and and the left hand panel of Figure 14 assess the impact of the reform on tax payable reported by firms that voluntarily registered with SARS (and are thus untargeted by the reform). Table 5 and the right hand panel of Figure 14 assess the impact of the reform on the timing of tax return submissions, measured as the gap between the due date of the return and the actual submission date of the return (in days). Specification (1) of Table 4 regresses the inverse hyperbolic sine of tax payable on full sets of month and firm fixed effects as well as control variables for age and firm size. The model compares firms in grids with no identified non-compliers (making up 15% of the sample but more than half of the grids) as defined above to firms in the upper quarter of the distribution of the 'treatment'-variable defined above. Specifications (2)-(3) add province-month fixed effects and grid-size-month fixed effects. The coefficient estimate for the treatment-interaction turns out small and statistically insignificant in all specifications. The same holds true when we adjust the definition of the treatment variable (Specification (4) compares firms in zero-grids to all other firms; Specification (5) interacts the post-dummy with a continuous treatment indicator; Specifications (6) and (7) compare zero-grids to grids where the treatment variable is in the upper 10% and 5% of the distribution). Specification (8) reestimates the model in Column (3) with PPML, again yielding a quantitatively small, albeit in this case statistically significant, coefficient estimate for the treatment interaction. Figure 14 compares the trend in average tax payable between zero-grids and strongly treated grids, confirming the common pre-trend and the absence of any significant treatment effect.

Table 5 and the right hand panel of Figure 14 redo the same analysis using the return submission gap to the due date of the return as dependent variable. Again, the results reject a significant effect of the reform on the timing of return submissions by untargeted taxpayers.

Concluding, this section provided evidence that the registry comparisons impacted on the timing of voluntary business tax registrations at SARS. In turn, we find no significant effect on the income reporting, as captured by tax payable, and tax return submission timing of other untargeted taxpayers.

Conclusion

This paper assessed the effectiveness of tax registration enforcement interventions in South Africa, where the South African Revenue Service, in 2008 and 2014, implemented snapshot-synchronizations of its business tax registry with the country's commercial register with the aim to identify firms that failed to sign up for business tax purposes. Both interventions led to large-scale expansions of the business taxpayer net.

Tax administrative data, moreover, allows us to identify the revenue contributions of the forcedly registered firms. Forcedly registered taxpayers are reported to submit tax returns at low rates and, conditional on return submission, report small tax liabilities. The analysis, moreover, suggests that a large fraction of the received revenues would have also been obtained in the absence of the intervention. Due to the large size of the tax net expansion, aggregate additional revenue receipts are nevertheless non-negligible. A significant part of these benefits stem from the fact that firms, which would, in the absence of the intervention, have voluntarily registered with SARS at a later point in time submit tax liabilities significantly earlier than in the absence of the intervention. Firms that would have never voluntarily registered with SARS, in turn, hardly make any revenue contribution.

The results, moreover, point to positive spillovers on voluntary business tax registrations: Specifically, we find improvements in the timing of voluntary registrations for business tax purposes: After the intervention, businesses registered with SARS much more quickly after their CIPC entry than before the intervention. We find no evidence for an increase in the number of tax registrations with SARS, however, or for spillover effects on the return submission or income reporting behavior of other taxpayers.

Taken together, the analysis suggests that the per-entity gains from registration enforcement interventions tend to be small and have to be carefully balanced against costs. While low implementation costs made the assessed interventions cost-effective, it is not clear that the same holds true for more resource-intensive registration enforcement measures that target less attractive firms segments within the shadow economy.

5 Appendix: Figures and Tables for Main Text

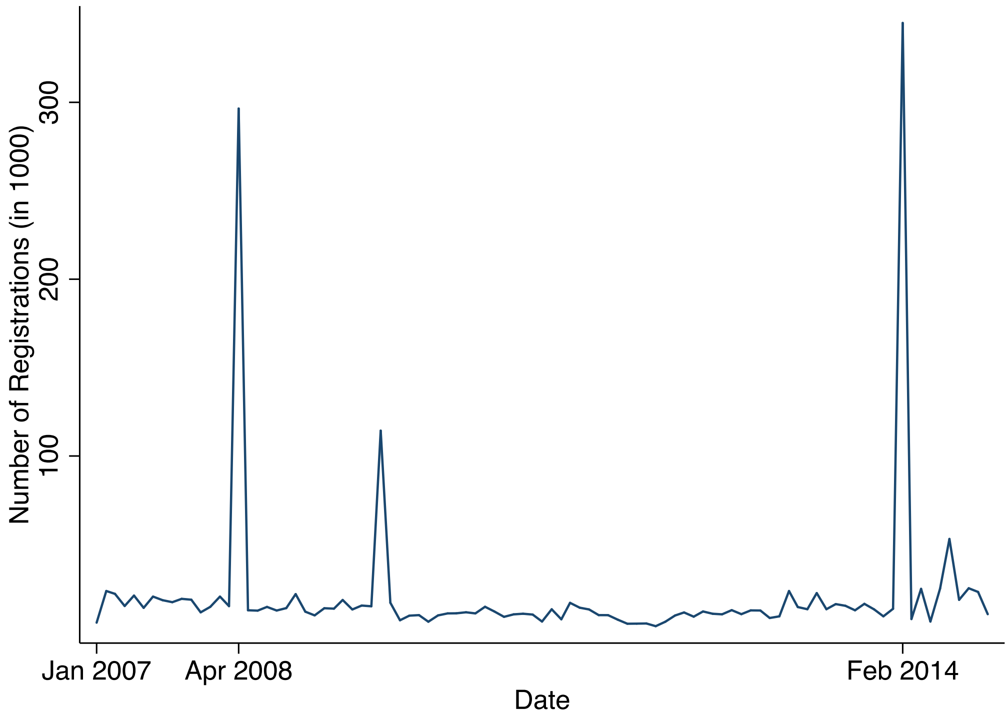
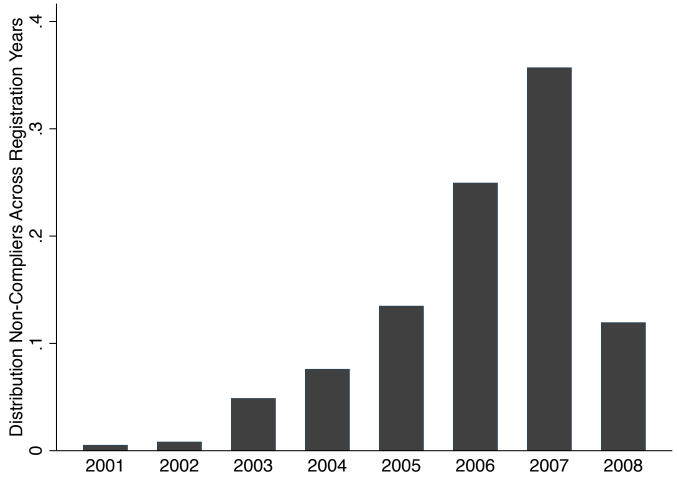
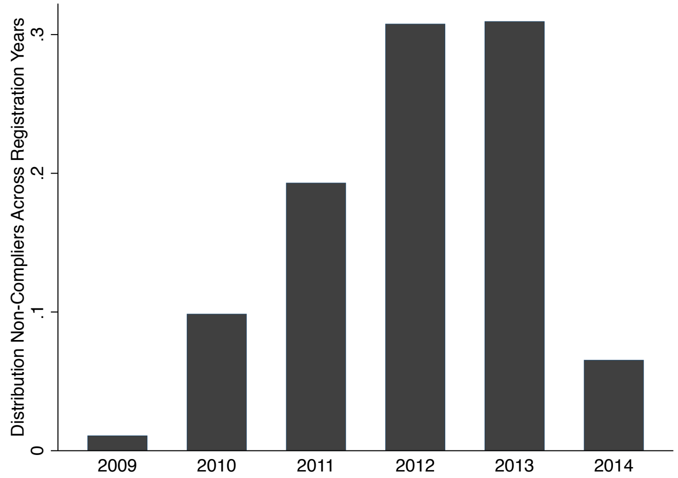


Figure 1: New Registrations for Business Tax Purposes with SARS per Month

Notes: The figure depicts the number of new firm registrations with SARS's business tax registry per registration month between January 2007 and December 2014.



Commercial Registry Comparison 2008



Commerical Registry Comparison 2014

Figure 2: Distribution Registration Year with CIPC of Non-Compliant Firms

Notes: The figure depicts the age of firms identified as non-compliant in the two registry comparisons by the distribution across CIPC registration years. It indicates that most firms identified in the registry comparisons are young and had registered with CIPC within two years before the interventions. Note that the comparisons took place in April 2008 and February 2014, explaining the relatively smaller shares attributed to the CIPC registration years 2008 (left figure) and 2014 (right figure) respectively.

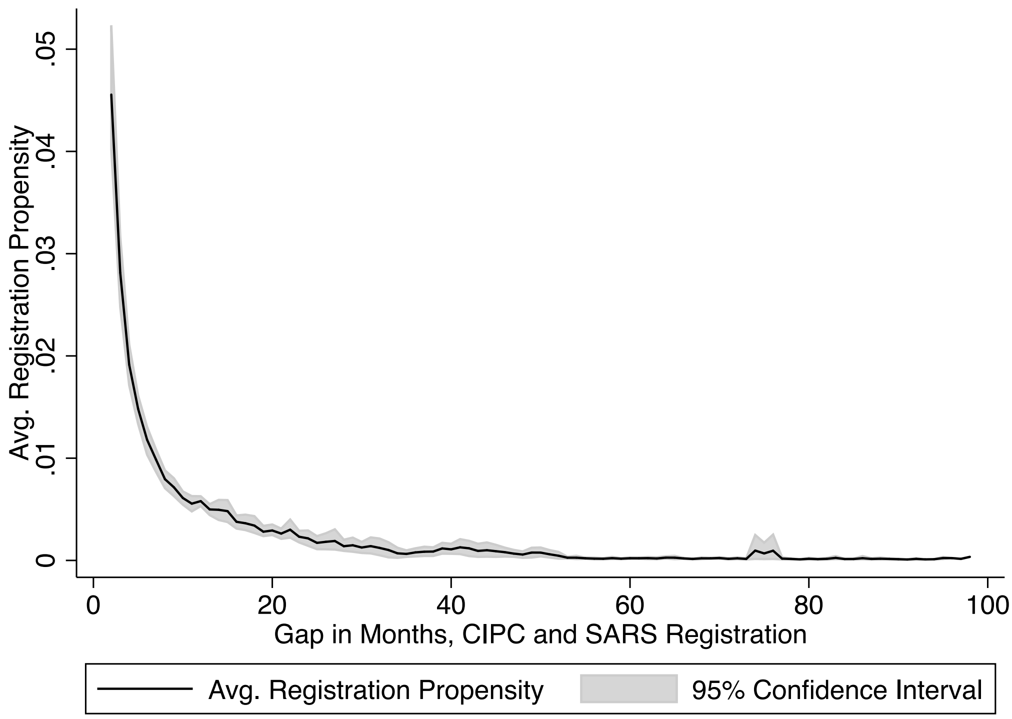


Figure 3: Propensity to Register with SARS with a Given Lag to CIPC Registration

Notes: The figure depicts the propensity to register with SARS with a given monthly lag to the registration month with CIPC. The propensity is calculated from information on SARS registrations between January 2007 and March 2008 and the date of registration of the respective firms with CIPC, as described in the main text. Confidence intervals are bootstrapped.

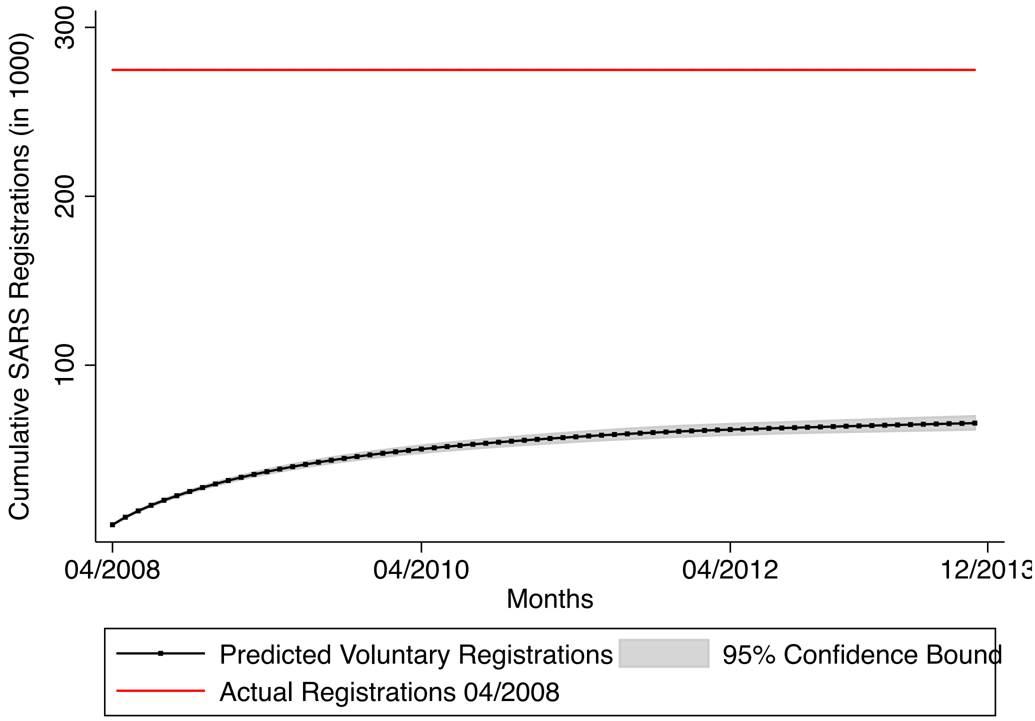


Figure 4: Cumulative Number of Registered Firms - 2008 Intervention: Actual vs. Voluntary Registrations under the Counterfactual

Notes: The figure depicts the actual number of non-compliers identified in the 2008 comparison of commercial and business tax registry (red line) and the cumulative number of identified non-compliers that would have voluntarily registered with SARS in the absence of the intervention (black line). Confidence intervals are bootstrapped.

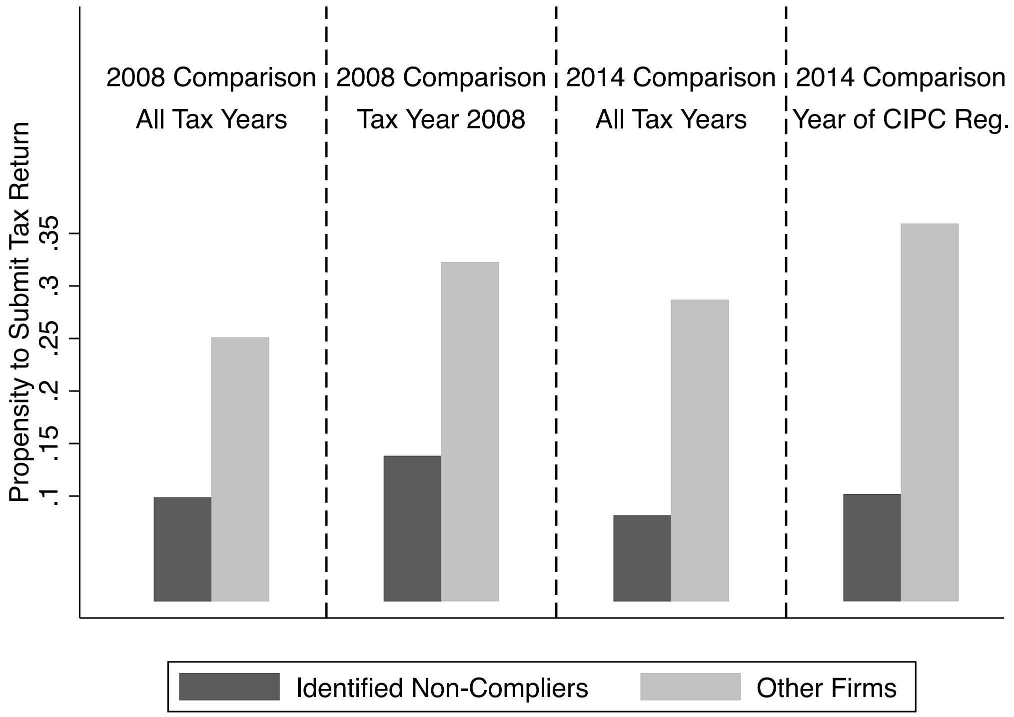


Figure 5: Propensity to Submit Tax Return - Identified Non-Compliers vs. Other Firms

Notes: The figure depicts the propensity to submit a tax return in the first liability year after SARS registration for firms with a registration gap of 6months, 6-18 months, 18-24 months and so on, in half year steps (figure indicates the end of the half year period). Confidence intervals are bootstrapped.

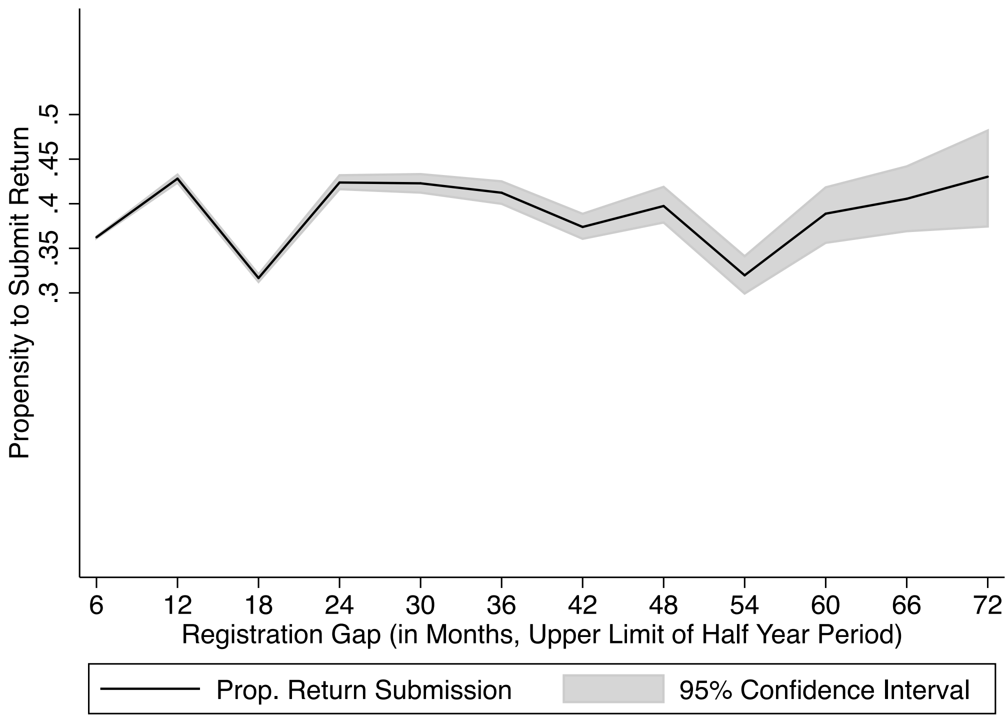


Figure 6: Propensity to Submit Tax Return - Voluntarily Late Registered Firms: First Liability Year

Notes: The figure depicts the propensity to submit a tax return in the first liability year after SARS registration for firms with a registration gap of 6months, 6-18 months, 18-24 months and so on, in half year steps (figure indicates the end of the half year period). Confidence intervals are bootstrapped.

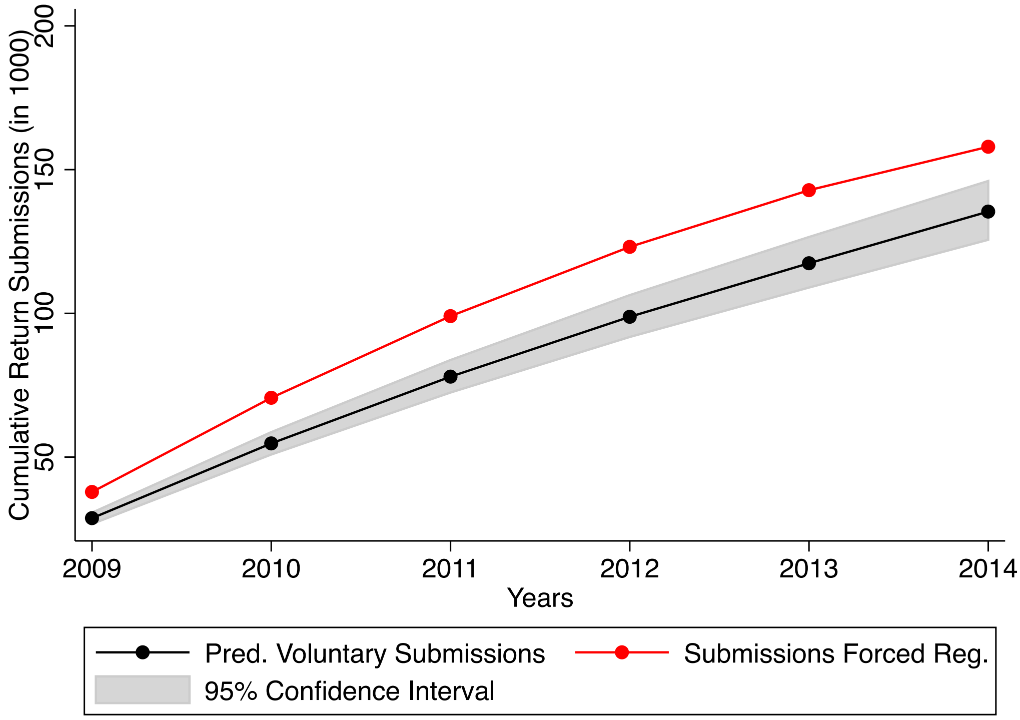


Figure 7: Cumulative Number of Submitted Tax Returns - 2008 Intervention: Actual vs. Voluntary Submissions under the Counterfactual

Notes: The figure depicts the number of submitted tax returns by non-compliant taxpayers identified in the 2008 registry comparison by tax year (red line) and the cumulative number of tax returns that would have been submitted voluntarily by these non-compliant taxpayers in the absence of the intervention. Confidence intervals are bootstrapped.

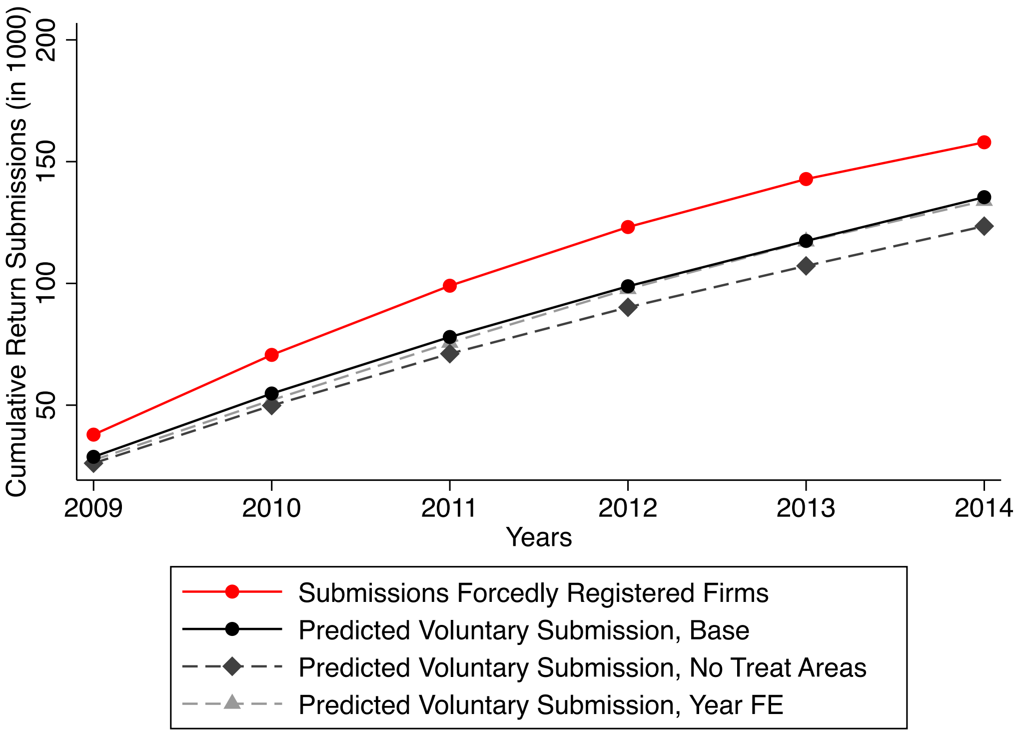
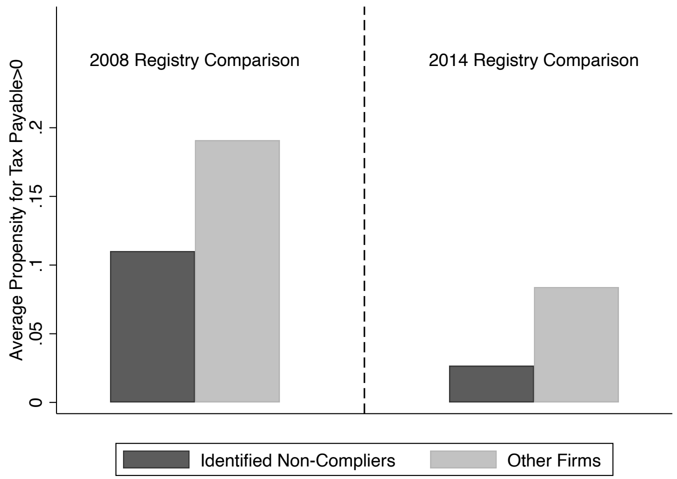
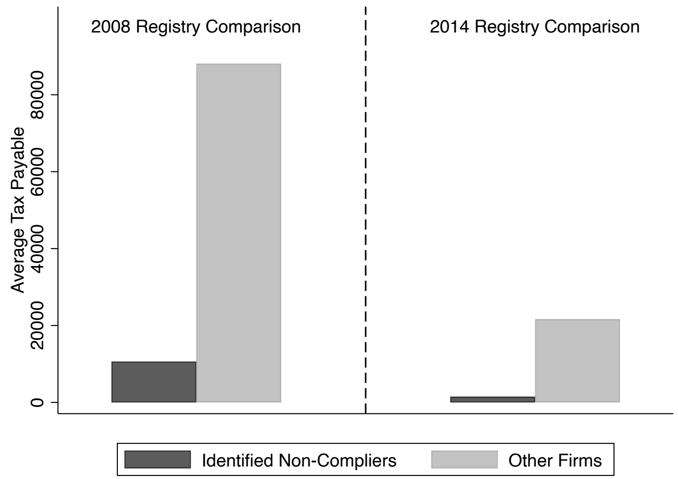


Figure 8: Cumulative Number of Submitted Tax Returns - 2008 Intervention: Robustness Checks

Notes: The figure depicts the number of submitted tax returns by non-compliant taxpayers identified in the 2008 registry comparison by tax year (red line) and the cumulative number of tax returns that would have been submitted voluntarily by these non-compliant taxpayers in the absence of the intervention. The black circle line is the cumulative number of returns under the counterfactual obtained in the base analysis; the grey diamond line shows the cumulative number of tax returns when submission propensities are calculated from firms in weakly treated areas and the light grey line with triangles indicates the cumulative number of return submissions when the return submission propensities are calculated account for differences in return submission propensities across tax years. Confidence intervals are bootstrapped.



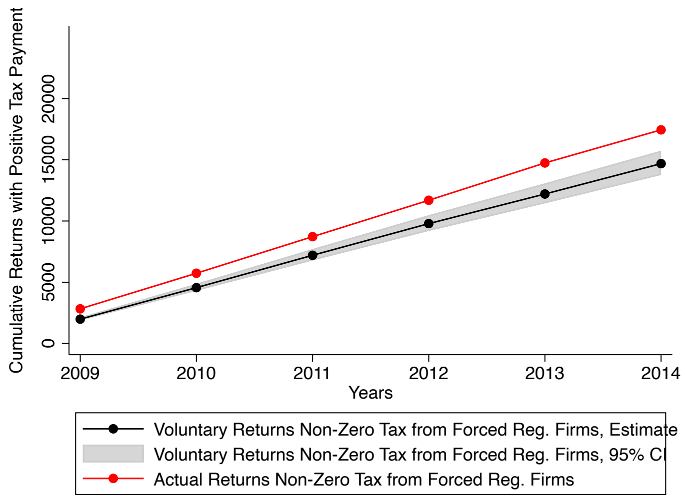
Commercial Registry Comparison 2008: Binary Tax Payment



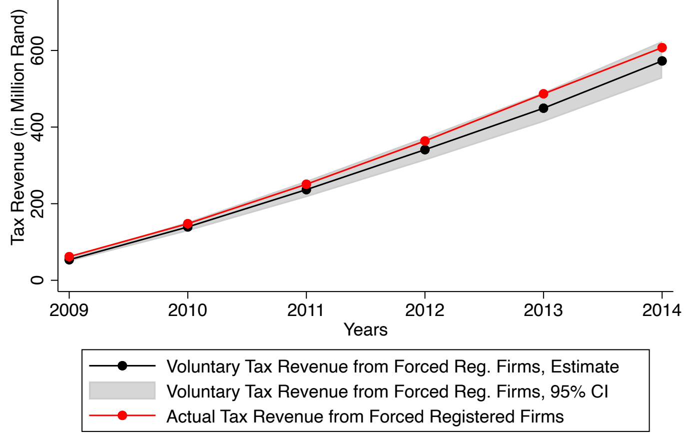
Commerical Registry Comparison 2014: Level Tax Payment

Figure 9: Comparison Tax Payment (Binary and in Levels)

Notes: The right hand panel depicts the average propensity to submit a return with a positive tax liability for firms that were identified in the 2008 and 2014 registry comparison respectively and control firms. The left hand panel depicts the average tax liability of firms that were identified in the 2008 and 2014 registry comparison respectively and control firms. Both groups (identified non-compliers and control entities) are restricted to entities that had registered with CIPC between January 2000 and February 2008 for the 2008 registry comparison and between January 2009 and December 2013 for the 2014 registry comparison. The difference in the propensity for non-zero tax liabilities and average tax liabilities between the 2008 and 2014 intervention likely relates to the fact that the former entities are larger in our observed sample frame (the tax years 2009 to 2014). Specifically, for the 2008 registry comparison, we observe returns submitted for tax years after the intervention. For the 2014 comparison, returns for tax years prior to the intervention (but after CIPC registration).



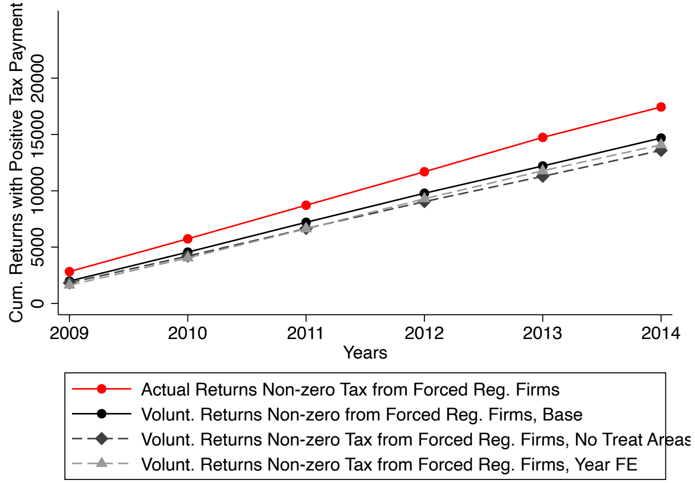
Binary Tax Payment



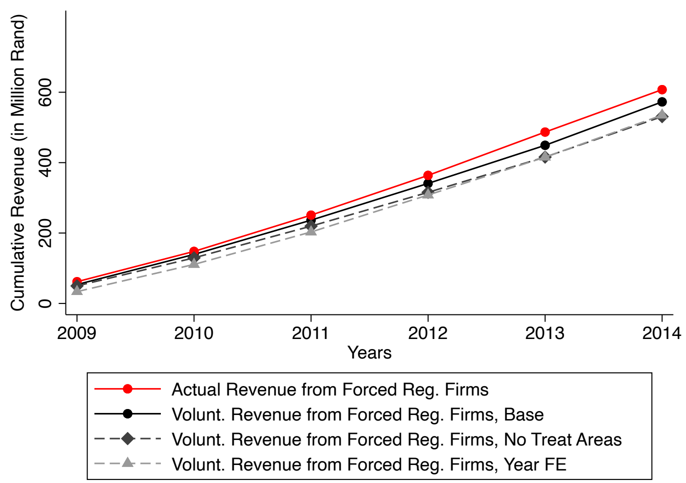
Level Tax Payment

Figure 10: Actual vs. Predicted Voluntary Revenue Received - Base Analysis

Notes: The right hand panel depicts the cumulative actual number of returns with a positive tax liability submitted by taxpayers identified in the 2008 registry comparison (red line) and the number of returns with a positive tax liability that would have been submitted by taxpayers identified in the 2008 registry comparison in the absence of the intervention (black line). Analogously, the right hand panel depicts the actual tax revenue submitted by taxpayers identified in the 2008 registry comparison (red line) and the tax revenue that would have been submitted by taxpayers identified in the 2008 registry comparison in the absence of the intervention (black line). Confidence intervals are bootstrapped.



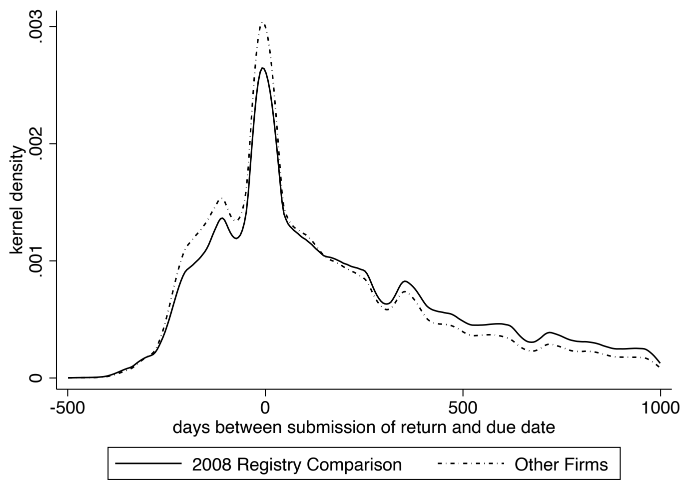
Binary Tax Payment



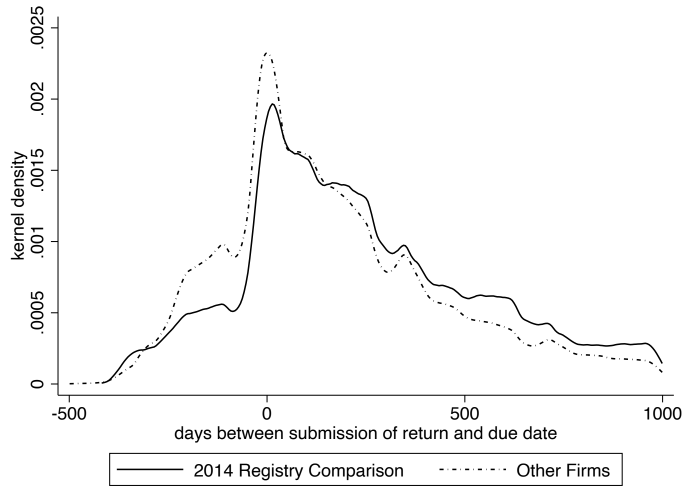
Level Tax Payment

Figure 11: Actual vs. Predicted Voluntary Revenue Received - Robustness Checks

Notes: The right hand panel depicts the cumulative actual number of returns with a positive tax liability submitted by taxpayers identified in the 2008 registry comparison (red line) and the number of returns with a positive tax liability that would have been submitted by taxpayers identified in the 2008 registry comparison in the absence of the intervention as calculated in the base analysis (black line) as well as in robustness checks where the calculation of $\hat{p}^s_{\ell,\tilde{t}}$ and $\hat{T}_{\ell,\tilde{t}}$ accounts for year-specific effects and is calculated from taxpayers in weakly treated areas only. Analogously, the right hand panel repeats the analysis for the tax revenue submitted. Confidence intervals are bootstrapped.



2008 Registry Comparison



2014 Registry Comparison

Figure 12: Timing of Tax Return Submissions

Notes: The graph depicts the kernel density of registration submission timing of forcedly registered firms and other firms, where registration timing is measured by the difference in days between the submission of a tax return with SARS and the due date of the return.

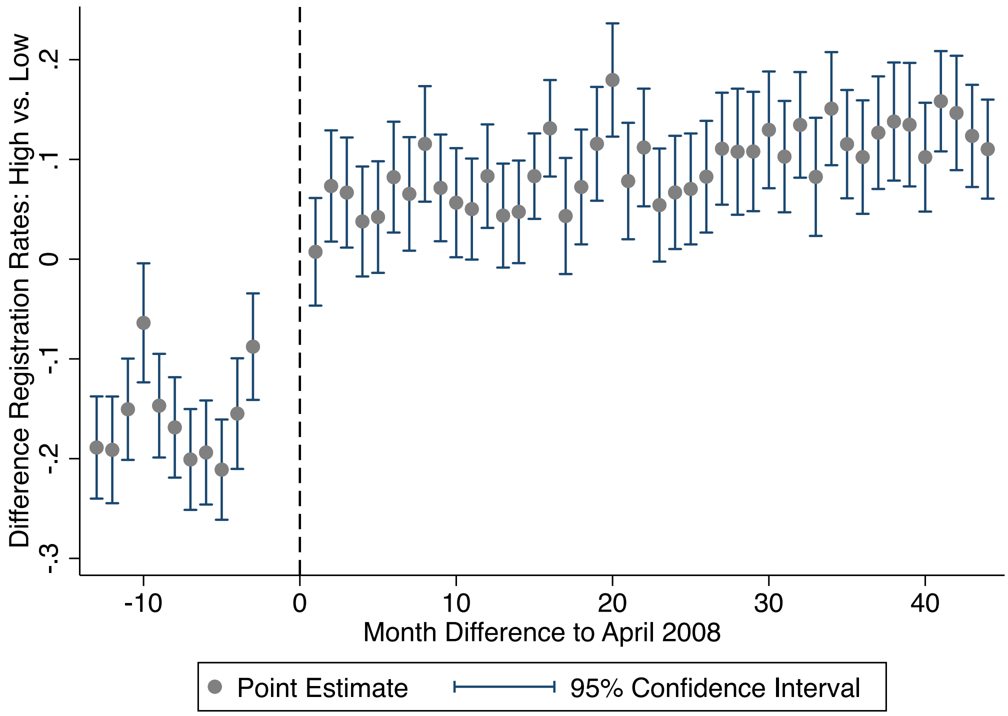


Figure 13: Voluntary On-Time Registrations

Notes: The graph depicts the difference in fraction of CIPC-registered firms that registered with SARS within the legally defined time frame between grids that were strongly and weakly treated by the 2008 intervention (in the sense that the number of identified non-compliers in the 2008 intervention over all SARS registrations in 2007 is in the lower and upper quarter of the distribution). The dashed line indicates the treatment month (April 2008). We abstract from reporting estimates for February and March 2008 as the ratio of on-time registrations in these months might be mechanically affected by the intervention.

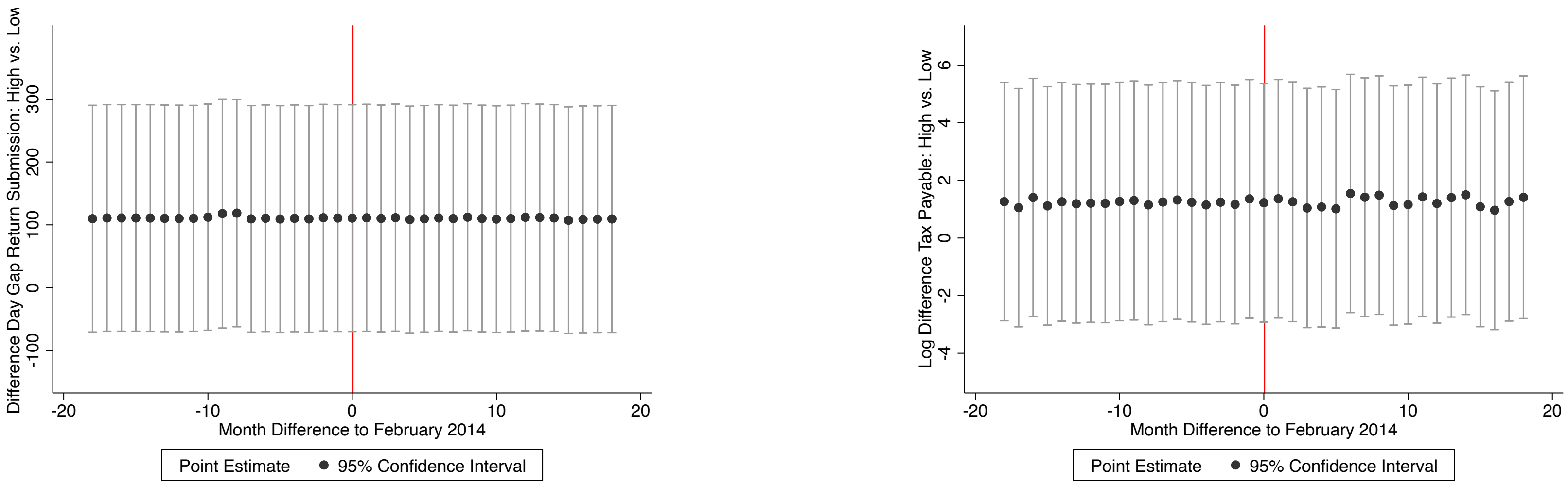


Figure 14: Effect on Non-Targeted Taxpayers

Notes: The figures depict the effect of the 2014 intervention on return submission timing responses (left hand panel) and tax reporting, conditional on return submission (right hand panel), of other taxpayers, that were not directly targeted by the reform (but had voluntarily registered for business tax purposes). The red line indicates the treatment month (February 2014).

Table 1: Spillovers - On-time Registrations: Base Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post · Strong Treat	0.2284***	0.2285***	0.2235***	0.1278***	0.3273***	0.0742***	0.1306***
	(0.0077)	(0.0079)	(0.0100)	(0.0062)	(0.0173)	(0.0056)	(0.0104)
Observations	56,366	56,366	56,193	110,418	22,214	110,418	55,691
Adjusted R-squared	0.186	0.187	0.197	0.183	0.212	0.183	0.182
Firm-Month Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Area-Month Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes
Treatment Definition	1st vs. 4th Quartile	1st vs. 4th Quartile	1st vs. 4th Quartile	Above/Below Median	1st vs. 10th Decile	Continuous	1st vs. 4th Quartile

Standard errors in parentheses, clustered at the area level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Spillovers - On-time Registrations: Timing			
	(1)	(2)	(3)
Post · Strong Treat	0.0752***	0.0774***	0.0242***
	(0.0124)	(0.0133)	(0.0063)
Observations	39,664	33,843	33,843
Adjusted R-squared	0.139	0.153	0.0645
Firm-Month Fixed Effects	Yes	Yes	Yes
Area-Month Fixed Effects	Yes	Yes	Yes
Dep. Variable	On time vs. 180 days	On time vs. 365 days	180 days vs. 365 days

Standard errors in parentheses, clustered at the area level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	(1)	(2)	(3)	(4)	(5)
Table 3: Spillovers - On-time Registrations: Level					
Post · Strong Treat	0.1187***	0.0393***	0.0196***	0.0112	-0.0079**
	(0.0131)	(0.0083)	(0.0072)	(0.0077)	(0.0036)
Observations	49,958	41,939	35,032	23,620	56,155
Adjusted R-squared	0.805	0.931	0.950	0.976	0.989
Firm-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Area-Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Dep. Variable	On time	<180 days	<365 days	<730 days	CIPC Reg.
	Level, IHS	Level, IHS	Level, IHS	Level, IHS	Level

Standard errors in parentheses, clustered at the area level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Log Tax Payable - Spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post · Treat	0.0104	0.0176	-0.0225	-0.0332	-0.0037	0.0149	0.0109	-0.0270***
	(0.0492)	(0.0485)	(0.0507)	(0.0476)	(0.0034)	(0.0304)	(0.0312)	(0.0100)
Log Age		0.9047***	0.9063***	0.9108***	0.9174***	0.8750***	0.9022***	0.4275***
		(0.0555)	(0.0556)	(0.0513)	(0.0516)	(0.0431)	(0.0459)	(0.0199)
Log Total Assets	0.4696***	0.4547***	0.4548***	0.4547***	0.4548***	0.4511***	0.4464***	0.2222***
	(0.0139)	(0.0139)	(0.0139)	(0.0123)	(0.0124)	(0.0049)	(0.0052)	(0.0160)
Observations	1,584,390	1,584,104	1,584,104	1,842,567	1,826,487	713,301	626,344	877,127
Adjusted R-squared	0.651	0.651	0.651	0.649	0.650	0.651	0.652	
Area-Year	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Yes								
Firm-Year	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Yes								
Teat	0 vs. top 25	0 vs. top 25	0 vs. top 25	0 vs. Other	Continuous	0 vs. top 10	0 vs. top 5	0 vs. top 25
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	PPML

Standard errors in parentheses, clustered at the area level. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the inverse hyperbolic sine of total tax payable (Specification (1)-(7)) and the level of total tax payable respectively (Specifications (8)-(9)). The dependent variable is regressed upon an indicator for the post-intervention period (February 2014 or later) and a treatment variable. The definition of the treatment variable and with it, the sample, differs across specifications: In Specifications (1)-(3) and Specification (8), grids with zero firms identified as non-compliers in 2014 registry comparison (and founded by 2011, see main text) are compared to grids where the ratio between identified non-compliers (founded by 2011) and firms registered with SARS in 2013 is in the upper quartile of the distribution; in Specification (4), they are compared to all grids with a non-zero-treatment variable; in Specification (5), the post-dummy is interacted with a continuous treatment variable (the grid's number of non-compliers identified in the 2014 registry comparison (and founded by 2011) over the grid's number of firms that registered with SARS in 2013); in Specification (6) (Specification (7)) grids with zero firms identified as non-compliers in 2014 registry comparison (and founded by 2011, see main text) are compared to grids where the ratio between non-compliers (founded by 2011) and firms registered with SARS in 2013 is in the top 10% (top 5%) of the distribution. Specifications (1)-(7) are estimated by OLS, Specifications (8)-(9) by PPML.

Table 5: Days Between - Spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post · Treat	-0.0252	0.1343	0.1504	-0.1079**	-0.0053	-0.2609	0.2408
	(0.4570)	(0.3071)	(0.3084)	(0.0438)	(0.5320)	(0.5861)	(0.3065)
Log Age		-1.3740***	-1.3728***	-1.4022***	-2.0805***	-1.9556***	-0.5393
		(0.3772)	(0.3774)	(0.3621)	(0.7813)	(0.8269)	(0.3696)
Log Assets	-0.5273***	-0.5066***	-0.5066***	-0.4838***	-0.4869***	-0.4855**	-0.4943***
	(0.0578)	(0.0504)	(0.0504)	(0.0482)	(0.1048)	(0.1114)	(0.0487)
Observations	1,510,374	1,526,103	1,526,103	1,658,563	605,696	493,000	1,668,125
Adjusted R-squared	0.960	0.960	0.960	0.960	0.960	0.960	0.981
Area-Year	No	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Year	No	No	Yes	Yes	Yes	Yes	Yes
Teat	0 vs. top 25	0 vs. top 25	0 vs. top 25	Continuous	0 vs. top 10	0 vs. top 5	0 vs. top 25
Drop	> 365	> 365	> 365	> 365	> 365	> 365	> 900

Standard errors in parentheses, clustered at the area level. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the number of days between the due date of the tax return and the submission date. The dependent variable is regressed upon an indicator for the post-intervention period (February 2014 or later) and a treatment variable. The definition of the treatment variable and with it, the sample, differs across specifications: In Specifications (1)-(3) and Specification (7), grids with zero firms identified as non-compliers in 2014 registry comparison (and founded by 2011, see main text) are compared to grids where the ratio between identified non-compliers (founded by 2011) and firms registered with SARS in 2013 is in the upper quartile of the distribution; in Specification (4), the post-dummy is interacted with a continuous treatment variable (the grid's number of non-compliers identified in the 2014 registry comparison (and founded by 2011) over the grid's number of firms that registered with SARS in 2013); in Specification (5) (Specification (6)) grids with zero firms identified as non-compliers in 2014 registry comparison (and founded by 2011, see main text) are compared to grids where the ratio between non-compliers (founded by 2011) and firms registered with SARS in 2013 is in the top 10% (top 5%) of the distribution.

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Online Appendix

Appendix A - Theory

Selection Effects - Correlation of Detection Risk Across Compliance Stages

This section presents comparative static analyses for the model presented in the subsection *Selection Effects - Correlation of Detection Risk Across Compliance Stages*. The impact of changes in δ_i on firms' tax underreporting e_i (stage 3), decision to submit a tax return or not, Φ_i (stage 2), and decision to register with the tax authority or not, Γ_i^s and Γ_i^N (stage 1) is given by

$$\frac{\partial e_i}{\partial \delta_i} = -\frac{(\tau + \frac{\partial F}{\partial e_i})}{p_i^I \frac{\partial^2 F}{\partial e_{it}^2}} \frac{\partial p_i^I}{\partial \delta_i} < 0 \tag{17}$$

$$\frac{\partial \Phi_i}{\partial \delta_i} = (F_s + \tau y - EV_i)\frac{\partial p_i^s}{\partial \delta_i} + (1 - p_i^s)\frac{EV_i}{\partial \delta_i} \gtreqless 0 \tag{18}$$

$$\frac{\partial \Gamma_i^s}{\partial \delta_i} = (F_R + \tau y - EV_i)\frac{\partial p_i^R}{\partial \delta_i} + (1 - p_i^R)\frac{EV_i}{\partial \delta_i} \gtreqless 0 \tag{19}$$

$$\begin{aligned} \frac{\partial \Gamma_i^N}{\partial \delta_i} = \; & F_R \frac{\partial p_i^R}{\partial \delta_i} + (F_s + \tau y - EV_i)(p_s \frac{\partial p_i^R}{\partial \delta_i} - (1 - p_i^R)\frac{\partial p_i^s}{\partial \delta_i}) \\ & (F_R + \tau y - EV_i) + (1 - p_i^R) p_i^s \frac{EV_i}{\partial \delta_i} \gtreqless 0 \end{aligned} \tag{20}$$

with $\frac{\partial EV_i}{\partial \delta_i} = -(\tau y + F(e_i))\frac{\partial p_i^I}{\partial \delta_i} < 0$. While a lower δ_i unambiguously raises tax evasion at the income reporting stage, the effect on return submission behavior and taxpayer registration is ambiguous. On the one hand, reductions in δ_i lower non-compliance detection risk at all compliance stages, making non-compliance more attractive. Firms with a low δ_i- realization are thus less likely to register for tax purposes and also behave systematically less compliant on later compliance stages when drawn into the tax net. On the other hand, firms with a low realisation of δ_i may have a higher propensity to register for tax purposes than firms with high δ_i-realizations as they care less about ending up on return submission and income reporting stage (as their low δ_i-realization allows for non-compliance on these stages), respectively. If drawn into the tax net, non-compliers at the registration stage may thus behave more compliant than firms that voluntarily registered with the authorities. Which effect prevails, depends on the level of and the δ_i-effect on detection risk at the different compliance stages.

Selection Effects - Firm Size

The base analysis in the main text assesses how selection of taxpayers into non-registration may affect compliance behavior of forcedly registered firms after registration and how this compares to firms that voluntarily signed up with the authorities. Another determinant of tax payments made by forcedly registered firms is their size and true underlying income.

If large firms with high underlying income are drawn into the tax net, larger revenue effects emerge than if identified non-compliers tend to be small. We thus drop the assumption of homogenous firm income and assume a taxpayer specific underlying income y_i. We, moreover, follow prior research and assume that non-compliance detection risk increases in firm size (see e.g. Ulyssea, 2018, Kumler et al., 2020): $\frac{\partial p_i^I}{\partial y_i}, \frac{\partial p_i^s}{\partial y_i}, \frac{\partial p_i^R}{\partial y_i} > 0$.[40] With these modifications, the level of tax evasion on the income reporting stage is still determined by Equation (3). The decision to submit a tax return or not and to register with the tax authorities or is governed by the following equations.

$$\begin{aligned}
\Phi_i^y &= p_i^s F_s - (1-p_i^s)(\tau y_i - EV_i) \\
\Gamma_i^{Sy} &= p_i^R F_R - (1-p_i^R)(\tau y_i - EV_i) \\
\Gamma_i^{Ny} &= p_i^R F_R - (1-p_i^R)p_i^s(\tau y_i - EV_i + F_s)
\end{aligned}$$

Comparative statics with respect to firm size read

$$\begin{aligned}
\frac{\partial \Phi_i^y}{\partial y_i} &= (F_s + \tau y_i - EV_i)\frac{\partial p_i^s}{\partial y_i} - (1-p_i^s)\{\tau(2-p_i^I) - (\tau y_i + F(e_i))\frac{\partial p_i^I}{\partial y_i}\} \gtreqless 0 \\
\frac{\partial \Gamma_i^{Sy}}{\partial y_i} &= (F_R + \tau y_i - EV_i)\frac{\partial p_i^R}{\partial y_i} - (1-p_i^R)\{\tau(2-p_i^I) - (\tau y_i + F(e_i))\frac{\partial p_i^I}{\partial y_i}\} \gtreqless 0 \\
\frac{\partial \Gamma_i^{Ny}}{\partial y_i} &= \{F_R + p_i^s(\tau y_i - EV_i + F_s)\}\frac{\partial p_i^R}{\partial y_i} - (1-p_i^R)(\tau y_i - EV_i + F_s)\frac{\partial p_i^s}{\partial y_i} - \\
&\quad -(1-p_i^R)p_i^s\{\tau(2-p_i^I) - (\tau y_i + F(e_i))\frac{\partial p_i^I}{\partial y_i}\} \gtreqless 0
\end{aligned}$$

The equations suggest that it is unclear whether small or large firms select into non-registrations. On the one hand, a larger firm size increases the risk that non-compliant behavior is detected, thus raising the propensity to register for tax purposes. On the other hand, larger firms have higher underlying income, resulting in larger taxes due.

[40] Large firms have more trading partners and may therefore be more visible to tax authorities than smaller entities and may be more easily be detected.

This diminishes incentives to register for tax purposes. On top of that, effects as described above apply: firm size raises non-compliance detection risk on later stages and thereby diminishes incentives to register for tax purposes.[41] Empirically, it is a well-established fact that firms in the shadow economy are smaller than formal entities, which suggests that the former effect prevails (La Porta and Shleifer., 2008, La Porta and Shleifer., 2014).

Concluding, this section showed that firms which do and do not voluntarily register for tax purposes may systematically differ in size and with regard to their compliance on later stages (when submitting tax returns or reporting taxable income). The sign of the effect is ambiguous in turn, making it unclear whether new taxpayers drawn into the tax net make above or below average revenue contributions.

Appendix A - Empirics: Voluntary Registrations - Adjustment for Changes in Economic Conditions

As sketched in the main text, one caveat of the base analysis is that the propensity that non-compliers identified in the 2008 registry comparison would have voluntarily registered with SARS at a later point in time is modelled from observed voluntary registration behavior at SARS before the intervention. Potential changes in registration rates over time hence remain uncaptured. Especially economic conditions may vary in the short and medium run and may potentially impact the decision of firms to register with SARS for tax purposes.

To account for this possibility, we test whether business environments impact firm registrations at SARS. The identifying variation stems from changes in unemployment rates and GDP per capita at the level of South African provinces. The data was obtained from OECD's regional statistics and is linked to information on firms' registration decisions drawn from our main data. To avoid estimates that are affected by the registry comparisons in 2008 and 2014, we focus on the years 2010 to 2013. Registration behavior at SARS is modeled by the propensity to register with SARS with a given lag ℓ in province p in month m, $\alpha_{\ell ptm}$ (specifically, the ratio of the number of firms located in province p that register with SARS for business tax purposes in month m of year t, having registered with CIPC ℓ month ago relative to the total number of firms that registered ℓ month ago with CIPC in province p). Analogous to our base analysis, we account for late registrations only, that is for cases where $\ell > 1$ as these are the ones relevant for our analysis.[42]

[41] Analogous incentives apply for the decision to submit a tax return or not.

[42] We account for CIPC registrations from January 2009 onwards. That is for firms registered with

Formally, the model reads

$$\alpha_{\ell ptm} = \gamma_1 unemp_{pt} + \gamma_2 GDP_{pt} + \phi_p + gap_\ell + CIPC_t + SARS_t + \epsilon_{\ell pmt} \quad (21)$$

where $\alpha_{\ell ptm}$ is the fraction of firms located in province p that register with SARS with registration lag ℓ, in a particular month m of year t between January 2010 and December 2013 over all firms that registered with CIPC ℓ months ago. This is regressed on the unemployment rate in state p in year t, GDP per capita in state p at time t as well as province fixed effects, year fixed effects (for years of SARS and CIPC registration) and dummy variables indicating the time gap (in months) between CIPC and SARS registration. The estimates in Table A1 show that increasing unemployment rates deteriorate registration rates in a statistically significant, albeit quantitatively moderate way. An increase in the unemployment rate by 1 percentage point lowers the propensity to register with the tax authorities by 0.04 percentage points. GDP per capita is not found to exert a significant effect.

The coefficient estimates from equation 21 are used to predict how changes in unemployment rates at the national level (cf. Figure A1) impact firms' registration behavior after the 2008 intervention.[43] We also run modified versions of Equation (21), where we allow γ_1 to vary with the registration gap between CIPC and SARS registration. This allows the impact of changes in economic conditions on registration behavior to differ between early and late registrations. The results are presented in Specification (2) of Table A1 and suggest that increasing unemployment rates have a slightly positive effect on the propensity to register with SARS in early months after the CIPC registration ($\ell \leq 6$). The effect on registrations with a larger time gap to CIPC registration is, in turn, negative and does not vary much with the time gap between CIPC and SARS registration. We reestimate the number of voluntary new registrations at SARS by adjusting for unemployment effects using $\hat{\gamma}_1$ and observed macro-changes in unemployment rates after 2008 (cf. Figure A1). When, for example, predicting the propensity to register voluntarily with SARS in 2009 with a 24 month gap from CIPC registration, we adjust the base estimate for registering late by 24 months by $\hat{\gamma}_1 \cdot 1.9$, where 1.9 corresponds to the percentage point difference in unemployment rates in South Africa

SARS in January 2010, the data includes 10 observations, which capture the propensity to register with SARS in that month with a lag of 2 months, 3 months,... up to 12 months from their CIPC registration. For firms in February 2010, 11 observations are included capturing the propensity to register with SARS in that month with a lag of 2 months, 3 months, ... up to 13 months from their CIPC registration. Analogously for the following months. For December 2013, our data includes 59 observations, reflecting the propensity to register with SARS in that month with a lag of 2 months, 3 months, ... up to 59 months from their CIPC registration.

[43] As the coefficient estimates reject a statistically significant effect of GDP per capita, we do not account for effects related to varying GDP per capita over time.

between the base year and 2010. The predicted number of new registrations based on this modification is presented in Figure A2. In line with Figure A1 (which shows a slight drop in unemployment rates from 2007 to 2008) and increasing unemployment rates thereafter, we find slightly higher cumulative predicted voluntary registrations in 2008 and lower predicted cumulative registration numbers thereafter. In total, the analysis suggests that until January 2014, 49,000 new firms would have voluntarily registered with SARS, which is broadly comparable to the base analysis.

An alternative approach to account for changes in voluntary registration behavior after the 2008 registry comparison is to not only rely on pre-intervention information when calculating this behavior but also drawing on registration rates after the intervention.[44] The obvious downside of this approach is that registration behavior might be affected by the 2008 intervention itself. To account for this, we calculate the post-intervention voluntary registration propensities from firms in areas that were only weakly treated by the 2008 intervention. Specifically, firms are assigned to 300mX300m-grids and we use only firms in grids that are weakly treated by the intervention as indicated by a ratio of identified non-compliers in the 2008 intervention over all SARS registrations in 2007 below the sample median. Note in this context that there was no media coverage of the interventions nor any official SARS communication. Taxpayers thus might have learned about the intervention through communication with other affected firms only, which is arguably particularly likely if taxpayers are located in close proximity. Lediga et al. (2020) show that enforcement spillovers from taxpayer audits in South Africa are limited to taxpayers in close geographic proximity, even if firms are operating in the same industry or are connected through value chains. Boning et al. (2018) and Drago et al. (2020) report comparable evidence for the US and Austria.

Drawing on this sample of firms, we model changes in voluntary registration behavior between the pre-treatment period before March 2008 (which is used to model voluntary registration behavior in the base analysis) and the post-intervention years by regressing voluntary registration rates on a full set of year dummies indicating the year of SARS registration. In doing so, we allow the year effects to vary by registration lag, going in half-year steps and accounting for registrations within less than 6 month, registrations

[44] For illustration, consider the following example. Calculating $\hat{R}$, among others, requires determining the propensity that firms which registered with CIPC in February 2008 registered with SARS with a 6-month lag and therefore in August 2008. In the base version of the model, we use the observed propensity prior to March 2008 to register with SARS with a 6-month lag. One might, however, also look at firms which registered with CIPC in May 2009 and their propensity to register with SARS in November 2008 and hence also with a 6-month lag. When determining the propensity that firms which registered with SARS in February 2008 to register with SARS with a 24 month lag and thus in February 2010, the baseline analysis calculates it from - chronologically distant - registration behavior prior to March 2008. Alternatively, one might turn to firms that registered with CIPC in May 2008 and their propensity to register with a 24 month lag, and therefore, in May 2010 - which is chronologically close to February 2010.

within 7-12 months, 13-18 months, and so on. Table A2 presents the estimation results.

The estimates in Table A2 are again used to adjust predicted voluntary registration rates after 2008 for common shocks. Specifically, we determine the relative change in registration rates over time from the estimates in A2 and multiply it with the base late registration propensity determined in the base analysis.

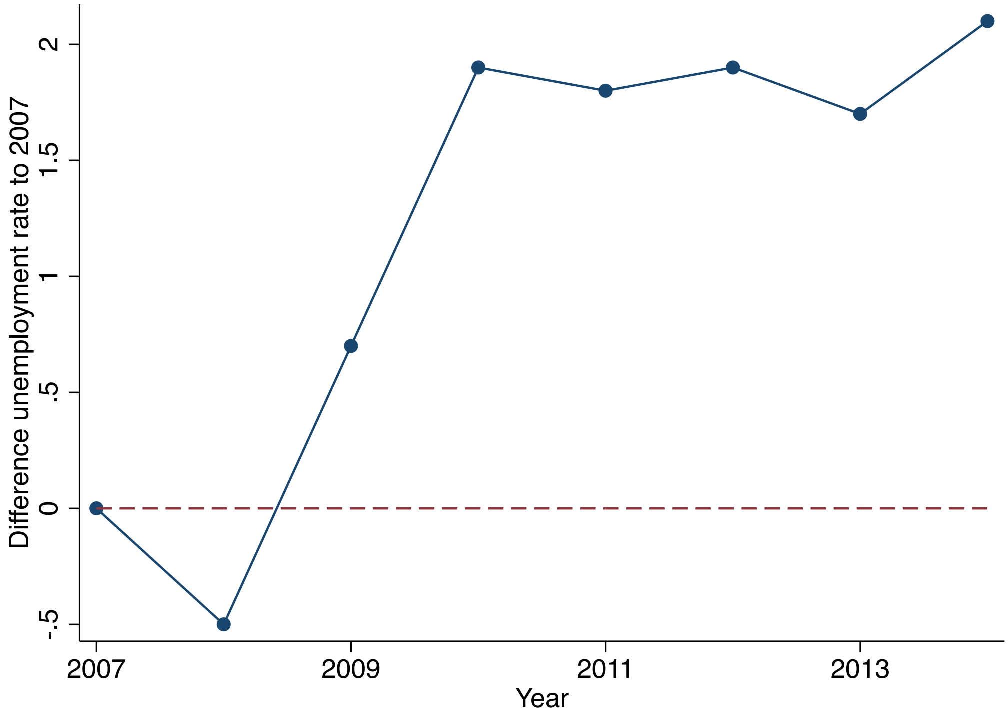


Figure A1: Nationwide Unemployment Rate in South Africa: Difference to 2007

Notes: The figure depicts the annual nation-wide unemployment rate in South Africa as drawn from the IMF Statistics.

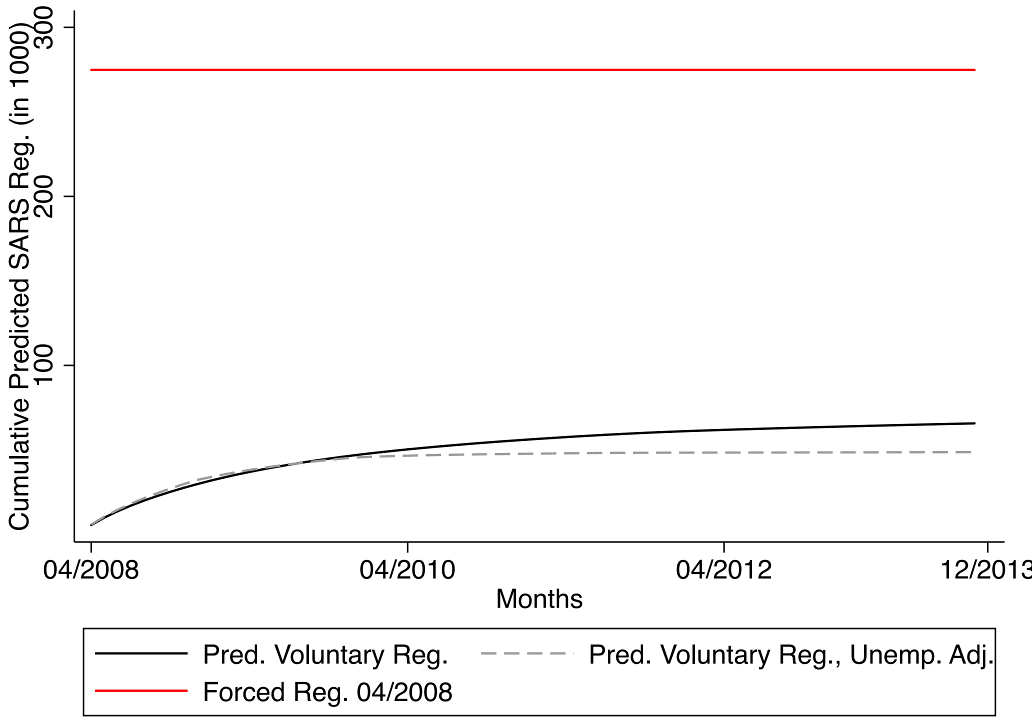


Figure A2: Cumulative Number of Voluntary New Business Tax Registration - 2008 Intervention: Adjustment for Changes in Business Cycle

Notes: The figure depicts the annual nation-wide unemployment rate in South Africa as drawn from the IMF Statistics.

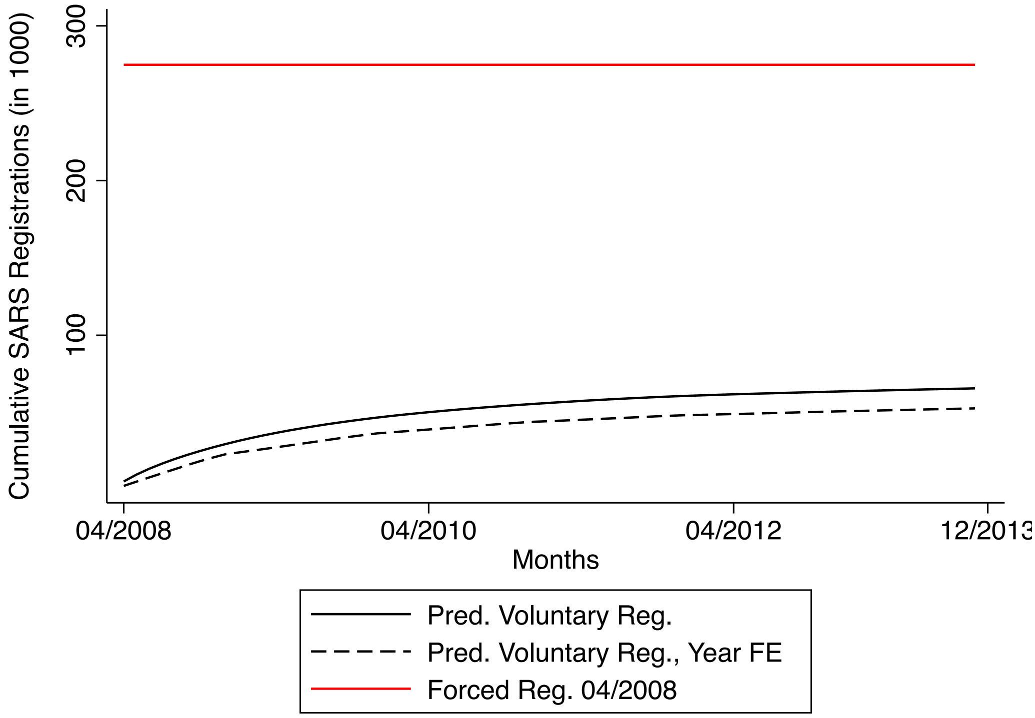


Figure A3: Cumulative Number of Voluntary New Business Tax Registration - 2008 Intervention: Adjustment for Year Fixed Effects

Notes: The figure depicts the annual nation-wide unemployment rate in South Africa as drawn from the IMF Statistics.

Table A1: Voluntary Registration Rates - Unemployment Effect		
	(1)	(2)
Unemployment Rate	-0.0004***	-0.0004***
	(0.0001)	(0.0001)
Unemployment Rate $\cdot$ Reg Lag < 6		0.0006***
		(0.0001)
Unemployment Rate $\cdot$ Reg Lag $6-12$		-0.0001
		(0.0001)
Unemployment Rate $\cdot$ Reg Lag $12-18$		-0.0001
		(0.0001)
GDP pC	-0.0000	-0.0000
	(0.0000)	(0.0000)
Observations	6,889	6,889
R-squared	0.7892	0.7908

Notes: *** p<0.01, ** p<0.05, * p<0.1. All specification include a full set of year fixed effects indicating the year of SARS registration and the year of CIPC registration, a full set of fixed effects for the registration lag in months.

Table A2: Voluntary Registration Rates - Year Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Registration Lag	<6	6-12	12-18	18-24	24-30	30-36	36-42	42-48	48-54	54-60	60-66
Year 2008	-0.0045										
	(0.0111)										
Year 2009	0.0063	0.0004	-0.0016*								
	(0.0083)	(0.0014)	(0.0009)								
Year 2010	-0.0088	-0.0029	-0.0003	-0.0008*	0.0001						
	(0.0065)	(0.0019)	(0.0007)	(0.0004)	(0.0005)						
Year 2011	0.0027	-0.0006	-0.0018***	-0.0017***	-0.0003	-0.0008**	-0.0011**				
	(0.0083)	(0.0012)	(0.0005)	(0.0006)	(0.0004)	(0.0004)	(0.0005)				
Year 2012	0.0369***	0.0114***	0.0003	-0.0006*	-0.0008***	-0.0011**	-0.0012**	-0.0016***	-0.0006		
	(0.0065)	(0.0019)	(0.0007)	(0.0003)	(0.0003)	(0.0004)	(0.0005)	(0.0004)	(0.0004)		
Year 2013	0.0209***	0.0097***	0.0067***	0.0075***	0.0005	-0.0002	-0.0008***	-0.0014***	-0.0004	0.0000	0.0003***
	(0.0057)	(0.0011)	(0.0005)	(0.0006)	(0.0004)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0002)	(0.0001)
Observations	265	282	246	204	182	140	145	93	67	54	50
R-squared	0.1793	0.3345	0.5244	0.5346	0.0769	0.0551	0.0829	0.1750	0.0511	0.0004	0.2267

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix B - Empirics: Tax Return Submissions - Additional Analyses

Table B1: Propensity to Submit Tax Returns - Registrations 04/2008

	(1)	(2)	(3)
Registrations 04/2008	-0.1516***	-0.1505***	-0.1561***
	(0.0110)	(0.0048)	(0.0084)
Constant	0.2502***	0.3979***	0.3304***
	(0.0147)	(0.0152)	(0.0118)
Observations	7,930,609	7,930,609	
271,560			
R-squared	0.0216	0.0470	0.0291
Controls	No	Yes	No
Sample	All	All	Active

Notes: Standard errors in parentheses, clustered at the area level. *** p<0.01, ** p<0.05, * p<0.1. Specifications are run in full sample (Specifications 1 and 2) and for a sample of firms which are, with a very high propensity, active at the time of the intervention and in the considered tax year (specification 3). In Specifications 1 and 2, the sample is restricted to firms which registered with CIPC between 2001 and 2008 and are deemed to be active in a given tax year. In Specification (3) the sample is restricted to firms which registered with CIPC between April 2007 and February 2008 and return submission propensities are compared for the tax year 2009. Dummy indicating whether firm i submitted a tax return in year t. 'Registrations 04/2008' is a dummy variable indicating firms that were identified as 'semi-formal' in SARS's 2008 commercial registry comparison. 'No'-entries in the 'Control'-row means that no control variables are added. In Specification 2, we added registration year fixed effects, tax year fixed effects as well as area fixed effects (indicating tax authority districts).

Table B2: Propensity to Submit Tax Returns - Registrations 02/2014			
	(1)	(2)	(3)
Registrations 02/2014	-0.2052***	-0.2572***	-0.1834***
	(0.0068)	(0.0064)	(0.0055)
Constant	0.2867***	0.2561***	0.3339***
	(0.0052)	(0.0067)	(0.0072)
Observations	3,500,199	3,500,199	218,002
R-squared	0.0361	0.0635	0.0428
Controls	No	Yes	No
Sample	All	All	All
Adjusted R-squared	0.0361	0.0635	0.0428
Sample	All	All	Active

Notes: Standard errors in parentheses, clustered at the area level. *** p<0.01, ** p<0.05, * p<0.1. Specifications are run in full sample (Specifications 1 and 2) and for a sample of firms which are, with a very high propensity, active at the time of the intervention and in the considered tax year (specification 3). In Specifications 1 and 2, the sample is restricted to firms which registered with CIPC between 2009 and 2013 and are deemed to be active in a given tax year. In Specification (3) the sample is restricted to firms which registered with CIPC between February 2013 and December 2013 and return submission propensities are compared for the tax year 2014. Dummy indicating whether firm i submitted a tax return in year t. 'Registrations 02/2014' is a dummy variable indicating firms that were identified as 'semi-formal' in SARS's 2014 commercial registry comparison. 'No'-entries in the 'Control'-row means that no control variables are added. In Specification 2, we added registration year fixed effects, tax year fixed effects as well as area fixed effects (indicating tax authority districts).

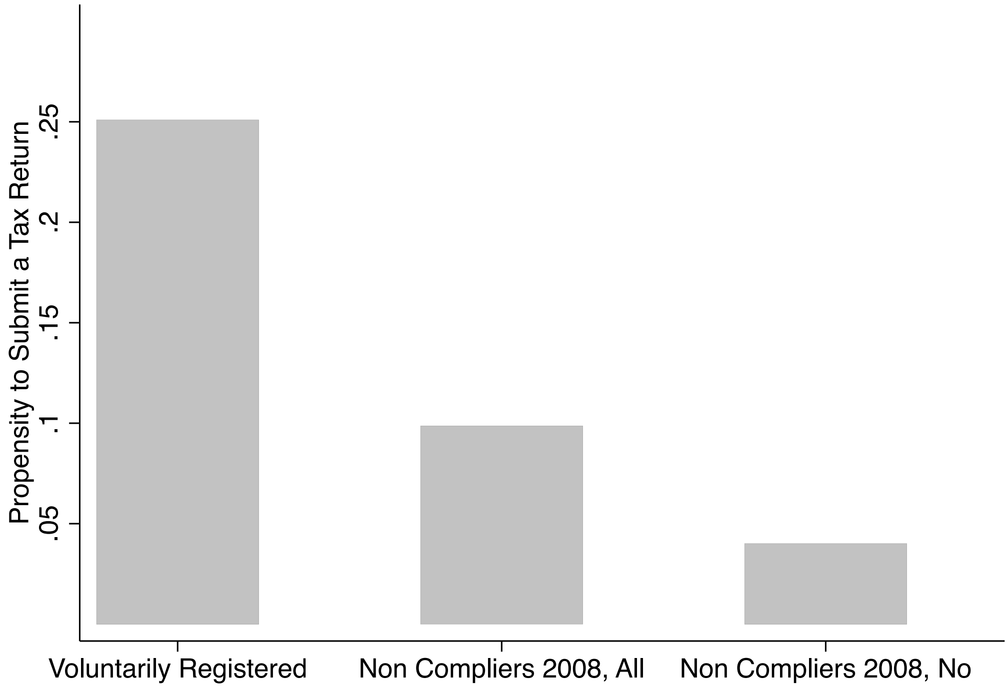
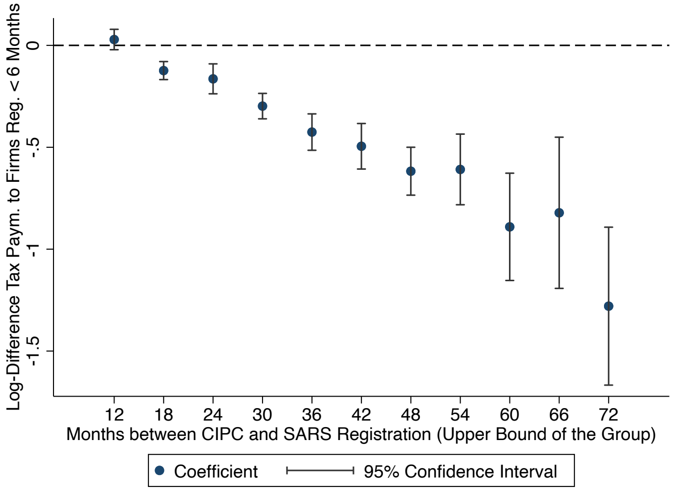
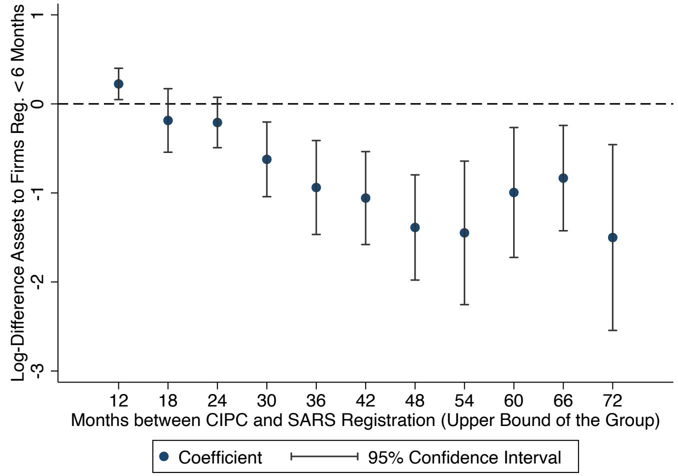


Figure B1: Propensity to Submit Tax Returns: All Identified Non-Compliers in 2008 vs. Firms That Would Have Never Voluntarily Registered

Appendix C: Tax Revenues - Additional Analyses



Late Registration and Tax Payments



Late Registration and Size

Figure C1: Late Registered Firms: Size and Tax Payments

Table C1: Registrations 04/2008, BASE				
	(1)	(2)	(3)	(4)
Registry Comparison 2008	-0.7988***	-0.5068***	-0.4743***	-0.0593*
	(0.0289)	(0.0403)	(0.0282)	(0.0295)
Log Assets				0.2144***
				(0.0087)
Observations	1,222,140	1,222,140	1,221,933	1,221,933
R-squared	0.0028	0.0151	0.0625	0.1506
Registration Year FE	No	Yes	Yes	Yes
Other Control Variables	No	No	Yes	Yes
Adjusted R-squared	0.00279	0.0151	0.0625	0.151

Note: The other control variables comprise tax return year fixed effects, area code and industry code fixed effects.

Table C2: Registrations 04/2008, By CIPC Registration Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Reg. Year	2004	2004	2005	2005	2006	2006	2007	2007	2008	2008
Registry Comparison 2008	-0.9790***	-0.2872***	-0.8757***	-0.2308***	-0.5739***	-0.0728	-0.3209***	0.0110	0.0863	0.0697
	(0.0501)	(0.0467)	(0.0471)	(0.0461)	(0.0541)	(0.0515)	(0.0318)	(0.0383)	(0.0575)	(0.0533)
Log Assets		0.2187***		0.2081***		0.2007***		0.1976***		0.2055***
		(0.0089)		(0.0097)		(0.0098)		(0.0077)		(0.0080)
Observations	137,478	137,478	179,613	179,613	198,702	198,702	219,596	219,596	157,255	157,255
Adjusted R-squared	0.0449	0.127	0.0534	0.140	0.0605	0.153	0.0603	0.158	0.0846	0.197

Notes: The specifications reestimate the baseline model by registration year.

Table C3: Registrations 02/2014 plus PPML and Binary Dependent

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	PPML	PPML	Bi	Bi
Registry Comparison 02/2014	-0.5935**	-0.8256***	-0.3921***	-0.4967***	-0.0672***	-0.0899***
	(0.2325)	(0.2370)	(0.1401)	(0.1118)	(0.0241)	(0.0256)
Log Assets		0.2147***		0.2730***		0.0244***
		(0.0060)		(0.0180)		(0.0005)
Observations	289,610	289,610	289,491	289,491	298,240	298,240
Controls Yes	Yes	Yes	Yes	Yes	Yes	

Notes: Control variables included in all specifications: indicators for tax year, for CIPC registration years, area codes industry fixed effects.