APPENDIX FOR ONLINE PUBLICATION

For

TAXATION, INFORMATION AND WITHHOLDING:

EVIDENCE FROM COSTA RICA*

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Contents

Appendix **1**

A Choice of Estimation Method . . . 1

A.1 Disadvantages of the Log-Linear Model . . . 1

A.2 Alternative 1: Log-Linar Estimation on Collapsed Data . . . 3

A.3 Alternative 2: Linear Estimation . . . 4

A.4 Alternative 3: PPML Estimation . . . 4

A.5 Comparison of Alternatives . . . 6

B The Impact of Information Reporting . . . 7

B.1 Heterogeneity in Bunching . . . 7

B.2 Event Study . . . 13

C The Anatomy of Compliance . . . 25

C.1 The Extensive Margin . . . 25

C.2 The Intensive Margin . . . 29

C.3 The Tax Remittance Margin . . . 33

D Algorithm to Estimate Non-Filing . . . 36

D.1 Income Tax . . . 36

D.2 Sales Tax . . . 37

E Data Appendix . . . 39

F The Impact of Withholding – Robustness . . . 42

G The Impact of Withholding – Mechanisms . . . 52

H Event Study of Withholding-Rate Reduction . . . 54

Appendix

A Choice of Estimation Method

This appendix explains our choice of estimation method. We first review the disadvantages of the standard log-linear model in our context, and then discuss several alternatives.

A.1 Disadvantages of the Log-Linear Model

A generic version of the models we would like to estimate in this paper is

$$y_{it} = \alpha_i + \gamma_t + \beta \cdot Treat_i \cdot Post_t + \epsilon_{it}, \tag{1}$$

where y is a firm-level outcome, e.g. tax remittance, the treatment $Treat$ is a policy change expected to affect the outcoume, and which is relevant only to a subset of firms, i indexes firms and t indexes time periods. It is standard practice to assume that this is a log-linearized version of an exponential relationship, and run an OLS estimation with a log-transformed outcome on the left-hand-side. This generates a point estimate of β which can be interpreted as a semi-elasticity, i.e. the proportional change in y in response to the treatment, and which is scale-invariant. If the distribution of the original outcome variable is right-skewed, the log-transformation reduces heteroscedasticity in the data and leads to a distribution that resembles the normal distribution more closely, moving towards more consistency in estimated standard errors. For outcome variables which feature zeros or negative values, the inverse hyperbolic sine (IHS) transformation ($(ln(y + sqrt(y^2 + 1))$ has been proposed as a log-like transformation that achieves benefits similar to those of the log transformation (Johnson 1949, Burbidge et al. 1988).[1]

In our case, however, the distribution of our main outcome variables y (e.g. profits, tax liability, tax remittance) is not only highly skewed, but also features an unusually large share of zeros ($>$ 5 percent of observations for some variables, over 30 percent for others). This means that no log-like transformation yields an approximately normally distributed outcome. Both the log and the IHS transform the non-zero part of the distribution, but leave the mass point at zero unchanged. In this case, an OLS estimation with the transformed outcome yields

[1] Compared to the log transformation $ln(y + \epsilon)$, which requires specifying ϵ - usually 1 - and which does not deal with negative values, the IHS transformation is considered less arbitrary. However, this view ignores the fact that the IHS transformation also has a parameter, referred to as θ by Johnson (1949) and as α by Ravallion (2017). The standard assumption is $\theta = 1$, in which case the transformation simplifies to $(ln(y + sqrt(y^2 + 1))$ (as above), but there is no clear theoretical justification for this assumption. Thus, neither the log nor the IHS transformation is non-arbitrary.

coefficients which are biased, scale-variant and not interpretable as a percentage change.

Santos Silva and Tenreyro (2006a) discuss the intuition behind this challenge more generally in the framework of the standard constant elasticity model $y_i = exp(\beta x_i)$. The stochastic version of the model is $y_i = exp(x_i\beta) + \epsilon_i$, with $\epsilon_i = y_i - E(y_i|x)$. In the log-linearized version of this model, the error is typically not independent of the regressor x_i because of Jensen's inequality, i.e. $E(ln(y)) \neq ln(E(y))$. Only under very specific functional forms would the errors in the log-linearized model be independent of the regressor, so that unbiased coefficient estimates can be obtained.

As we show in Table A.1, the results from estimations on data transformed using $ln(y+1)$ or the IHS are very similar. Both are qualitatively consistent with but quantitatively inconsistent with non-parametric evidence and estimation results using more appropriate estimators, which we discuss in the following sections.[2]

Table A.1: Different Estimation Methods

	Log(x+1)	IHS	Log	Linear			PPML
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Collapsed Data	Point Estimate (Absolute Effect)	Pre-Reform Mean Among Treated	Proportional Effect	
Treatment	1.069***	1.158***	0.811***	205.7***	451.7***	0.455***	0.316***
	(0.0406)	(0.0448)	(0.0259)	(43.68)	(14.16)	(0.0966)	(0.0321)
post			0.0541***				
			(0.0192)				
Month FE	Yes	Yes	No	Yes	n/a	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	n/a	Yes	Yes
CharacteristsXMonth	Yes	Yes	No	Yes	n/a	Yes	Yes
Observations	252,648	252,648	13,166	252,648	133,342	282,924	252,000

Notes: This table displays the results from estimating equation 1, using different estimation methods/transformations to the outcome variable as indicated by the column title. The outcome variable is total sales tax remittance, and the treatment indicates whether the firm was subject to the increase in the tax withholding rate in August 2011. The data is a monthly balanced panel of firms filing sales tax during 2010-2012. The data is trimmed at the 99th percentile of total sales. The results in columns 2, 6 and 7 correspond to those in Tables F.3, F.2 and 3, column 2, respectively.

[2]Relatedly, Bellemare and Wichman (2019) show that coefficient estimates from regressions involving IHS transformations can differ substantially from a semi-elasticity, and are sensitive to whether the IHS transformation is applied to the dependent or the independent variable or both.

A.2 Alternative 1: Log-Linar Estimation on Collapsed Data

As discussed by Bertrand et al. (2004) and used in Naritomi (2019), one option of dealing with the presence of zeros in a difference-in-differences estimation is to collapse (i.e. average) the data for the pre-reform and post-reform period at the firm-level, and estimate a simple log-linear difference-in-differences model on the collapsed data. The collapsing ensures that only observations which have a zero outcome for an entire period are dropped from the sample. The procedure also leads to more accurate standard error estimates in serially-correlated data, which is why Bertrand et al. (2004) initially suggested this method. When using this method, the identifying assumption should of course still be verified non-parametrically.

The downsides of working with collapsed data in our context are twofold. First, it does not allow for flexibly controlling for time trends and treatment-group-specific seasonality. As a precaution, we thus use only 12 month before and after the reform, and take out observations in December, as differential changes in December spikes between treatment and control group could disproportionately drive the treatment effect estimates. Second, the strategy faces the same challenges as the standard log-linear model if many observations have zero-outcomes for an entire period (either pre-reform or post-reform). In our context, we still have a significant share of zero observations even after collapsing (see the number of observations in Table F.8 compared to Table 3).[3] This means that the estimates still apply only to a subsample of firms.

This method is thus not our preferred specification, but yields results that are qualitatively similar but still much larger than those from our preferred specification, discussed below.

Instead of aiming to estimate a treatment effect for the full sample, including zero and non-zero observations, we also estimate separately the "extensive" margin effects (reporting a non-zero outcome) and the intensive margin effect. The former is estimates as OLS on a dummy outcome variable, and the latter is estimated as as log-linear model on the observations that consistently (or mostly) report non-zero outcomes pre-reform. This exercise allows us to show that our effect is driven by both extensive and intensive margin responses, but the analysis remains incomplete, as it ignores the share of firms which register both zero and non-zero outcomes before the reform. In addition, the sample on which intensive margin effect can be estimated changes from outcome to outcome. This means that a method which can deal with both zero and non-zero outcomes still preferred.

[3] This is particularly true for the amount of tax withheld, reclaims of tax withheld and taxpayer tax remittance.

A.3 Alternative 2: Linear Estimation

As a second alternative to the log-linear model, we run the estimation as a simple linear model without transforming the outcome variable. If the model is correctly specified, this yields an unbiased estimate of the absolute rather than the proportional effect of the treatment. This absolute effect estimate is useful for our decomposition exercise, in which we decompose the treatment effect on total tax remittance into changes in the components of the tax remittance (gross tax liability, deductions, tax withheld etc). Relying on an absolute effect estimate makes the decomposition simple and transparent. However, we are interested in the proportional effect as well, which we can obtain by scaling the coefficient of interest by the treatment-group mean in the last pre-reform month/year (or, alternatively, the treatment group mean over the entire pre-reform period, which is valid in the absence of time trends). We use the delta rule to obtain standard errors on these transformed point estimates (using boostrap yields very similar results). To ensure our results are not driven by influential outliers, we test robustness to winsorizing the outcome variable at different levels (the 99.9th, 99th and 95th percentile).

The key shortcoming of this strategy is that, if the true model is not linear, the linear estimation yields biased estimates of β. Another disadvantage is that results from an augmented difference-in-difference model with interaction terms (e.g. to estimate differential effects for different subsampls of firms) are difficult to interprete, as subsamples may differ in both absolute and relative effects.

A.4 Alternative 3: PPML Estimation

Given the shortcomings of the linear model, our main and preferred strategy uses the Poisson Pseudo Maximum Likelihood estimator (PPML) suggested by Santos Silva and Tenreyro (2006a). This estimator is based on the constant elasticity model $y_i = exp(\beta x_i) + \epsilon_i$ and the appealing assumption that $V(y_i|x) \propto E(y_i|x) = exp(x_i\beta)$. The estimator maximizes the log-likelihood $\sum_{i=1}^{n}[y_i x_i \beta - exp(x_i b)]$, with first-order condition $\sum_{i=1}^{n}[y_i - exp(x_i\beta)]\, x_i = 0$. The PPML is a consistent estimator for β if the conditional mean is correctly specified, i.e. $E(y_i|x) = exp(x_i\beta)$. Importantly, consistency does not require that the data follow a Poisson distribution (or that the data contain only integers) nor that the variance is correctly speci-

fied.[4],[5]

This estimator coincides with the standard Poisson maximum-likelihood estimator (MLE), which is derived from the assumption of the Poisson distribution where $V(y_i|x) = E(y_i|x)$ - a phenomenon referred to as equidispersion, as opposed to overdispersion where $V(y_i|x) > E(y_i|x)$).[6] The estimator proposed by Santos Silva and Tenreyro (2006a) is a "pseudo" MLE because, unlike standard MLE estimators, it does not require complete specification of the joint density distribution (Gourieroux et al. 1984). The conditional mean is assumed to be correctly specified, while the conditional variance is allowed to be misspecified. Efficiency, however, is achieved only with correct specification of both the conditional mean and conditional variance. If the data is overdispersed, so that the second moment assumption is incorrect, the PPML-estimated standard errors are downward biased (a finding which Ryan et al. (2018) illustrate more generally for Poisson estimators). We thus follow Santos Silva and Tenreyro (2006a)'s recommendation to use robust standard errors (essentially an extension of White (1980) robust standard errors for OLS).[7]

The PPML was devised for consistent estimation of constant elasticity models in multiplicative form, with a focus on the gravity model and characteristics of trade data, namely non-negativity, heteroscedasticity, and the occurence of zeros. The PPML is surprisingly suitable for work with tax data as well, as these data share many features with trade data. Both types of data usually feature a highly skewed distribution, a large share of zeros (country pairs with no trade, and firms with zero tax liability) and non-negativity. Besides, estimation in both trade and public finance usually requires controlling for multi-dimensional fixed effects, which the PPML estimator (and accompanying Stata package) was developed to do. While the PPML is still most popular among trade economists, its application has been extended to topics as diverse as international taxation (Azémar and Dharmapala 2019), R&D investment (Azoulay et al. 2018), drug addiction (Alpert et al. 2018, Powell et al. 2018) and migration (Parsons and Vézina 2018).

The advantages of the PPML over the above-discussed alternatives can be summarized as

[4]Instead of the PPML, one could use the non-linear least squares estimator $\hat{\beta} = argmin \sum_{i=1}^{n}[y_i - exp(x_i b)]^2$, with first order condition $\sum_{i=1}^{n}[y_i - exp(x_i\hat{\beta})]exp(x_i\hat{\beta})x_i = 0$. However, this estimator is inefficient as it gives more weight to large observations which are also the more noisy ones.

[5]In two follow-up papers, Santos Silva and Tenreyro (2011) show that the PPML behaves well even if the share of zeroes in the data is very large and Santos Silva et al. (2014) develop a method to choose between competing models for non-negative data with many zeros.

[6]The model assumed for the PPML, $y_i = exp(x_i\beta)$, is the log-linear version of the poisson regression model.

[7]Alternatively, valid inference can be done with standard errors based on the negative binomial distribution of type 1 or type 2 (if the assumption that the variance is a multiple of the mean or a quadratic function of the mean, respectively, is reasonable), or by bootstrapping (Cameron and Trivedi (1998), chapter 3). Also see Winkelmann (2003) on count data econometrics.

follows:

- All data points, including zeros, are considered in the estimation[8] without the need to make ad hoc transformations such as taking $ln(y+1)$ or the IHS assuming $\theta = 1$. All observations are weighted equally.

- Estimates for β are consistent despite heteroscedasticity, and robust to different patterns of heteroscedasticity, if the conditional mean is correctly specified.

- The estimates are semi-elasticities, so no transformation or scaling is needed for interpretation. This ensures that results from subsample regressions and coefficients on different interactions with the treatment indicator can easily be compared.

For our main estimation of the policy treatment effect on tax remittance, Table A.1 (columns 6 and 7) shows that the PPML estimation and the linear estimation yield very similar results.[9]

A.5 Comparison of Alternatives

All estimation strategies yield biased estimates of β if the conditional mean is misspecified. The linear model has the additional drawback that it requires an ad hoc derivation of the semi-elasticity, the log-linear model on collapsed data cannot take into account the remaining zero-observations, and the PPML estimator with robust standard errors has the additional drawback that it is inefficient. If it is theoretically unclear whether the true relationship between the outcome and the treatment is linear or log-linear, and either of those assumptions is unlikely to hold exactly, it convenes to emphasize results for analyses for which the various estimation strategies yield qualitiatively identical and quatitatively very similar results. Given that the linear model gives more weight to large observations, whereas the PPML treats all observations equally, the emphasized results would also be the ones that are relatively more stable across the sample. The PPML as our preferred method yields the quantiatively smallest and hence most conservative estimates.

[8] The exception are observations in panel data that register a zero outcome for each period, which are dropped by the PPML estimator with fixed effects.

[9] Instead of the poisson estimation, one could consider a negative binomial (NB) estimator, a generalization of the poisson estimator suitable for over-dispersed data. The NB estimator can provide more efficiency if an assumption on the conditional variance (as either a multiple or a quadratic function of the mean) can be correctly specified. However, the efficiency gains come at the cost of a higher risk of inconsistency of $\hat{\beta}$, as consistency requires correct specification of the full density. We therefore do not use the NB estimator in this paper. To the best of our knowledge, there is no Stata package available to implement the negative binomial estimation with high-dimensional fixed effects.

B The Impact of Information Reporting

This section presents estimates of the impact of third-party information on self-reported taxable income. We begin by analyzing the heterogeneity of bunching–a proxy for misreporting–across subsamples of firms with different degrees of third-party information coverage. We then conduct an event study of firms' responses to the first third-party report. Table B.2 presents summary statistics on the different samples of analysis.

B.1 Heterogeneity in Bunching

Table B.1 shows the income tax schedule in Costa Rica, which features kinks for the self-employed individuals and notches for corporations. Numerous studies have used bunching at kinks or notches in the tax schedule to estimate tax base responses to the tax rate. Bunching is usually shown to be driven by tax evasion or avoidance rather than a real response (e.g. Bachas and Soto 2019, Almunia and Lopez-Rodriguez 2018, Seim 2017). In Costa Rica, we observe large and sharp bunching at the first kink for self-employed individuals and at the first notch for corporations. Bunching moves every single year with the location of the kink, as shown in Figure B.1 for self-employed individuals.[10] This speedy adjustment supports the interpretation of bunching as a reporting response rather than a real production change.[11] We thus use bunching as a proxy for misreporting.

To examine the heterogeneity of bunching with the coverage of third-party information, we pool the data for 2006 to 2015 and display the distribution as a percentage difference from the year-specific threshold location in 1 percent bins. To estimate the size of bunching, we fit a flexible polynomial to the observed distribution, excluding a range around the thresholds, as is standard in the bunching literature (Chetty et al. 2011, Kleven and Waseem 2013). Given the asymmetric nature of bunching, we estimate bunching to the left of the kink and the missing mass to the right of the kink. As the missing mass does not seem to be the same size as the excess mass, at least for self-employed filers, we apply the estimation strategy suggested by Best and Kleven (2018) rather than the convergence method.[12]

[10] We focus on the first threshold as it is the most salient one, and also the largest in terms of the tax rate change for the self-employed.

[11] Strikingly, the excess mass is always concentrated to the left of the kink, and in some years the distribution exhibits a missing mass to the right of the kink. Such asymmetric bunching at kinks is at odds with the prediction of standard utility theory and might instead reflect reference-point dependence (Kleven 2016). While caution should be exercised when using bunching to estimate the elasticity of taxable income, this does not prevent us from interpreting bunching as a measure of misreporting that generates a revenue loss for the government.

[12] We choose the lower bound of the excluded range as the point where bunching starts and the upper bound as the point where the derivative of the observed distribution shifts from positive to negative. The convergence method would require the missing mass and the excess mass to be of the same size and assumes that there are no extensive-margin responses, which is unlikely in our context due to the large share of non-filers.

Figure B.2 displays the observed distribution (dotted blue line), the estimated counterfactual (solid red line), and excess-mass estimates for different sub-samples of the self-employed individuals (row A) and corporations (row B). Among both firm types, the largest excess mass is found in the sample of firms not subject to third-party reporting (panels A1 and B1). The subsample of firms subject to third-party reporting (panels A2 and B2) still exhibits a large excess mass around both the kink and the notch, but in both cases the excess-mass estimate is significantly smaller than the estimate for firms not subject to third-party reporting. The excess mass drops from 4.5 to 2.08 for self-employed individuals and from 4.49 to 3.17 for corporations, and those changes are statistically significant at the 1 percent level.[13] The fact that bunching is smaller but still highly significant, among firms subject to third-party reporting is consistent with the fact that the information trail is incomplete, and that firms could still bunch through legal tax avoidance.

Third-party reporting by state institutions and credit-card companies, which also act as withholding agents, is associated with a further reduction in misreporting (panels A3 to B4). For self-employed filers, the excess mass among firms subject to state reporting is similar to the excess mass among firms subject only to third-party reporting by other firms, but the excess mass drops to 0.52 for firms subject to reporting by credit-card companies. For corporations, the excess mass drops to 1.44 and 1.35, respectively, for firms subject to reporting by state institutions and credit-card companies. Once again, these changes are highly statistically significant. While the heterogeneity of bunching across subsamples captures a correlation rather than a causal relationship, it is consistent with a compliance impact of third-party information reporting and an even stronger impact of withholding.[14]

[13]Note that the change in the missing-mass estimate is driven by a change in the counterfactual density that scales the excess mass, rather than by a change in the absolute size of the excess mass. The missing mass drops for corporations, but increases for self-employed individuals. In fact, the missing mass for the latter is clearly visible only in panels A2 to B3. This suggests that some self-employed individuals in subsample may erroneously perceive the threshold to be a kink not covered by third-party reporting.

[14]Our results are also consistent with estimates from the United States, where the Internal Revenue Service reports tax evasion rates of 63 percent, 7 percent and 1 percent, respectively, on income covered by little third-party reporting, income covered by substantial third-party reporting, and income subject to withholding (IRS 2016).

Table B.1: Income Tax Schedule

	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
Panel A: Self-Employed										
Kink 1	1,858	2,074	2,252	2,599	2,747	2,890	3,042	3,171	3,339	3,522
Kink 2	2,775	3,097	3,362	3,880	4,102	4,316	4,543	4,735	4,986	5,259
Kink 3	4,629	5,167	5,609	6,473	6,843	7,199	7,577	7,898	8,317	8,773
Kink 4	9,276	10,354	11,241	12,972	13,713	14,427	15,185	15,827	16,667	17,581
Panel B: Corporations										
Notch 1	27,811	31,043	33,701	38,891	41,112	43,253	45,525	47,451	49,969	52,710
Notch 2	55,943	62,444	67,791	78,231	82,698	87,004	91,573	95,447	100,513	106,026

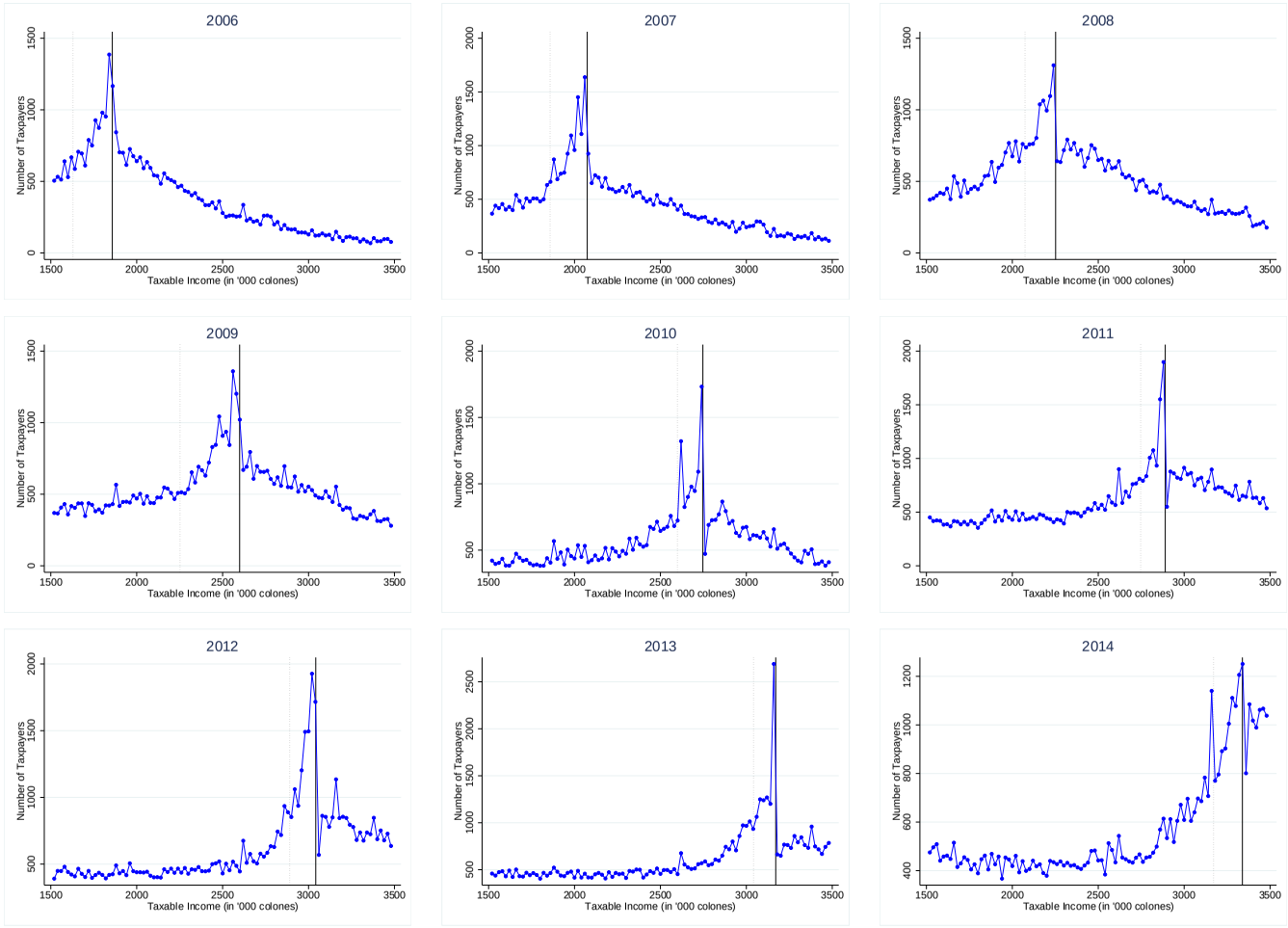
Notes: This table shows the income tax schedule for the years 2006 to 2015. Amounts are in thousands of CRC (1USD=573CRC). Panel A shows the location of the kinks on taxable income that separate the five tax brackets for the self-employed. The tax is applied to taxable income at marginal rates of 0, 10, 15, 20 and 25 percent respectively for the first to fifth tax bracket. Panel B shows the location of the notches on revenue that separate the three tax brackets for corporations. The tax is applied to taxable income at average rates of 10, 20 and 30 percent respectively for the first to third tax bracket. For more information on the tax base, tax schedule and the filing procedure, see http://www.hacienda.go.cr/contenido/12994-regimen-tradicional.

Table B.2: Summary Statistics for Different Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2010				2013			
	Turnover	Profit Rate	Years in Data	% Corporation	Turnover	Profit Rate	Years in Data	% Corporation
Panel 1: All Income Tax Filers								
Mean	83	28.1	1.9	41.5	75.7	34.4	3.1	35.1
P50	4.3	14.4	1	.	4.2	22.6	2	.
P90	77.8	75.7	4	.	72	90.8	7	.
N	355354	251745	355354	311800	402964	291053	402964	397610
Panel 2: Income Tax Filers in Bunching Sample								
Mean	24	39.2	1.8	21.5	21.1	41.7	2.9	18.9
P50	11.3	31.3	1	.	9.7	36.2	2	.
P90	55.9	76	4	.	51.4	82.6	7	.
N	90798	90479	90798	90798	115406	115405	115406	115406
Panel 3: Income Tax Filers in Event Study								
Mean	122.1	27.2	1.8	37.5	121.9	29.9	1.8	37.5
P50	8.9	13.5	1	.	8.6	17.5	1	.
P90	131.6	75	4	.	130.5	75	4	.
N	218952	189670	218952	218097	218952	180187	218952	218097
Panel 4: All Sales Tax Filers								
Mean	261.6	10.4	1.9	55.8	240.8	12.6	3	50.4
P50	19.4	3.2	1	.	18.7	4.5	1	.
P90	341.1	33.5	4	.	317.9	36.6	7	.
N	67734	56646	67734	59566	75018	63402	75018	74927
Panel 5: Regular Sales Tax Filers								
Mean	460.3	8.7	1.4	59.4	344.7	10.7	2.3	55.7
P50	49.2	3.1	1	.	41.3	4.3	1	.
P90	731.6	24.7	3	.	520.4	28.9	6	.
N	27710	25353	27710	27541	38451	35494	38451	38435
Panel 6: Irregular Sales Tax Filers								
Mean	124.1	11.9	2.2	52.7	131.6	15.1	3.7	44.9
P50	9.7	3.4	1	.	7.3	4.9	3	.
P90	162.6	41.5	5	.	135	48	8	.
N	40024	31293	40024	32025	36567	27908	36567	36492

Notes: The variables are from the 2010 (2013) income tax declaration. Turnover is in 2015 constant CRC millions, profit rate in percent. The samples are defined as follows: (1) all income tax filers; (2) income tax filers in the bunching sample, i.e. corporations (self-employed) with revenue (profits) within [-50 percent,+50 percent] around the first notch (kink) in the tax schedule; (3) income tax filers that experienced at least one of the events analyzed in the event study during 2009-2012 (4) all sales tax filers; (5) regular sales tax filers, filing each month within a 36 months window around July of the relevant year, [July of t-1, June of t+2] i.e. the equivalent of the sample used in the withholding DiD; (6) similar to (5), but retaining only firms that file at least once but not each month within the 36-months window.

Figure B.1: Bunching at the First Income Tax Kink for Self-Employed Individuals

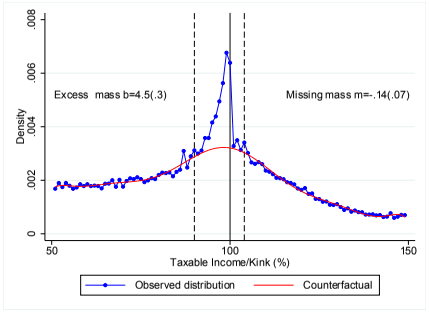


Notes: This figures show the frequency distribution of taxable income of the self-employed (personas fisicas con actividad lucrativa) around the first kink in the income tax schedule, for the years 2006-2014. The data is aggregated in bins of CRC 20,000. The black solid line marks the kink in year t (as per the figure title), the black dashed line marks the kink in year $t-1$.

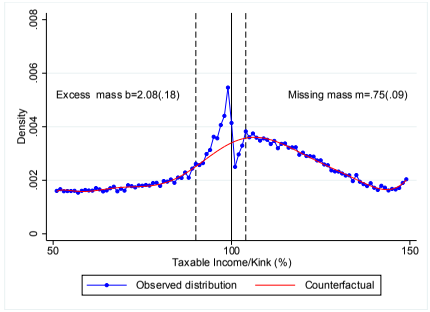
Figure B.2: Bunching, Information Reporting and Withholding

A: Self-Employed

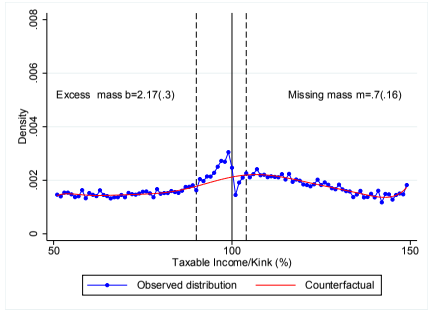
A1: No Information Reporting



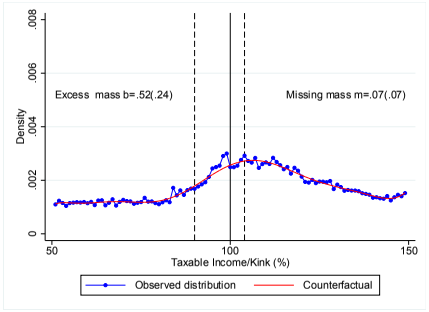
A2: Reporting by Firms



A3: Reporting by State

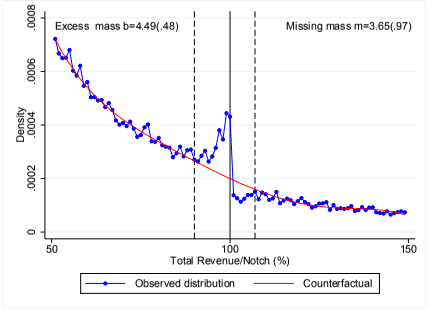


A4: Reporting by Credit/Debit Card

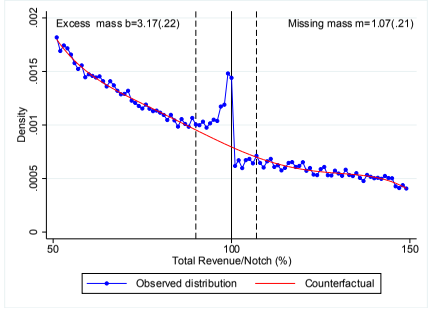


B: Corporations

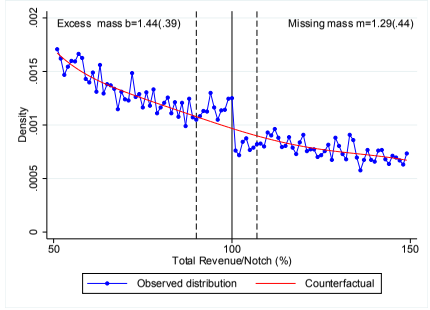
B1: No Information Reporting



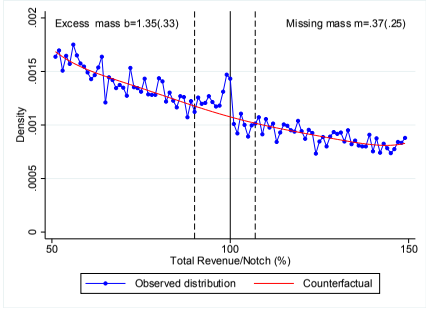
B2: Reporting by Firms



B3: Reporting by State



B4: Reporting by Credit/Debit Card



Notes: This figure shows the density distribution of taxable income for the self-employed around the first kink in the income tax schedule (top row, A), and the density distribution of total revenue for corporations around the first notch in the corporation tax schedule (bottom row, B). The data is pooled for years 2006-2015, represented as percentage distance from the kink, and aggregated in taxable income bins of 1 percent. The panels show the distribution for different subsamples, as per the panel titles that indicate whether and by what type of information reporting firms in the subsample are covered.. The blue dotted line marks the empirical distribution, the red solid line marks the counterfactual, fitted as a flexible polynomial to the observed distribution outside the excluded range. We use an tenth-degree polynomial for self-employed and a sixth-degree polynomial for corporations. The excluded range above the threshold covers four and seven bins respectively in the two groups of firms. The excess mass b and missing mass m are estimated as the difference between the observed and estimated density, weighted by the height of the counterfactual density. The standard errors are bootstrapped.

B.2 Event Study

To move towards estimating a causal effect of information reporting, we exploit within-firm variation across time in the coverage of information reporting. Each year, over a thousand Costa Rican firms become subject to third-party reporting for the first time. Our conceptual framework predicts that, among firms which correctly perceive the enforcement parameters, third-party reporting of sales R_T imposes a lower bound on reported taxable sales $\hat{R}$, and an increase in R_T weakly increases reported sales and profits.

We thus conduct an event study of firm behavior around the time of its first third-party report, distinguishing reports by the different informing agents, which may be other firms, state institutions, or credit-card companies. As we seek to identify a reporting rather than a real response to the information reports, we are mindful of two identification challenges. First, the receipt of a first information report may coincide with a real growth acceleration the firm is experiencing. Second, the event leading to the first information report may itsself cause a growth acceleration. This is most relevant for the receipt of a first information report by a state institution and card company, which are generated by the award of a public procurement contract and the adoption of a credit card machine respectively.[15] We argue below that our estimates capture a reporting rather than a real response, because of the sharpness and large size of the response, the fact that almost the entire response is realized immediately in the event period after otherwise parallel trends between event-group firms and control-group firms, and the absence of a trend-break in the wage bill, a real outcome unlikely to be misreported.

Our main specification considers the event group E of firms that become subject to third-party reporting for the first time at event time $k = 0$ and a control group C of firms that have not become subject to third-party reporting by $k = 0$. As a precaution, but without substantively modifying the estimates, we follow Hilger (2016) and Naritomi (2019) in re-weighting the control group to match the treatment group. We estimate each firms' propensity score of being reported by a third party for the first time in $k = 0$, and then re-weight the control group by quintile bins of the propensity score to match the propensity-score distribution of the event group, following DiNardo et al. (1996).[16] We consider a balanced sample of firms we can observe for at least four periods before and three periods after the event. This means that

[15] Incomplete compliance by firms with third-party reporting obligations is not a concern for our analysis, which seeks to identify the impact of actual (observed) third-party reports rather than that of reporting obligations, as the former is the policy-relevant effect.

[16] The weight is thus constant within a firm over time. The propensity score is estimated separately for each of the four different third-party reports, using firm-type and tax-administration dummies and the two lags of a third-order polynomial of total income and taxable income. See Yagan (2015) for a detailed description of the re-weighting procedure.

we use events in event periods $p = \{2010, 2011, 2012, 2013\}$(for the income tax) and between February 2009 and August 2014 (for the sales tax).

Table B.3 illustrates why estimates from a balanced panel are most meaningful. The definition of the event requires that a firm is economically active, which is correlated with filing a tax declaration. A substantial fraction of firms only start filing at or after the event, and a few firms file prior to the event but not afterwards. Moving from a balanced to an unbalanced panel thus increases the sample size but adds little useful variation. Moving from a panel in which a firm appears at least once before and after the event to a fully balanced panel around the event has little effect on the number of observations. We thus focus on the balanced panel in our main results. The appendix shows robustness of our results to numerous other specifications.[17]

Our main results are shown in Figure B.3. Each panel displays the trend in reported taxable income for the event group (orange dots) and the control group (blue crosses), scaled by the pre-event average, along with the difference-in-differences coefficient obtained from estimating

$$y_{i_p k} = \gamma_k + \alpha_{i_p} + \beta \cdot I\{k \geq 0, g = E\} + u_{i_p k}. \tag{2}$$

The unit of observation in this estimation is a firm i in event period p at event time k. We estimate the firm's reported taxable income as a function of event-time dummies γ_k, firm-event-period fixed effects α_{i_p}, and the post-event and treatment group dummy $I\{k \geq 0, i \in E\}$.[18] A challenge for all estimations in this paper is that our preferred outcome variables (tax base, tax liability or tax remittance) take the value zero for a large fraction of observations, which means that estimates from a log or inverse hyperbolic sine transformation are difficult to interpret. To obtain an estimate that is equivalent to a percentage effect, we use the Poisson Pseudo Maximum Likelihood (PPML) estimator pioneered by Santos Silva and Tenreyro (2006b) throughout the paper. Appendix A discusses this choice and the properties of this estimator.[19]

[17] First, we show that the results hardly change when dropping the propensity score reweighting (Figure B.4), when considering shorter pre- and post-event periods which means considering a larger number of events over more event periods (Figure B.5), and when considering a semi-balanced panel (in which case there is also no propensity score reweighting, Figure B.6). To further explore the robustness of the results to unbalanced or semi-balanced samples, Table B.4 reports the difference-in-difference estimates from all event studies, for five different samples, adopting the preferred specification presented in this section, but without propensity score reweighting. Table B.5 does the same for the specifications with shorter pre- and post-periods (as in Figure B.5). These tables show that the effects are robust to all sample definitions, and that our preferred estimates from the balanced sample are in fact on the lower end.

[18] A firm could, for example, be in the control group for events happening in 2010 and 2011, but in the treatment group for events in 2012. Each firm-year observation for this firm would appear in the event dataset three times, for event years 2010, 2011, and 2012. Firm-event-period fixed effects and clustering of standard errors at the firm level account for the potentially repeated appearance of firm-year/firm-month observations.

[19] Our estimates are qualitatively robust to running an OLS estimation on log or IHS-transformed data, and are quantitatively very similar when running an OLS estimation on untransformed data, and scaling the coefficient by the pre-event mean to obtain a proportional effect (see Figure B.7).

For most firms, the first transaction partner that reports to the tax authority is a supplier providing a report about the firm's purchase. As panels A1 and B1 in Figure B.3 show, this first third-party report is associated with a 20 percent increase in reported taxable income for self-employed individuals and a 40 percent increase for corporations. This large effect emerges precisely at event time, after otherwise similar trends in the event and control groups. Almost the entire treatment effect materializes in the event period, after which the event and control group return to parallel trends. This is perfectly consistent with a reporting response but difficult to reconcile with a real growth effect, which would emerge less suddenly and prove more persistent.

Over time, firms gradually become subject to more third-party reports, including reports from their clients. This event, which happens on average one year after the first supplier report, is considered in panels A2 and B2. The first client report is still associated with an increase in reported taxable income but the deviation is now less sharp. This is likely because firms receiving the first client report have already become more compliant when receiving the first supplier report, so that the new report does not provide much additional information. A sharp deviation at event time emerges again when firms receive the first report by a state institutions, or by a card company, which raises reported taxable income by 20-23 percent (panels A3-B4). Although most firms that become subject to reporting by a state institution or a credit-card company are already subject to reporting by other firms, these new reports expand the coverage of third-party reporting to transactions that were previously not reported, and should therefore have an additional effect on the self-reported tax base.[20]

In all figures, the event and control group follow almost identical trends in the pre-event period, and then diverge precisely at event time $k = 0$[21], until the difference between the two groups stabilizes at approximately $k = 1$. It is particularly striking that the sharp deviation at event time can be observed even in the sales tax data with monthly frequency (panels A4 and B4). In further robustness tests (available upon request), we find that the of pattern results – a sharp deviation at event time and a large increase in the reported tax base/liability in the event group – is still present after controlling for the wage bill or the number of employees. Neither of these two proxies of firm size changes discontinuously at event time. We thus conclude that

[20]Figure B.7 shows that, consistent with the fact that firms under-report both sales and costs, the taxable-income response to all events is driven by a similarly-sized percent increase in reported sales and reported costs. The increase in reported taxable income is also associated with an increase in the reported profit rate. For reporting by credit-card companies (last two panels), we use the reported tax liability (rather than reported taxable income) as the main outcome, and use sales tax collected and input tax credits deducted (rather than sales and costs) for the decomposition, as sales and costs are not available on monthly sales tax declarations.

[21]The only exception to this pattern are corporations receiving a first client report from other firms (fourth panel), whose trend diverges from the control group at $k = -1$ rather than $k = 0$.

the size and timing of the effect is hard to reconcile with a pure growth effect, and must be largely driven by a compliance response to information reporting.

Table B.3: Timing of Events (First Third-Party Report) and First Tax Filing

	(1)	(2)	(3)
	First Report Supplier	First Report Client	First Report State
Share started filing in event year	.39	.17	.18
Share filing before and after event	.39	.48	.7
Share filing before event only	.05	.11	.04
Share filing after event only	.18	.24	.08

Notes: This table examines firms' filing behavior around the timing of the main events considered in the event studies if Figure B.3. The sample includes firms experiencing the event (first third-party report) in 2010-2013 and filing at least once during 2006-2016.

Table B.4: Event Study of Third-Party Reporting: No Propensity-Score Reweighting, Long Panel

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Self-Employed				Corporations			
	Event of First Third-Party Report By							
	Supplier	Client	State	Card	Supplier	Client	State	Card
1: Balanced Sample								
DD	.203	.094	.216	.199	.344	.188	.207	.221
SE	.009	.005	.009	.016	.035	.022	.024	.023
N event group	8810	27460	4408	5215	5827	9447	3003	5160
N control group	21565	37347	116919	529890	20145	22083	83476	532355
2: Unbalanced Sample, Missings Dropped								
DD	.242	.108	.254	.267	.433	.275	.276	.315
SE	.007	.004	.008	.014	.027	.018	.02	.022
N event group	31220	63268	9329	7803	14827	20601	5842	7586
N control group	58199	95896	260701	640616	61155	61691	172274	625759
3: Semi-Balanced Sample: Missings Replaced by Zero								
DD	.356	.102	.26	.257	.374	.208	.232	.255
SE	.01	.005	.009	.016	.033	.02	.023	.023
N event group	20027	52944	7394	6170	8835	13568	4031	5971
N control group	47196	75070	225234	645234	33332	34325	115436	632621
4: Semi-Balanced Sample: Missings Replaced by Zero for Max Three Consecutive Years								
DD	.321	.088	.262	.218	.368	.203	.229	.231
SE	.01	.005	.009	.015	.033	.02	.023	.022
N event group	17688	48763	7119	5809	8439	13087	3983	5713
N control group	40572	65204	208362	598623	30949	32095	112181	598254
5: Unbalanced Sample of Firms Filing at Least Twice Pre-Event, Missings Dropped								
DD	.272	.119	.288	.278	.441	.283	.295	.317
SE	.008	.005	.008	.015	.028	.018	.021	.022
N event group	22330	61727	8109	7360	13657	20641	5521	7248
N control group	57078	96000	249835	654899	62739	64639	174975	637775

Notes: This table displays difference-in-differences estimates from event study specification 2, standard errors and the number of observations in the event and control groups. All parameters of the specification are as in the main results in Figure B.3, except that the control group is not propensity-score-reweighted. Table B.5 provides identical results for a shorter period (three pre-event years/months and two post-event years/months). We conduct the estimation for five different panels, as indicated by the sub-table titles: 1) the fully balanced panel; 2) the unbalanced sample in which missing observations are dropped; 3) and 4) two different semi-balanced panels described in more detail below; and 5) an unbalanced sample of firms that file at least twice prior to the event, and in which we again drop missing observations. The semi-balanced panel in sub-table 3 is constructed in the following way: we sort the data by firm-year/month, replace by zero any missing outcome variables that are followed and preceeded by non-missings for the same firm, drop the remaining missing observations, and retain those firms that are now in the data in all four periods before and three periods after the event. In panel 4, we impose the additional restriction that we do not replace missings by zeros for a spell of missing periods longer than three years/months.

Table B.5: Event Study of Third-Party Reporting: No Propensity-Score Reweighting, Short Panel

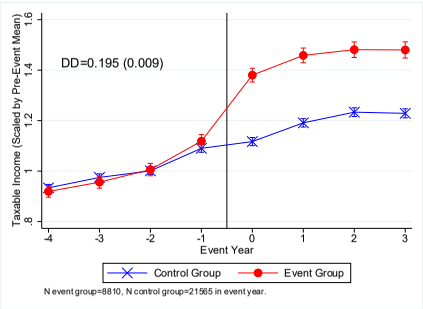
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Self-Employed				Corporations			
	Event of First Third-Party Report By							
	Supplier	Client	State	Card	Supplier	Client	State	Card
1: Balanced Sample								
DD	.213	.089	.215	.21	.38	.147	.173	.242
SE	.006	.004	.007	.016	.025	.014	.018	.024
N event group	18357	54367	7949	5896	12289	21917	5934	5827
N control group	44386	77324	226661	560378	45154	48513	161300	557042
2: Unbalanced Sample, Missings Dropped								
DD	.25	.098	.252	.26	.416	.194	.211	.304
SE	.005	.003	.006	.015	.021	.013	.016	.024
N event group	47484	93608	13819	7803	22455	34261	8750	7586
N control group	87483	142926	391715	641035	87719	89166	249437	625301
3: Semi-Balanced Sample: Missings Replaced by Zero								
DD	.431	.101	.282	.266	.438	.179	.213	.273
SE	.007	.004	.008	.016	.024	.014	.017	.024
N event group	40484	93290	12956	6785	17723	28933	7569	6592
N control group	91442	142259	403373	669883	68102	69234	212445	649849
4: Semi-Balanced Sample: Missings Replaced by Zero for Max Three Consecutive Years								
DD	.407	.096	.279	.234	.421	.176	.213	.252
SE	.007	.004	.008	.016	.024	.014	.017	.024
N event group	38046	89200	12685	6535	17279	28433	7528	6402
N control group	80642	126382	379588	624113	64643	65755	207256	617188
5: Unbalanced Sample of Firms Filing at Least Twice Pre-Event, Missings Dropped								
DD	.276	.114	.279	.273	.417	.203	.228	.303
SE	.006	.004	.007	.015	.022	.014	.017	.024
N event group	32647	89419	11719	7305	20432	33991	8260	7185
N control group	83507	139486	366676	648472	88755	91016	251341	631205

Notes: This table is identical to Table B.4, but considers a shorter period (three pre-event years/months and two post-event years/months).

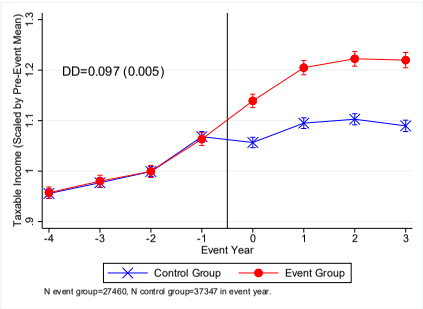
Figure B.3: Event study of First Information Report

A: Self-Employed

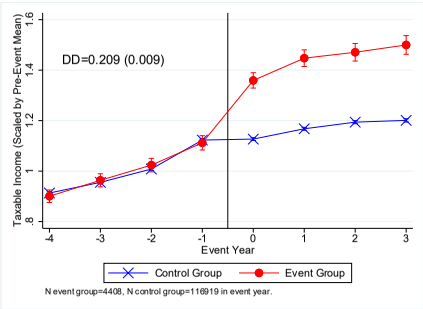
A1: Report by Supplier



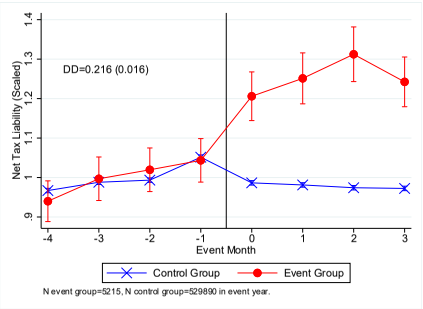
A2: Report by Client



A3: Report by State

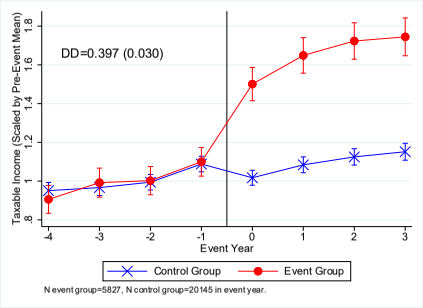


A4: Report by Credit/Debit Card

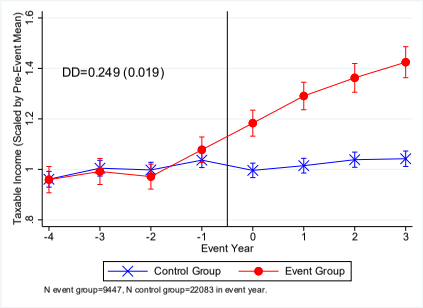


B: Corporations

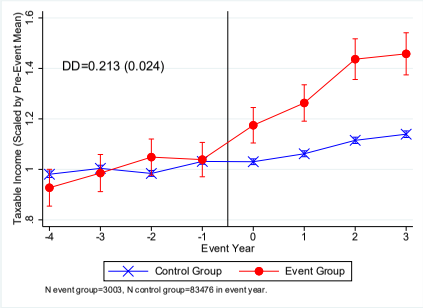
B1: Report by Supplier



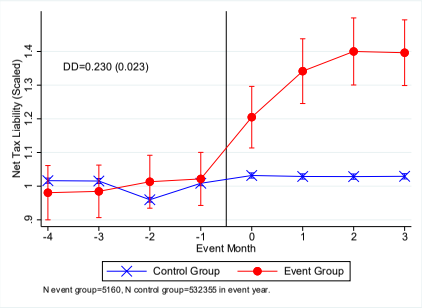
B2: Report by Client



B3: Report by State



B4: Report by Credit/Debit Card

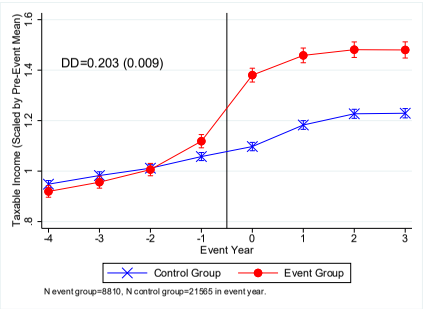


Notes: This figure displays event studies of firms' behavior after being reported for the first time by different reporting agents. Each panel displays the reported taxable income (tax liability, in panels A4 and B4) around the year (month) of the event, scaled by the pre-event mean, for an event group (orange doted line) and an event control group (blue crossed line). The black solid line marks event time 0, when firms in the event group are reported to the tax authority for the first time by the reporting agent indicated in the panel title. The data is top-coded at the 98th percentile by event-group-month. The control group is re-weighted by its propensity score of experiencing the event (cf. section B.2 and footnote 16 for details). Each group consists of a balanced panel of firms that can be observed for the period displayed (four years/months before and three years/months after the event). Panels A1-A3 and B1-B3 consider events happening in 2010-2013. Panels A4 and B4 consider events happening in 02/2009-08/2014. The text displays the difference-in-differences coefficient from estimating equation 2. Standard errors are clustered at the firm-level. Figures B.4, B.5, B.6 and B.7, and Tables B.5 and B.4 show robustness of the results to alternative specifications.

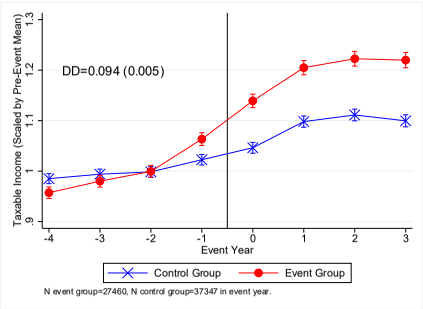
Figure B.4: Event Study Robustness I: No Propensity Score Reweighting

A: Self-Employed

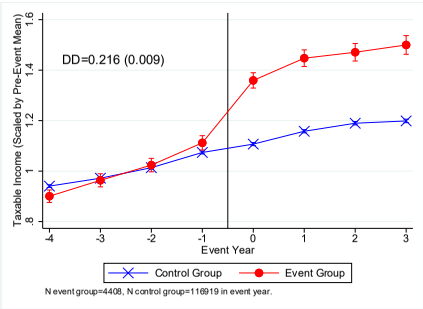
A1: Report by Supplier



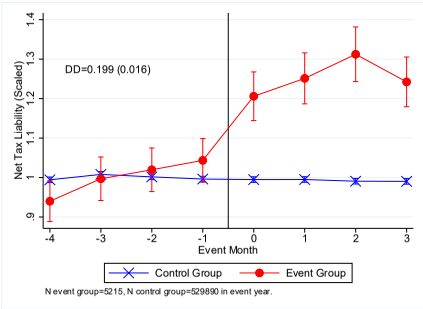
A2: Report by Client



A3: Report by State

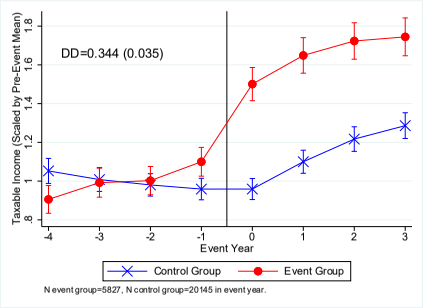


A4: Report by Credit/Debit Card

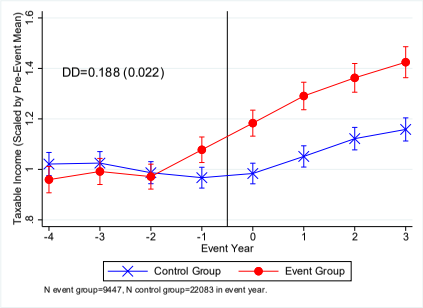


B: Corporations

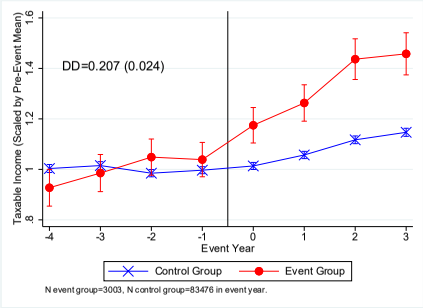
B1: Report by Supplier



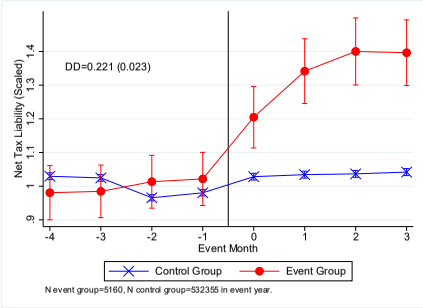
B2: Report by Client



B3: Report by State



B4: Report by Credit/Debit Card

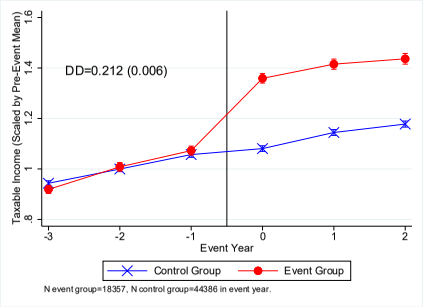


Notes: This figure is identical to Figure B.3, except that the control group is not reweighted by its propensity to experience the event.

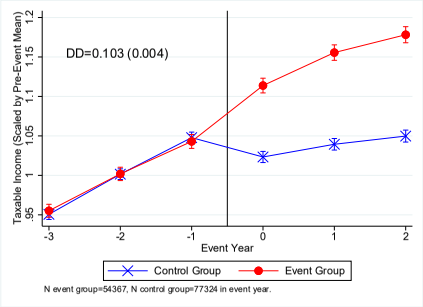
Figure B.5: Event Study Robustness II: Shorter Pre- and Post-Event Periods, Hence More Events Considered

A: Self-Employed

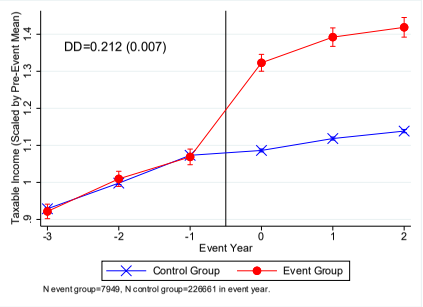
A1: Report by Supplier



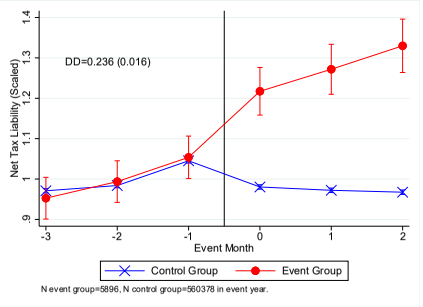
A2: Report by Client



A3: Report by State

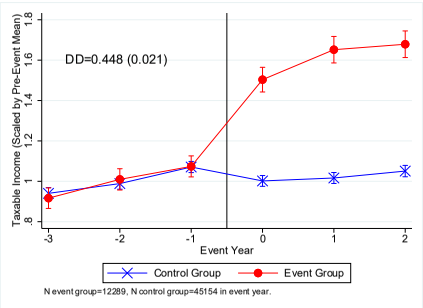


A4: Report by Credit/Debit Card

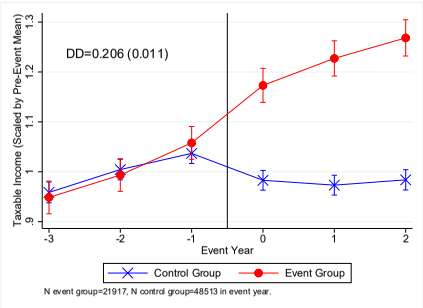


B: Corporations

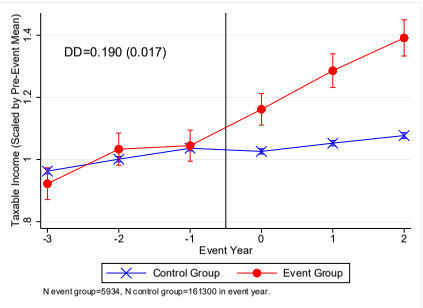
B1: Report by Supplier



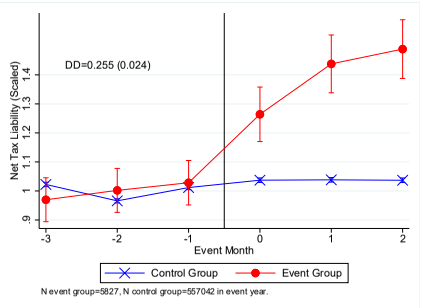
B2: Report by Client



B3: Report by State



B4: Report by Credit/Debit Card

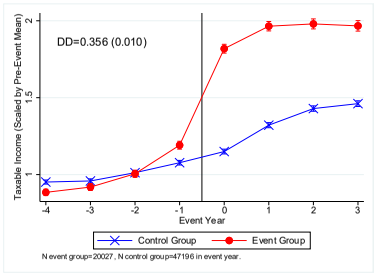


Notes: This figure is identical to Figure B.3, except that it focuses on a shorter period around the event (three years/months before the event and two years/months after), and thus considers a larger number of events and firms.

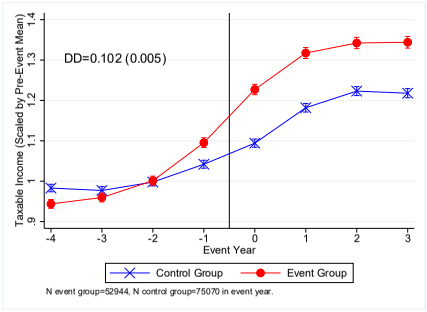
Figure B.6: Event Study Robustness III: Semi-Balanced Panel

A: Self-Employed

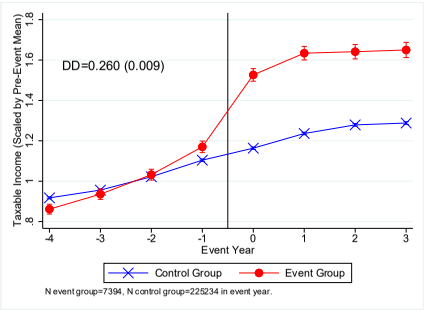
A1: Report by Supplier



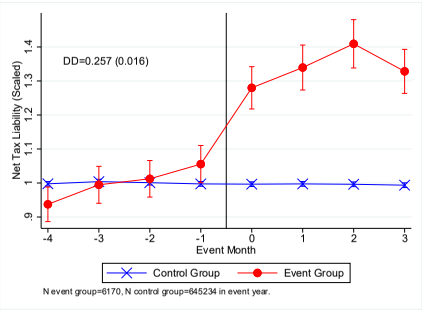
A2: Report by Client



A3: Report by State

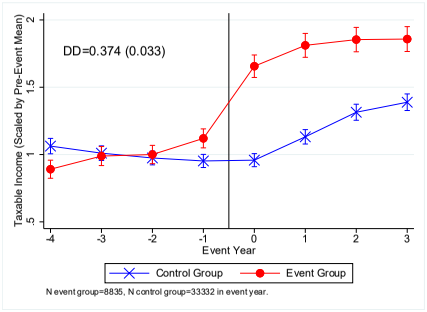


A4: Report by Credit/Debit Card

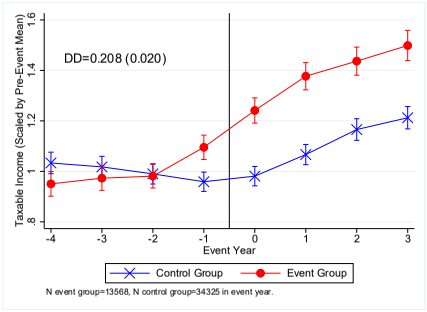


B: Corporations

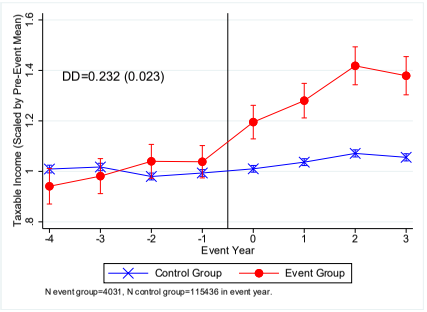
B1: Report by Supplier



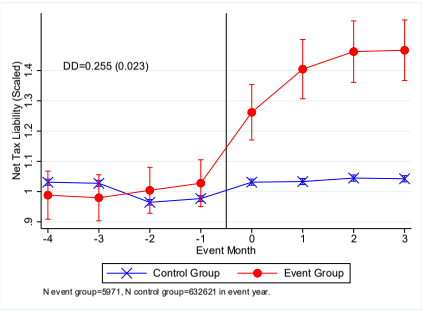
B2: Report by Client



B3: Report by State



B4: Report by Credit/Debit Card

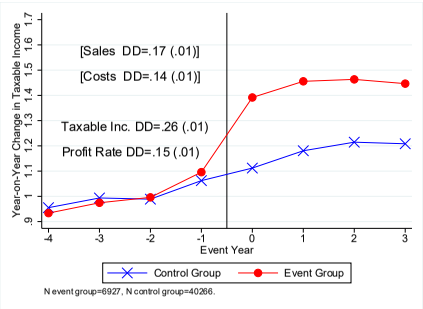


Notes: This figure is identical to Figure B.3, except that it considers a semi-balanced panel of firms (as in sub-table 3 of Tables B.4 and B.4). To construct this semi-balanced panel, we sort the data by firm-year, replace by zero any missing outcome variables that are followed and preceeded by non-missings for the same firm, drop the remaining missing observations, and retain those firms that appear in the data in all four periods before and three periods after the event. In these figures, the control group is not reweighted by its propensity to experience the event.

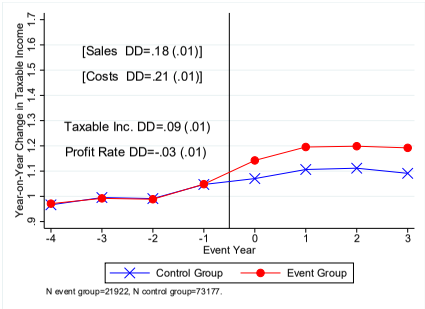
Figure B.7: Event Study Robustness IV: OLS Estimation, Multiple Outcome Variables

A: Self-Employed

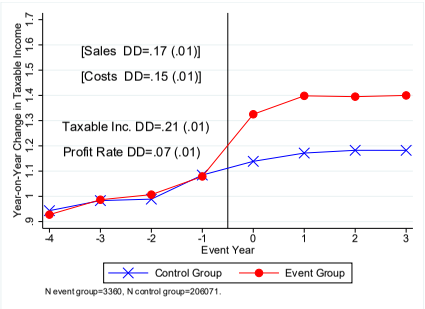
A1: Report by Supplier



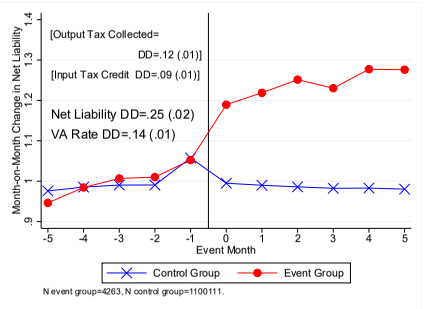
A2: Report by Client



A3: Report by State

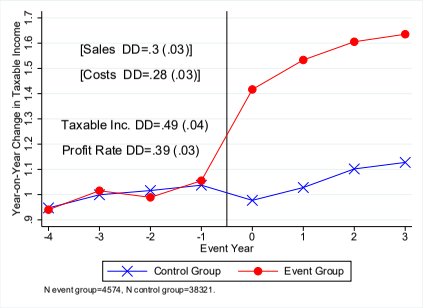


A4: Report by Credit/Debit Card

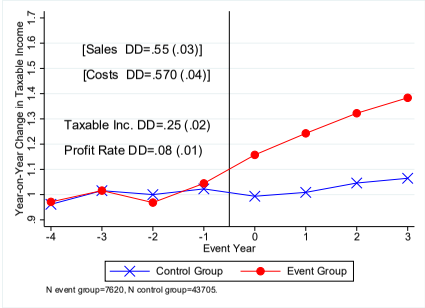


B: Corporations

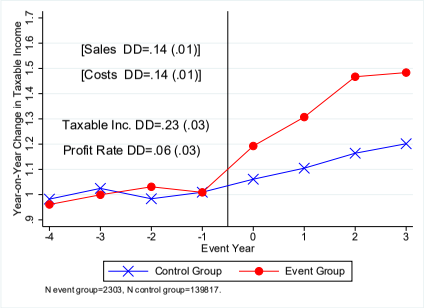
B1: Report by Supplier



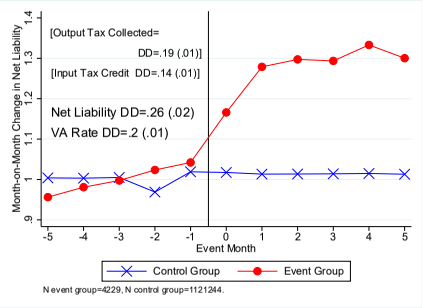
B2: Report by Client



B3: Report by State



B4: Report by Credit/Debit Card



Notes: This figure is similar to B.3, except that the data is top-coded at the 95th percentile for each event-group-year/month, and the estimation is OLS. The figures display DiD estimates of equation 2 for different outcome variables, as noted in the figures. VA stands for value added.

C The Anatomy of Compliance

Having shown that third-party reporting substantially increases reported tax liability, we now study whether it moves taxpayers close to full compliance. Our conceptual framework suggests that this should be the case if taxpayers correctly perceive the enforcement parameters R_T and $p()$, but not if taxpayers misperceive those parameters. Following Fisman and Wei (2004), we examine compliance by comparing two data reports on the same tax base. We consider successively the extensive, intensive and remittance margin of compliance.

C.1 The Extensive Margin

To examine compliance on the extensive margin, we construct the set of tax-liable firms based on all tax declarations, third-party reports, and registration reports, and compare it to self-reported income tax and sales tax declarations. The algorithm to identify tax-liable firms is described in detail in the appendix.[22] Table C.1 reports the share of non-filers for different taxes and subsamples. The overall share of non-filers for the income tax is substantial in all years and rose from 38 percent of tax-liable firms in 2010 to 55 percent in 2013 (panel A, column 1). Non-filing for the sales tax seems less prevalent at about 20 percent of tax-liable firms, which is consistent with the self-enforcing nature of Costa Rica's VAT-like sales tax. However, identifying non-filers is more difficult for sales tax than for income tax, as third-party reports provide no information on which firms are liable for sales tax. The majority of the sales tax non-filers that we identify are registered firms that file only intermittently. By contrast, the majority of income tax non-filers are identified through third-party reports (column 2). This suggests that although third-party information helps identify taxable activities, it does not necessarily induce reported firms to comply with their tax-filing obligations.

An analysis of filing behavior across firm types shows that non-filing rates are generally lower for registered firms (panel B, column 1). Among registered firms, non-filing rates are lower for corporations than for self-employed individuals (columns 2 and 4).[23] The coverage of third-party reporting is also correlated with tax filing among registered firms, as theory would predict (columns 3 and 5), and this correlation is stronger for corporations. Non-filing rates

[22] Note that our algorithm is more conservative than the tax authority's own algorithm, which considers firms to be tax-liable if they have filed in the past three years and have not deregistered since. Appendix Table C.2 reports estimates using a more lenient algorithm, which goes back three years for income tax and 12 months for sales tax. The estimates are marginally higher for the income tax and about one-third higher for the sales tax. The three-year window reflects the tax authority's practice of deregistering a firm *de oficio* if it has not filed a tax declaration for three years.

[23] Note that column 1 in panel B is not the average of columns 2 and 4, as column 1 also includes firms for which the firm-type indicator, which identifies self-employed individuals and corporations, is missing.

are significantly lower among firms reported by state institutions or credit-card companies than among firms reported only by their suppliers or clients (panel C). This suggests that reporting mechanisms have a stronger compliance impact when accompanied by withholding.

To proxy the loss of tax revenue due to non-filing, we estimate that the share of undeclared sales represents 16-23 percent of declared sales and that the estimated share of unreported income tax liabilities represents 7-10 percent of reported liabilities (panel A, columns 4-5). The estimates rely on non-filers' third-party-reported sales or their most recent available tax return. It is assumed that the distribution of profit rates by firm size is similar for non-filers and filers and that the tax schedule is applied according to Costa Rican law (see the notes to Table C.1 for details).

As our data does not capture firms that are fully informal and do not transact with any third-party reporting agents, our estimates provide a weak lower bound for extensive-margin compliance gaps. However, they should still capture the policy-relevant subsample of extensive-margin non-compliers. Indeed, while several studies find that formalizing fully informal firms is difficult and costly (de Mel et al. 2013, Bruhn and McKenzie 2014), Brockmeyer et al. (2019) show that low-cost deterrence messages can significantly increase filing rates among firms known to the tax authority, especially those covered by third-party reporting.

Table C.1: Non-Filing

Panel A: Non-filing among all tax-liable firms

	(1)	(2)	(3)	(4)	(5)
	% Non-filers	Of which registered non-filers	Non-filers' TPR sales as % of declared TPR sales	Undeclared sales as % of declared sales	Unreported liability as % of declared liability
Income Tax 2010	0.384	0.185	0.155	0.211	0.071
Income Tax 2011	0.485	0.125	0.195	0.230	0.069
Income Tax 2012	0.528	0.116	0.115	0.168	0.071
Income Tax 2013	0.556	0.121	0.114	0.181	0.099
Sales Tax 2011	0.196	0.894	.	.	.
Sales Tax 2012	0.185	0.884	.	.	.
Sales Tax 2013	0.198	0.891	.	.	.

Panel B: Non-filing among registered firms

	All	Self-Employed	Self-Employed with TPR	Corporations	Corporations with TPR
Income Tax 2010	0.104	0.095	0.082	0.081	0.045
Income Tax 2011	0.105	0.090	0.078	0.061	0.033
Income Tax 2012	0.115	0.080	0.070	0.065	0.028
Income Tax 2013	0.131	0.128	0.094	0.137	0.043
Sales Tax 2011	0.180	0.263	0.122	0.094	0.060
Sales Tax 2012	0.168	0.247	0.103	0.085	0.045
Sales Tax 2013	0.181	0.269	0.079	0.085	0.034

Panel C: Non-filing among firms covered by information reporting

	All	Reported by firms	Reported by state	Reported by card companies	
Income Tax 2010	0.574	0.579	0.235	0.293	.
Income Tax 2011	0.653	0.659	0.202	0.297	.
Income Tax 2012	0.673	0.679	0.186	0.308	.
Income Tax 2013	0.677	0.682	0.177	0.319	.

Notes: These panels show the share of non-filers (tax liable firms that do not file) for the income tax (rows 1-4) and the sales tax (rows 5-7). The algorithm used to construct the share of non-filers is explained in the appendix. Panel A shows the share of non-filers among all tax liable firms (column 1), the share of non-filers that are registered (2), non-filers' third-party reported sales as share of filers' reported sales (3), non-filers' estimated sales as share of declared sales (4), and non-filers' estimated tax liability as share of declared liability (5). TPR stands for third-party reports, and TPR sales is the sum of all third-party reports except cost reports. A non-filing firm's estimated sales in period t is max(third-party reported sales in t ; self-reported sales in the most recent prior reporting period). A non-filer's tax liability is estimated using its estimated sales, applying the average profit rate of filers in the corresponding decile of the sales distribution of filers, and then applying the tax schedule. Panel B reports the share of non-filers among all registered firms (1), and among subsamples of registered firms as indicated by the column headings (2-5). Panel C reports the share of non-filers among all firms covered by information reporting (1), and among subsamples of firms reported by different informing agents, as indicated by the column headings (2-4).

Table C.2: Robustness of Non-Filing Estimates

Panel A: Non-filing among all tax-liable firms

	(1) % Non-filers	(2) Of which registered non-filers	(3) Non-filers' TPI sales as % of declared TPI sales	(4) Undeclared sales as % of declared sales	(5) Unreported liability as % of declared liability
Income Tax 2010	0.420	0.335	0.154	0.225	0.081
Income Tax 2011	0.509	0.239	0.195	0.246	0.077
Income Tax 2012	0.547	0.218	0.114	0.189	0.076
Income Tax 2013	0.586	0.266	0.112	0.203	0.104
Sales Tax 2011	0.335	0.960	.	.	.
Sales Tax 2012	0.325	0.955	.	.	.
Sales Tax 2013	0.372	0.962	.	.	.

Panel B: Non-filing among registered firms

	All	Self-Employed	Self-Employed with TPI	Corporations	Corporations with TPI
Income Tax 2010	0.195	0.158	0.131	0.155	0.073
Income Tax 2011	0.198	0.161	0.134	0.143	0.071
Income Tax 2012	0.209	0.153	0.126	0.128	0.051
Income Tax 2013	0.273	0.215	0.158	0.216	0.067
Sales Tax 2011	0.326	0.439	0.155	0.192	0.073
Sales Tax 2012	0.315	0.423	0.126	0.185	0.054
Sales Tax 2013	0.363	0.478	0.101	0.185	0.040

Panel C: Non-filing among firms covered by information reporting

	All	Reported by firms	Reported by state	Reported by card companies	
Income Tax 2010	0.573	0.578	0.235	0.293	.
Income Tax 2011	0.652	0.658	0.202	0.297	.
Income Tax 2012	0.672	0.678	0.186	0.308	.
Income Tax 2013	0.676	0.681	0.177	0.320	.

Notes: The construction of this table is identical to Table C.1, except that it uses a less conservative algorithm to calculate the share of non-filers, as explained in Appendix D.

C.2 The Intensive Margin

To examine compliance on the intensive margin, we compare taxpayers' self-reports and third-party reports, for sales and costs respectively. We construct a taxpayer's third-party reported sales as the sum of sales reported by other firms (the taxpayer's clients), state institutions, and credit-card companies, as well as sales recorded in export data from the customs service. A taxpayer's third-party-reported costs (purchases) are the sum of sales reported by the taxpayer's suppliers as well as purchases recorded in import data. Firms reporting an amount at least 0.25 percent smaller that the relevant comparison amount are defined as "under-reporters," while firms reporting an amount at least 0.25 percent larger than the relevant comparison amount are defined as "over-reporters."

Table C.3 shows estimated under-reporting for tax year 2012, distinguishing sales reports and cost reports and self-employed individuals from corporations. Panel A focuses on income tax under-reporting, panel B on estimating under-reported income tax liability, and panel C on the internal consistency between the income tax and sales tax.[24]

While 16 percent of self-employed individuals and 13 percent of corporations under-report sales compared to third-party reports, the share of firms under-reporting their costs is even higher, at 51 percent for self-employed individuals and 35 percent for corporations (row 1). This indicates that firms not only under-report sales, they also under-report the scale, which is consistent with the findings of Carrillo et al. (2017). The presence of an exempt tax bracket in the self-employed tax schedule explains the larger share of cost under-reporters among self-employed individuals. While under-reporters leave 41-46 percent of their third-party-reported sales and 36-40 percent of their third-party-reported costs unreported (row 5), these amounts represent about 20 percent of total third-party reports (row 6). The share of unreported sales in total third-party-reported sales is slightly larger than the share of sales under-reporters, at least among corporations, suggesting that under-reporters are not disproportionately likely to be small firms (rows 1 vs 6, column 2). The share of under-reported costs in total third-party-reported costs is significantly smaller than the share of cost under-reporters (rows 1 vs 6, columns 3 and 4). This suggests that although cost under-utilization is widespread, it is modest in scale.

With a few assumptions, we estimate that if all third-party-reported sales were declared, reported tax liability would increase by 19 percent for corporations and by 48 percent for self-

[24]Estimating under-reporting for sales tax is more challenging, due to its narrow base and the fact that third-party reports do not distinguish between sales that are liable for sales tax and those that are not.

employed individuals (row 9).[25] The especially large increase among self-employed individuals is driven by their high initial reported profit rates, given the exempt tax bracket. However, self-employed individuals report tax liabilities that are, on average, much smaller than those reported by corporations. If all third-party reported sales were declared and taxed, overall income tax revenue from firms would increase by about 22 percent.

Combining estimates from the extensive and intensive margin indicates that fully enforcing compliance with third-party reports could boost income tax revenue by up to 30 percent. However, enforcement is costly, and the limited impact of desk audits (phone calls to misreporting taxpayers requesting that they file an amended tax declaration) suggests that it is unlikely to substantially increase compliance rates. Figure C.1 displays the results of desk audits for the income tax (panel A) and the sales tax (panel B). Comparing a firm's initial tax return to the post-audit amended return, the figure plots the change in reported costs against the change in reported revenue (the change in reported input tax credit against the change in sales tax collected in panel B). The figures focus on the small share of desk-audited firms that actually amend their declarations in response to the desk audit: 19 percent of firms for income tax and 16 percent for sales tax. Firms that amend their declarations clearly offset increases in reported revenue by increasing reported costs. Such changes offset each other by almost 100 percent for income tax and by about two-thirds for sales tax. On average, firms that file an amended declaration more than double their reported tax liability, as their initial reported liability is extremely low, but the number of such firms and their aggregate liability are so small that amended declarations increase total revenue by less than 0.5 percent.[26]

[25] We assume that under-reporters declare all third-party-reported sales, apply the initially reported profit rate to their initially unreported sales, and then apply the tax schedule. This means we allow under-reporters to offset additional reported sales with additional reported costs in proportion to their initial declared profit rate. This assumption is supported by evidence from Carrillo et al. (2017) and Slemrod et al. (2017), and it is consistent with firms' response to desk audits discussed below.

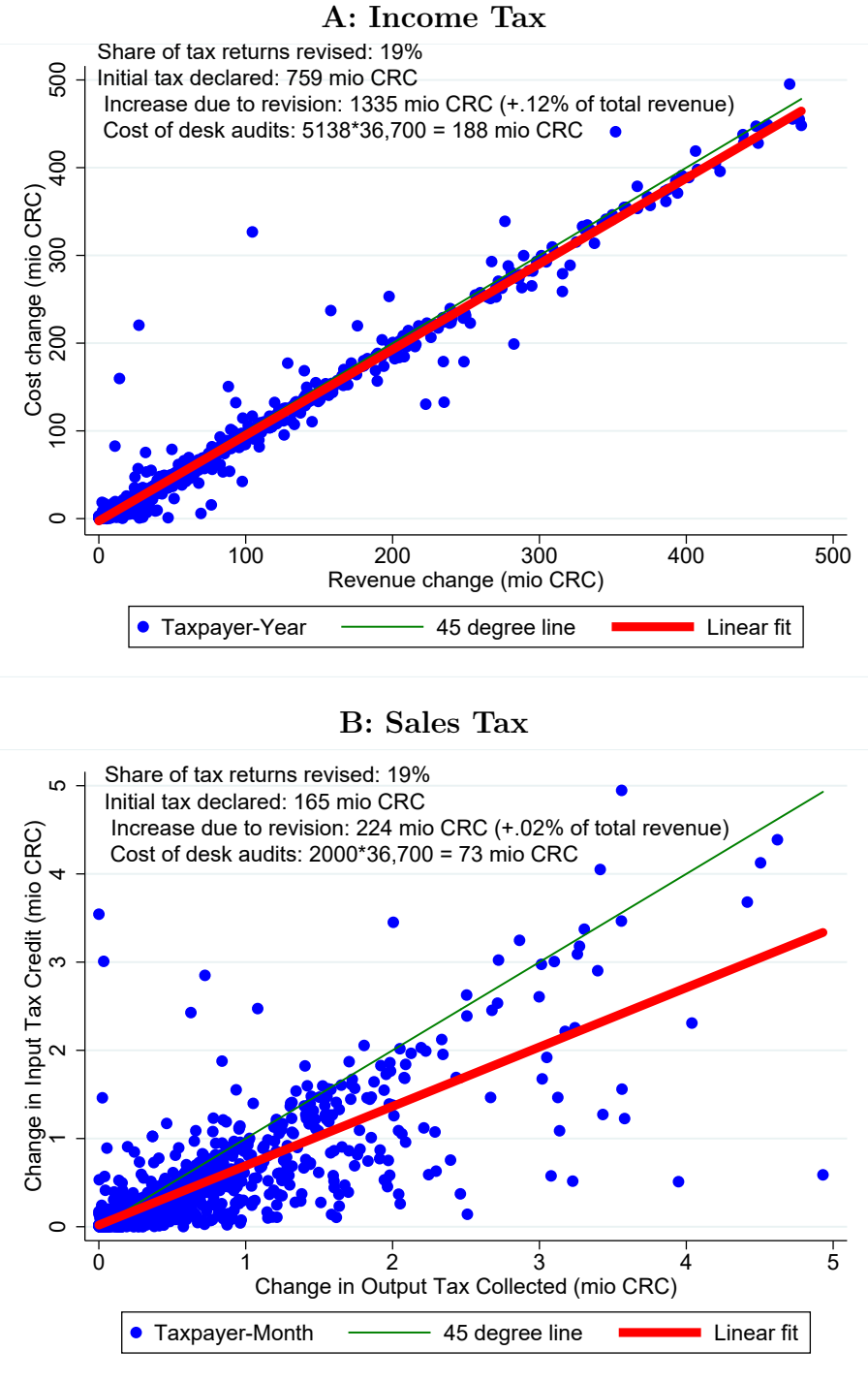
[26] Whether it is optimal for the tax authority to invest in desk audits rather than full audits or follow-up communications with non-filers or late payers depends on the relative revenue elasticities of these different enforcement methods. See, e.g., Keen and Slemrod (2017).

Table C.3: Misreporting

	(1)	(2)	(3)	(4)
	Sales Reports		Cost Reports	
	Self-Employed	Corporations	Self-Employed	Corporations
Panel A: Underreporting for Income Tax				
1) % Underreporters	16.1	13.2	51.4	35.6
2) Unreported Amount (bio CRC)	283	4540.9	448.6	3572.7
3) Underreporters' TPR	680.2	9679.7	1244.4	8865.6
4) Total TPR	2088.8	19489.9	1902.7	16140.3
5) Unreported Amount(% UR TPR)	41.6	46.9	36.1	40.3
6) Unreported Amount(% TPR)	13.6	23.3	23.6	22.1
Panel B: Underreported Liability				
7) Unreported Tax	17.2	61.4		
8) Reported Tax	35.7	318.8		
9) Unreported Tax (% Reported Tax)	48.4	19.3		
Panel C: Internal Consistency, Income Tax vs Sales Tax				
10) % Underreporters IT vs ST	7.8	8.4	12.5	6
11) % Overreporters IT vs ST	56.9	60.3	84.8	93.5

Notes: This table displays estimates of compliance gaps between third-party reports and self-reports. Third-party reported sales for the income tax is the sum of sales reported by clients, state institutions and credit-card companies, and exports. Third-party reported costs for the income tax is the sum of costs reported by suppliers, and imports. Third-party reported sales for the sales tax is the sum of sales reported by credit-card card companies. All figures in this table are either in percent (as indicated), or in billions of constant 2015 CRC. Under-reporters (over-reporters) are firms reporting an amount at least 0.25 percent smaller (larger) than the relevant comparison amount. Rows 1-6 examine under-reporting of third-party reported sales/costs. They show the share of under-reporters among firms subject to third-party reporting for the income tax (1), the amount unreported (as compared to third-party reports) (2), the total third-party reports for under-reporters (3), the total third-party reports for the full sample (4), and the unreported amount as a share of the underreporters third-party reports (5), and as a share of total third-party reports (6). Rows 7-9 convert unreported sales into tax liabilities. They show an estimate of the unreported tax liability (7), the reported tax liability (8), and the unreported tax as a share of the reported tax (9). The estimation of the unreported (gross) tax liability assumes that the profit rate on unreported sales is the same as the profit rate on reported sales, and applies the tax schedule as displayed in Table B.1. Rows 10 and 11 analyze internal consistency in filing, comparing income tax reports to sales tax reports. All calculations are based on 2012 data, and we drop 2,200 firms that file following a non-standard fiscal year. Results are similar in the full sample and in other years.

Figure C.1: Impact of Desk Audits on Misreporting



Notes: This figure shows the revenue and cost adjustments made by firms after they are informed that a desk audit uncovered a discrepancy between self-reported and third-party reported sales. The desk audits are for income tax returns for 2013 and 2014, and for sales tax returns for 2013. The figures focus on firms submitting a revised tax return, and display the change in revenue and costs (output tax collected and input tax credit deducted in panel B), comparing the initial return with the revised return. The text displays the share of audited firms that submit a revised return, the initial tax declared by revisers, the increase in declared tax due to the revision, and the cost of the desk audit intervention. The cost is calculated by multiplying the number of cases by the daily wage of a tax officer, as tax officers are asked to handle one case per day.

C.3 The Tax Remittance Margin

Finally, to examine taxpayers' compliance with the obligation to remit their net tax liability, we match income and sales tax returns with remittance records (payment receipts). Importantly, Costa Rican remittance records display the remittance date, the tax period, and the taxpayer to which each remittance corresponds, allowing us to exactly match remittances with liabilities. To our knowledge, this is the first attempt to estimate remittance compliance for the income and sales tax and to test the previously implicit assumption that declared tax liabilities automatically translate into tax remittances.[27] The relevant liability is the taxpayer's final tax liability and is to be remitted per the final (amended) tax declaration, after deductions, advance remittances, and withheld taxes have been subtracted. We compare this liability to each taxpayer's final tax remittance, excluding remittances made by withholding agents and advance remittances made by the taxpayer.[28] We then take the share of remittance over liability for each taxpayer, and average this share across all taxpayers in each fiscal period.

The results are displayed in Figure C.2, where panel A corresponds to the income tax and panel B correspond to the sales tax, and the thick blue and thin red lines correspond to corporations and self-employed individuals, respectively. In both panels, the average remittance share is below 100 percent in all fiscal periods and decreases in more recent periods, dropping to 85 percent for the sales tax and 70 percent for the income tax in the most recent period considered (solid lines). This patterns is clear despite the fact that we consider remittances made until April 2015, the remittance deadline for fiscal year 2014. There are two potential explanations for this downward sloping profile of the average remittance rate: delayed remittance and diminished compliance. If taxpayers remit tax only after a substantial delay, then more recent periods will mechanically display lower remittance rates than earlier periods, for which a longer data series is available. It is also possible that a rising number of firms is failing to remit tax entirely, and these two explanations are not mutually exclusive.

To distinguish these two explanations, we add two more remittance profiles, considering only remittances made until April 2013 and April 2011, respectively (dashed and dotted lines). These remittance profiles are similarly downward sloping and shifted to the left, suggesting that remittance delays do indeed play a role in the observed decline in average remittance rate. For

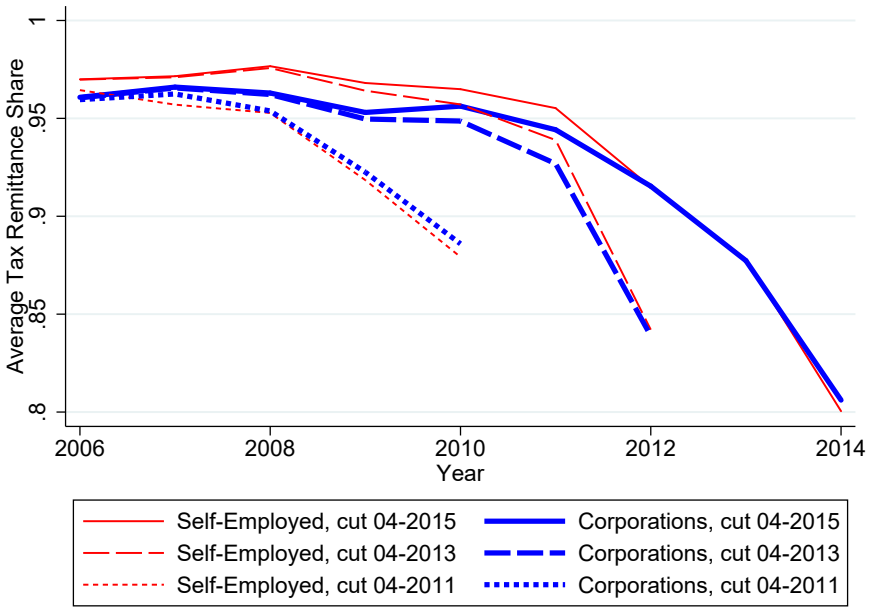
[27] The estimates of property tax compliance in Peru by Del Carpio (2014) are conceptually different from our estimates, as property taxes are assessed by the government and thus have no misreporting margin.

[28] Note that we use the net liability derived from the firm's tax return, and take into account only the amount of advance tax remittances and withheld taxes that the taxpayer chose to reclaim on the tax declaration. Including remittances that are enforced retroactively by the tax authority through administrative or judicial procedures does not significantly affect the results.

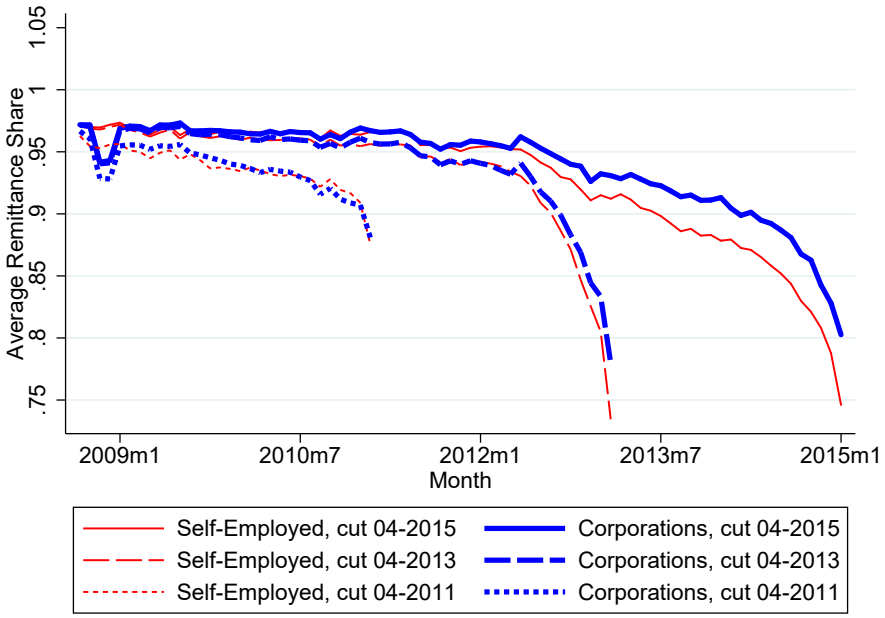
instance, while the income tax remittance share for 2010 is about 88 percent when measured in April 2011, it is above 95 percent when measured in April 2015–indicating that a small share of taxpayers remit their tax after a substantial delay. This finding is consistent with anecdotal evidence that cash-constrained firms remit tax when they have adequate liquidity rather than when the remittance is due, as fines and interest fees for late remittance are small. Meanwhile, remittance compliance is relatively high, especially in the aggregate. As firms that do not remit tax or remit after a significant delay are disproportionately small, the aggregate remittance rate (i.e., the sum of remittances divided by the sum of final liabilities) approaches 100 percent shortly after the remittance deadline and remains stable over time.

Figure C.2: Remittance of Tax Liabilities

A: Income Tax, Average Share of Net Liability Remitted



B: Sales Tax, Average Share of Net Liability Remitted



Notes: This figure shows the average tax remittance share, defined as the remittance made by the taxpayer for a specific tax period, divided by the tax liability to be remitted for that period. The average is an unweighted average across all taxpayers with a positive final liability. The tax liability is net of any deduction made for tax withheld, and the remittance data does not include tax withheld. The income tax data includes all declarations filed and remittances made by June 2015. The sales tax data includes all declarations filed and remittances made by October 2015. The thin red lines correspond to the self-employed and the thick blue lines corresponds to corporations. We show the series for three different cuts in the remittance data, taking into account all remittances made before the cut date.

D Algorithm to Estimate Non-Filing

D.1 Income Tax

This section describes the procedure by which the pool of income tax-liable taxpayers is constructed for each year between 2010 and 2013. This pool forms the sample for Tables C.1 and C.2. The following algorithm is used to identify tax-liable firms:

- **Income tax filing**: Firms that declare income tax in current fiscal year t and are not found to have deregistered or switched to a simplified (non-tax-liable) regime by year t are included in the pool of income tax-liable taxpayers. Additionally, firms that filed in the previous year $t-1$ and did not deregister or switch regimes in the interim are included to capture taxpayers who were previously identified as income tax-liable and then do not file in subsequent years. This condition is applied to generate the sample used in Table C.1; for the sample used in Table C.2, the rule is extended to include firms that declared income tax in any year between $t-1$ and $t-3$ for years 2011 and later and between $t-1$ and $t-4$ for years prior to 2011, which are the historical rules officially applied by the tax authority for determining income tax liability, though in practice this condition may not have been systematically applied.

- **Registration**: Firms that are found to have registered with the tax authority in any of the previous three years prior to the current fiscal year t but did not file income tax returns in the current fiscal year are included. This step identifies firms that are officially registered as tax-liable but which did not file for the years during which these firms were registered, which step 1 does not capture as it relies only on the records of filings.

- **Subsequent deregistration**: In addition to identifying firms that are registered but do not file for income tax in prior years in step 2, firms that deregister in years following the current fiscal year t but are not found to have registered in year t or the previous three years are classified as having been income tax-liable despite not appearing on the official registration roster.

- **Declaring sales tax in current fiscal year**: Firms that declare sales tax in any of the months of the current fiscal year t - which spans from October of year $t-1$ to September of year t - are included in the pool of income tax-liable taxpayers. This rule corresponds to the tax authority's official regulation that firms liable for sales tax are also deemed liable for remitting income tax (though the reverse is not true).

- **Informative declarations**: Records of transactions between firms or between firm and the government provide information on the sales and costs of individual firms that can be used to identify income tax-liable taxpayers. Firms that report or are reported as having tax-liable sales in the current fiscal through the third-party reporting mechanisms described above are included in the pool of income tax-liable firms.

D.2 Sales Tax

This section describes the procedure by which the pool of sales tax-liable taxpayers are constructed for each month of fiscal years 2011 to 2013. This pool forms the sample for Tables C.1 and C.2, which reports the average of the monthly values for each measure in corresponding fiscal year. The following algorithm, which is similar to the method used to identify income tax-liable firms, is used to identify sales tax-liable firms:

- **Income tax filing**: Firms that declare sales tax in month m of the current fiscal year t and are not found to have deregistered or switched to a simplified (non-tax-liable) regime by month m in year t are included in the pool of sales tax-liable taxpayers. Additionally, firms that filed in the previous four months - $m-4$ to $m-1$ - and did not deregister or switch regimes in the interim are included to capture taxpayers who were previously identified as sales tax-liable and then do not file in subsequent months. This could include firms that either declare sales tax in previous months within the same fiscal year or in months falling within the previous fiscal year $t-1$, depending on the month. This condition is applied to generate the sample used in Table C.1; for the sample used in Table C.2, the rule is extended to include firms that declared sales tax in months between $m-12$ and $m-1$.

- **Registration**: Firms that are found to have registered with the tax authority in any of the previous thirty six months (three years) prior to the current month m of fiscal year t but did not declare sales tax in month m are included. This step identifies firms that are officially registered as tax-liable but which did not file for the years during which these firms were registered, which step 1 does not capture as it relies only on the records of filings.

- **Subsequent deregistration**: In addition to identifying firms that are registered but do not file for income tax in prior years in step 2, firms that deregister in months following the current month m but are not found to have registered within the previous thirty

six months are classified as having been income tax-liable despite not appearing on the official registration roster.

- **Informative declarations**: Firms that are reported as having been subject to withholding by credit-card companies are considered tax-liable for the sales tax.

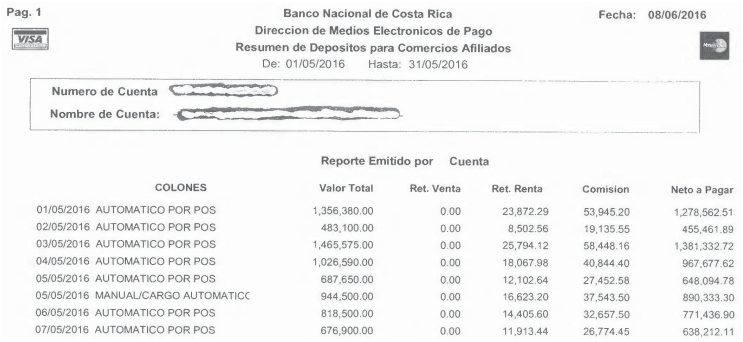
E Data Appendix

Table E.1: Informative Declarations to the Tax Authority

(1) Form	(2) Purpose	(3) Record Type	(4) Coverage of Corporations	(5) Coverage of Self-Employed	(6) % Matched With Income Tax Records
D151	Reporting of firm-firm transactions	Sales	46.4	38.9	36.8
	N=17,251,681 (53.4% sales)	Purchases	49.2	53.9	65.1
D150	Reporting of and withholding on	Sales to state institutions	8.4	5.0	78.2
	sales to specific clients	Sales to specific firms	0.22	0.08	40.2
	for purpose of income tax compliance				
	N=864,695 (98.7% state purchase)				
D153	Reporting of and withholding on	Sales	11.5 [29.4]	5.8 [20.7]	67.8 [48.7]
	credit/debit-card sales for sales tax				
	for purpose of sales tax compliance				
	N=3,928,545				

Notes: This table provides information about the nature and coverage of third-party informative declarations used by the tax authority in Costa Rica, for 2006-2015. Columns 4 and 5 display the share of income tax filers covered by the different third-party declarations, distinguishing corporations and the self-employed. Column 6 displays the share of informative declarations which are matched with an income tax declaration. In the last row, the shares in brackets refer to the match rate with the monthly sales tax declarations for 2008-2014. The shares are calculated on the pooled data for all years/months. All declarations identify the reporter and taxpayer by their administration-internal anonymous tax ID, and provide information on the transaction amount, and (where applicable) the amount of the tax withheld. Amounts are accrued. Since January 2012, all declarations must be prepared using the DECLAR@7 software. All declarations are annual, except D153, which is monthly. D151 requires reporting of transactions >2.5 million CRC annually with a transaction partner, and transactions of >50,000 CRC annually for rent, commissions, professional services or interests. For D150, the withholding rate is 2 percent and 3 percent respectively for state and private purchases. For D153, the withholding rate is firm specific, following the schedule in Table 1. For more information on the filing of informative declarations, see http://www.hacienda.go.cr/contenido/12997-declaraciones-informativas.
This table is mentioned in Section 3.2.

Figure E.1: Card-Machine Statement



Pag. 1

Banco Nacional de Costa Rica
Direccion de Medios Electronicos de Pago
Resumen de Depositos para Comercios Afiliados
De: 01/05/2016 Hasta: 31/05/2016

Fecha: 08/06/2016

Numero de Cuenta [redacted]
Nombre de Cuenta: [redacted]

Reporte Emitido por Cuenta

COLONES	Valor Total	Ret. Venta	Ret. Renta	Comision	Neto a Pagar
01/05/2016 AUTOMATICO POR POS	1,356,380.00	0.00	23,872.29	53,945.20	1,278,562.51
02/05/2016 AUTOMATICO POR POS	483,100.00	0.00	8,502.56	19,135.55	455,461.89
03/05/2016 AUTOMATICO POR POS	1,465,575.00	0.00	25,794.12	58,448.16	1,381,332.72
04/05/2016 AUTOMATICO POR POS	1,026,590.00	0.00	18,067.98	40,844.40	967,677.62
05/05/2016 AUTOMATICO POR POS	687,650.00	0.00	12,102.64	27,452.58	648,094.78
05/05/2016 MANUAL/CARGO AUTOMATICC	944,500.00	0.00	16,623.20	37,543.50	890,333.30
06/05/2016 AUTOMATICO POR POS	818,500.00	0.00	14,405.60	32,657.50	771,436.90
07/05/2016 AUTOMATICO POR POS	676,900.00	0.00	11,913.44	26,774.45	638,212.11

Notes: This figure shows an example of the credit-card marchine statement, that card processing companies provide to their affiliated businesses (clients) on a daily or monthly basis. The statement lists transaction amounts, withholding for the income tax and the sales tax remitted to the tax authority and commissions to the card processing company.

F The Impact of Withholding – Robustness

Table F.1: The Impact of Withholding: Elasticity Estimates

	Pre-Reform Mean Among Treated			Treatment Effect			Implied Elasticity of Sales Tax Remittance		
	(1) Trimmed 99.9th pctile	(2) Trimmed 99th pctile	(3) Trimmed 95th pctile	(4) Trimmed 99.9th pctile	(5) Trimmed 99th pctile	(6) Trimmed 95th pctile	(7) Trimmed 99.9th pctile	(8) Trimmed 99th pctile	(9) Trimmed 95th pctile
Total Sales Tax Remittance	713.2	451.7	265.5	0.270***	0.316***	0.428***	.	.	.
	(47.07)	(14.16)	(5.451)	(0.0315)	(0.0321)	(0.0247)	(.)	(.)	(.)
Withholding Rate	1.486	1.485	1.476	0.869***	0.870***	0.882***	0.310***	0.363***	0.485***
	(0.00929)	(0.00940)	(0.00978)	(0.0175)	(0.0177)	(0.0185)	(0.0363)	(0.0369)	(0.0280)
Withheld Tax	140.4	87.92	58.49	0.721***	0.723***	0.828***	0.374***	0.437***	0.516***
	(8.770)	(4.452)	(1.949)	(0.0760)	(0.0672)	(0.0379)	(0.0437)	(0.0444)	(0.0298)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXmonth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	257,796	252,648	235,944	257,796	252,648	235,944	257,796	252,648	235,944

This table displays mean outcomes in the treatment group (columns 1-3), DiD estimates of the impact of the (predicted) withholding-rate increase as per equation 10 (columns 4-6), and the associated elasticities (columns 7-9). The treatment effect in columns 4-6 is a semi-elasticity/proportional effect. The elasticity estimate is derived using the DiD estimate for total sales tax remittance and for the withholding rate (or for the withheld tax). Standard errors are derived using the delta rule. The DiD estimates are based on the Poisson Pseudo Maximum Likelihood Estimator (PPML, see Appendix A). They allow for firm fixed effects, month fixed effects and firm characteristics (sector, deciles of card usage at the beginning of the period) interacted with month fixed effects. Standard errors are clustered at the firm level. The estimations are for a fully balanced panel (firms filing every month during 2010-2012). We show robustness to weaker balancing restrictions in Table 3. To reduce the effect of outliers while maintaining the internal consistency of the tax declaration, we trim rather than topcode outliers, at the 99.9th, 99th or 95th percentile in the distribution of reported sales (as indicated in the column headings). This table is discussed in Section F.

Table F.2: The Impact of Withholding: OLS Estimation

	Fully-Balanced Panel			Semesterly-Balanced Panel		
	(1) Trimmed 99.9th pctile	(2) Trimmed 99th pctile	(3) Trimmed 95th pctile	(4) Trimmed 99.9th pctile	(5) Trimmed 99th pctile	(6) Trimmed 95th pctile
Filed Sales Tax				-0.00199	-0.00183	-0.00193
				(0.00225)	(0.00229)	(0.00243)
Card-Company Reportee	0.00310	0.00300	0.00220	0.00206	0.00234	0.00184
	(0.00506)	(0.00513)	(0.00544)	(0.00451)	(0.00458)	(0.00486)
Total Sales Reported	0.0566	-0.0133	0.000767	0.0339	0.0158	0.0146
	(0.0383)	(0.0200)	(0.0154)	(0.0314)	(0.0157)	(0.0126)
Sales Tax Collected	0.0603	-0.000929	0.00134	0.0324	0.0292	0.00714
	(0.0481)	(0.0235)	(0.0190)	(0.0403)	(0.0180)	(0.0173)
Input Tax Credits	0.0278	-0.0806***	-0.0872***	-0.00387	-0.0562***	-0.0798***
	(0.0473)	(0.0230)	(0.0141)	(0.0395)	(0.0173)	(0.0129)
- Import Credits	-0.0195	-0.0922	-0.156**	-0.0820	-0.0548	-0.161**
	(0.0963)	(0.0710)	(0.0637)	(0.0852)	(0.0571)	(0.0641)
- Local Purchase Credits	0.0546	-0.0758***	-0.0756***	0.0349	-0.0568***	-0.0686***
	(0.0486)	(0.0213)	(0.0141)	(0.0384)	(0.0185)	(0.0123)
Gross Tax Liability	0.156	0.348***	0.513***	0.148	0.437***	0.491***
	(0.147)	(0.0821)	(0.0708)	(0.137)	(0.0657)	(0.0645)
Withholding Base	0.127**	-0.00981	0.0199	0.110**	0.0558*	0.0354
	(0.0642)	(0.0569)	(0.0330)	(0.0553)	(0.0304)	(0.0261)
Withheld Tax	1.776***	1.467***	1.962***	1.730***	1.843***	1.892***
	(0.405)	(0.346)	(0.151)	(0.371)	(0.156)	(0.125)
Withheld Tax Reclaims	1.215***	1.023***	1.523***	1.114***	1.325***	1.532***
	(0.386)	(0.325)	(0.194)	(0.353)	(0.185)	(0.167)
Compensation Requests	1.231***	1.201***	0.502*	1.214***	1.147***	0.629**
	(0.356)	(0.383)	(0.277)	(0.334)	(0.368)	(0.261)
Final Tax To Remit	0.0205	0.251**	0.401***	0.0631	0.292***	0.321***
	(0.165)	(0.114)	(0.125)	(0.156)	(0.104)	(0.112)
Taxpayer Sales Tax Remittance	0.0752	0.211**	0.354***	0.0883	0.270***	0.270***
	(0.160)	(0.0855)	(0.0736)	(0.151)	(0.0740)	(0.0627)
Total Sales Tax Remittance	0.410***	0.455***	0.708***	0.405***	0.588***	0.666***
	(0.137)	(0.0967)	(0.0698)	(0.129)	(0.0681)	(0.0606)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXmonth FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	257,796	252,648	235,944	332,100	325,764	303,300

Notes: This table is similar to the main DiD results Table 3, except that these estimates here are based on an OLS estimation with untransformed outcome variables. The point estimates (absolute effects) are then scaled by the pre-reform mean to obtain a relative effect, which we display in this table. The standard errors are derived using the delta rule.

Table F.3: The Impact of Withholding: IHS Estimation

	Fully-Balanced Panel			Semesterly-Balanced Panel		
	(1) Trimmed 99.9th pctile	(2) Trimmed 99th pctile	(3) Trimmed 95th pctile	(4) Trimmed 99.9th pctile	(5) Trimmed 99th pctile	(6) Trimmed 95th pctile
Total Sales Reported	0.0846**	0.0822**	0.0932**	0.0704*	0.0759*	0.0825*
	(0.0393)	(0.0400)	(0.0422)	(0.0417)	(0.0422)	(0.0444)
Sales Tax Collected	0.0468	0.0453	0.0503	0.0324	0.0365	0.0404
	(0.0326)	(0.0331)	(0.0351)	(0.0332)	(0.0336)	(0.0355)
Input Tax Credits	-0.0950***	-0.0958***	-0.0952***	-0.0824**	-0.0779**	-0.0760**
	(0.0333)	(0.0337)	(0.0355)	(0.0327)	(0.0331)	(0.0347)
- Import Credits	-0.00474	-0.0116	-0.0163	-0.0191	-0.0242	-0.0228
	(0.0226)	(0.0224)	(0.0230)	(0.0198)	(0.0196)	(0.0201)
- Local Purchase Credits	-0.116***	-0.115***	-0.111***	-0.0942***	-0.0873***	-0.0874**
	(0.0350)	(0.0353)	(0.0368)	(0.0335)	(0.0337)	(0.0350)
Gross Tax Liability	0.585***	0.591***	0.611***	0.531***	0.545***	0.546***
	(0.0422)	(0.0425)	(0.0436)	(0.0385)	(0.0386)	(0.0400)
Withholding Base	0.0456	0.0406	0.0305	0.0421	0.0424	0.0371
	(0.0426)	(0.0431)	(0.0454)	(0.0380)	(0.0385)	(0.0403)
Withheld Tax	1.592***	1.586***	1.594***	1.593***	1.592***	1.578***
	(0.0536)	(0.0537)	(0.0541)	(0.0462)	(0.0463)	(0.0466)
Withheld Tax Reclaims	0.744***	0.740***	0.747***	0.704***	0.709***	0.700***
	(0.0485)	(0.0488)	(0.0495)	(0.0423)	(0.0425)	(0.0434)
Compensation Requests	0.0832***	0.0830***	0.0578**	0.0798***	0.0790***	0.0636***
	(0.0225)	(0.0229)	(0.0230)	(0.0189)	(0.0192)	(0.0192)
Final Tax To Remit	0.0516	0.0737	0.126**	0.0277	0.0517	0.0760*
	(0.0504)	(0.0502)	(0.0506)	(0.0439)	(0.0437)	(0.0443)
Taxpayer Sales Tax Remittance	0.0987**	0.119**	0.161***	0.0669	0.0920**	0.107**
	(0.0496)	(0.0495)	(0.0500)	(0.0437)	(0.0435)	(0.0442)
Total Sales Tax Remittance	1.160***	1.158***	1.165***	1.169***	1.173***	1.160***
	(0.0447)	(0.0448)	(0.0462)	(0.0395)	(0.0395)	(0.0407)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXmonth FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	257,796	252,648	235,944	332,100	325,764	303,300

Notes: This table is similar to to the main DiD results Table 3, except that the estimates here are based on an OLS estimation, with outcome variables transformed by the inverse hyperbolic sine (IHS) transformation.

Table F.4: The Impact of Withholding: OLS Estimation on Collapsed Data

	Fully-Balanced Panel			Semesterly-Balanced Panel		
	(1) Trimmed 99.9th pctile	(2) Trimmed 99th pctile	(3) Trimmed 95th pctile	(4) Trimmed 99.9th pctile	(5) Trimmed 99th pctile	(6) Trimmed 95th pctile
Total Sales Reported	0.00910	0.00558	0.0104	0.0135	0.0120	0.0157
	(0.0119)	(0.0120)	(0.0120)	(0.0114)	(0.0115)	(0.0115)
Sales Tax Collected	0.00732	0.00595	0.00550	0.0120	0.0112	0.0117
	(0.0121)	(0.0122)	(0.0130)	(0.0117)	(0.0118)	(0.0126)
Input Tax Credits	-0.0639***	-0.0649***	-0.0655***	-0.0669***	-0.0680***	-0.0699***
	(0.0156)	(0.0159)	(0.0168)	(0.0145)	(0.0147)	(0.0154)
- Import Credits	-0.0828	-0.0931	-0.146**	-0.139***	-0.153***	-0.209***
	(0.0541)	(0.0572)	(0.0655)	(0.0504)	(0.0531)	(0.0609)
- Local Purchase Credits	-0.0590***	-0.0629***	-0.0591***	-0.0610***	-0.0634***	-0.0635***
	(0.0187)	(0.0188)	(0.0195)	(0.0170)	(0.0171)	(0.0175)
Gross Tax Liability	0.429***	0.432***	0.443***	0.410***	0.414***	0.420***
	(0.0209)	(0.0212)	(0.0220)	(0.0188)	(0.0190)	(0.0197)
Withholding Base	-0.00237	-0.00499	-0.00779	-0.0000743	-0.00124	-0.000623
	(0.0158)	(0.0160)	(0.0168)	(0.0138)	(0.0139)	(0.0140)
Withheld Tax	1.321***	1.319***	1.328***	1.307***	1.304***	1.309***
	(0.0306)	(0.0307)	(0.0315)	(0.0270)	(0.0271)	(0.0277)
Withheld Tax Reclaims	1.065***	1.065***	1.072***	1.027***	1.025***	1.027***
	(0.0334)	(0.0339)	(0.0353)	(0.0302)	(0.0307)	(0.0320)
Compensation Requests	0.589***	0.583***	0.484***	0.515***	0.491***	0.439***
	(0.165)	(0.166)	(0.169)	(0.148)	(0.149)	(0.154)
Final Tax To Remit	0.125***	0.129***	0.136***	0.120***	0.124***	0.128***
	(0.0257)	(0.0260)	(0.0272)	(0.0230)	(0.0232)	(0.0242)
Taxpayer Sales Tax Remittance	0.114***	0.118***	0.129***	0.104***	0.108***	0.117***
	(0.0276)	(0.0279)	(0.0287)	(0.0245)	(0.0247)	(0.0256)
Total Sales Tax Remittance	0.805***	0.811***	0.829***	0.815***	0.822***	0.836***
	(0.0257)	(0.0259)	(0.0264)	(0.0227)	(0.0228)	(0.0235)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXmonth FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,322	14,036	13,108	18,450	18,098	16,850

Notes: This table is similar to to the main DiD results Table 3, except that the estimates are from an OLS estimation using outcome data that is collapsed at the taxpayer level for the pre-reform and post-reform period, and then log-linearized. The procedure is based on the suggestion by Bertrand et al. (2004), as implemented in Naritomi (2019).

Table F.5: The Impact of Withholding: Binary Outcomes (1/2)

	Fully-Balanced Panel			Semesterly-Balanced Panel		
	(1) Trimmed 99.9th pctile	(2) Trimmed 99th pctile	(3) Trimmed 95th pctile	(4) Trimmed 99.9th pctile	(5) Trimmed 99th pctile	(6) Trimmed 95th pctile
Total Sales Reported	0.00791**	0.00800**	0.00923**	0.00537	0.00594	0.00684
	(0.00397)	(0.00404)	(0.00428)	(0.00429)	(0.00434)	(0.00461)
Sales Tax Collected	0.00219	0.00217	0.00327	-0.000297	0.000345	0.00135
	(0.00432)	(0.00440)	(0.00468)	(0.00450)	(0.00456)	(0.00484)
Input Tax Credits	0.00204	0.00225	0.00342	0.00345	0.00419	0.00554
	(0.00482)	(0.00490)	(0.00521)	(0.00487)	(0.00494)	(0.00524)
- Import Credits	0.000833	0.000403	0.000149	-0.00128	-0.00168	-0.00139
	(0.00303)	(0.00304)	(0.00318)	(0.00267)	(0.00268)	(0.00280)
- Local Purchase Credits	-0.00259	-0.00186	-0.000510	-0.000103	0.00109	0.00227
	(0.00523)	(0.00530)	(0.00559)	(0.00514)	(0.00520)	(0.00548)
Gross Tax Liability	0.0510***	0.0518***	0.0546***	0.0468***	0.0485***	0.0496***
	(0.00636)	(0.00644)	(0.00671)	(0.00590)	(0.00596)	(0.00624)
Withholding Base	0.00301	0.00292	0.00214	0.00201	0.00228	0.00179
	(0.00492)	(0.00499)	(0.00529)	(0.00440)	(0.00446)	(0.00473)
Withheld Tax	0.207***	0.209***	0.216***	0.214***	0.216***	0.219***
	(0.00962)	(0.00972)	(0.00987)	(0.00831)	(0.00837)	(0.00850)
Withheld Tax Reclaims	0.102***	0.103***	0.107***	0.0977***	0.0998***	0.101***
	(0.00846)	(0.00856)	(0.00876)	(0.00742)	(0.00751)	(0.00771)
Compensation Requests	0.0117***	0.0118***	0.00900**	0.0125***	0.0125***	0.0109***
	(0.00408)	(0.00415)	(0.00429)	(0.00342)	(0.00348)	(0.00359)
Final Tax To Remit	-0.00610	-0.00404	0.00175	-0.00972	-0.00735	-0.00420
	(0.00789)	(0.00797)	(0.00824)	(0.00703)	(0.00709)	(0.00735)
Taxpayer Sales Tax Remittance	-0.00330	-0.00146	0.00289	-0.00682	-0.00449	-0.00258
	(0.00774)	(0.00781)	(0.00808)	(0.00696)	(0.00702)	(0.00729)
Total Sales Tax Remittance	0.111***	0.112***	0.113***	0.120***	0.121***	0.121***
	(0.00683)	(0.00688)	(0.00716)	(0.00605)	(0.00609)	(0.00631)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXmonth FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	257,796	252,648	235,944	332,100	325,764	303,300

Notes: This table is similar to the main DiD results Table 3, except that the outcome is a binary variable capturing whether the underlying outcome is positive or not and we run an OLS rather than a PPML estimation. Table F.6 displays pre-reform means of the outcomes and absolute effect size estimates for the balanced sample.

Table F.6: The Impact of Withholding: Binary Outcomes (2/2)

	Pre-Reform Mean Among Treated			Treatment Effect			Proportional Effect		
	(1) Trimmed 99.9th pctile	(2) Trimmed 99th pctile	(3) Trimmed 95th pctile	(4) Trimmed 99.9th pctile	(5) Trimmed 99th pctile	(6) Trimmed 95th pctile	(7) Trimmed 99.9th pctile	(8) Trimmed 99th pctile	(9) Trimmed 95th pctile
Total Sales Reported	0.984	0.984	0.983	0.00791**	0.00800**	0.00923**	0.00803	0.00813	0.00939
	(0.00161)	(0.00163)	(0.00169)	(0.00397)	(0.00404)	(0.00428)			
Sales Tax Collected	0.979	0.978	0.977	0.00219	0.00217	0.00327	0.00224	0.00222	0.00334
	(0.00161)	(0.00163)	(0.00169)	(0.00432)	(0.00440)	(0.00468)			
Input Tax Credits	0.969	0.969	0.967	0.00204	0.00225	0.00342	0.00211	0.00232	0.00353
	(0.00160)	(0.00162)	(0.00168)	(0.00482)	(0.00490)	(0.00521)			
- Import Credits	0.109	0.0987	0.0747	0.000833	0.000403	0.000149	0.00765	0.00408	0.00200
	(0.00104)	(0.00101)	(0.000934)	(0.00303)	(0.00304)	(0.00318)			
- Local Purchase Credits	0.953	0.953	0.952	-0.00259	-0.00186	-0.000510	-0.00272	-0.00196	-0.000536
	(0.00161)	(0.00163)	(0.00169)	(0.00523)	(0.00530)	(0.00559)			
Gross Tax Liability	0.682	0.682	0.683	0.0510***	0.0518***	0.0546***	0.0748	0.0759	0.0800
	(0.00196)	(0.00198)	(0.00205)	(0.00636)	(0.00644)	(0.00671)			
Withholding Base	0.973	0.973	0.973	0.00301	0.00292	0.00214	0.00310	0.00300	0.00220
	(0.00163)	(0.00164)	(0.00170)	(0.00492)	(0.00499)	(0.00529)			
Withheld Tax	0.625	0.623	0.617	0.207***	0.209***	0.216***	0.332	0.335	0.350
	(0.00199)	(0.00201)	(0.00209)	(0.00962)	(0.00972)	(0.00987)			
Withheld Tax Reclaims	0.271	0.268	0.257	0.102***	0.103***	0.107***	0.377	0.383	0.416
	(0.00180)	(0.00181)	(0.00186)	(0.00846)	(0.00856)	(0.00876)			
Compensation Requests	0.0318	0.0322	0.0320	0.0117***	0.0118***	0.00900**	0.367	0.365	0.281
	(0.000578)	(0.000588)	(0.000609)	(0.00408)	(0.00415)	(0.00429)			
Final Tax To Remit	0.632	0.633	0.635	-0.00610	-0.00404	0.00175	-0.00965	-0.00639	0.00275
	(0.00194)	(0.00196)	(0.00203)	(0.00789)	(0.00797)	(0.00824)			
Taxpayer Sales Tax Remittance	0.620	0.621	0.623	-0.00330	-0.00146	0.00289	-0.00531	-0.00236	0.00464
	(0.00194)	(0.00196)	(0.00203)	(0.00774)	(0.00781)	(0.00808)			
Total Sales Tax Remittance	0.804	0.803	0.802	0.111***	0.112***	0.113***	0.138	0.139	0.141
	(0.00188)	(0.00190)	(0.00197)	(0.00683)	(0.00688)	(0.00716)			
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXmonth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	257,796	252,648	235,944	257,796	252,648	235,944	257,796	252,648	235,944

Notes: This table is similar to Table F.5, but it focuses on the balanced panel, and presents more detailed results, displaying the pre-reform means in the treatment group (columns 1-3), the point estimates from an OLS estimation with binary outcome variables (columns 4-6), and the proportional effect, i.e. the point estimates scaled by the pre-reform mean in the treatment group (columns 7-9).

Table F.7: The Impact of Withholding: IHS Estimation on Sample With Non-Zero Outcomes Pre-reform (1/2)

	Fully-Balanced Panel			Semesterly-Balanced Panel		
	(1) Trimmed 99.9th pctile	(2) Trimmed 99th pctile	(3) Trimmed 95th pctile	(4) Trimmed 99.9th pctile	(5) Trimmed 99th pctile	(6) Trimmed 95th pctile
Total Sales Reported	0.0836**	0.0811**	0.0873**	0.0334	0.0381	0.0349
	(0.0379)	(0.0386)	(0.0410)	(0.0380)	(0.0384)	(0.0405)
Sales Tax Collected	0.0653**	0.0641**	0.0687**	0.0244	0.0275	0.0275
	(0.0309)	(0.0315)	(0.0334)	(0.0302)	(0.0305)	(0.0323)
Input Tax Credits	-0.0563*	-0.0565*	-0.0560	-0.0729**	-0.0684**	-0.0711**
	(0.0317)	(0.0322)	(0.0342)	(0.0308)	(0.0311)	(0.0327)
- Import Credits	-0.0467	-0.0587	-0.118	-0.0778	-0.113	-0.146
	(0.107)	(0.116)	(0.159)	(0.109)	(0.120)	(0.164)
- Local Purchase Credits	-0.0547*	-0.0529	-0.0497	-0.0692**	-0.0631**	-0.0701**
	(0.0330)	(0.0334)	(0.0350)	(0.0315)	(0.0318)	(0.0329)
Gross Tax Liability	0.426***	0.438***	0.459***	0.362***	0.376***	0.391***
	(0.0416)	(0.0418)	(0.0432)	(0.0383)	(0.0383)	(0.0398)
Withholding Base	0.0310	0.0243	0.0255	0.0281	0.0283	0.0311
	(0.0419)	(0.0423)	(0.0450)	(0.0375)	(0.0379)	(0.0399)
Withheld Tax	0.916***	0.906***	0.938***	0.899***	0.891***	0.905***
	(0.0493)	(0.0495)	(0.0486)	(0.0424)	(0.0426)	(0.0420)
Withheld Tax Reclaims	0.699***	0.706***	0.714***	0.645***	0.647***	0.626***
	(0.0617)	(0.0619)	(0.0619)	(0.0576)	(0.0579)	(0.0587)
Compensation Requests	0.688	0.688	0.684	-0.000953	-0.000953	0.101
	(0.584)	(0.584)	(0.587)	(0.646)	(0.646)	(0.681)
Final Tax To Remit	-0.117**	-0.0818	-0.0251	-0.155***	-0.121**	-0.0878*
	(0.0553)	(0.0548)	(0.0553)	(0.0494)	(0.0490)	(0.0492)
Taxpayer Sales Tax Remittance	-0.0845	-0.0506	-0.00285	-0.124**	-0.0916*	-0.0635
	(0.0573)	(0.0568)	(0.0575)	(0.0515)	(0.0512)	(0.0517)
Total Sales Tax Remittance	0.779***	0.782***	0.811***	0.758***	0.764***	0.778***
	(0.0376)	(0.0380)	(0.0393)	(0.0338)	(0.0341)	(0.0353)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXmonth FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	257,796	252,648	235,944	332,100	325,764	303,300

Notes: This table is similar to Table F.3, except that it retains for each outcome variable only firms for which at least 14 of the (at most) 19 pre-reform obervations for this variable were positive. We transform the outcomes using the inverse hyperbolic sine transformation, and conduct an OLS estimation. Given the sample restriction, the estimation sample is different for each outcome variable. Table F.8 shows the number of observations for each outcome and specification in the balanced panel.

Table F.8: The Impact of Withholding: IHS Estimation on Sample With Non-Zero Outcomes Pre-reform (2/2)

	Treatment Effect			N		
	(1) Trimmed 99.9th pctile	(2) Trimmed 99th pctile	(3) Trimmed 95th pctile	(4) Trimmed 99.9th pctile	(5) Trimmed 99th pctile	(6) Trimmed 95th pctile
Total Sales Reported	0.0836**	0.0811**	0.0873**	280368	274716	255816
	(0.0379)	(0.0386)	(0.0410)			
Sales Tax Collected	0.0653**	0.0641**	0.0687**	275184	269568	251100
	(0.0309)	(0.0315)	(0.0334)			
Input Tax Credits	-0.0563*	-0.0565*	-0.0560	260820	255312	236988
	(0.0317)	(0.0322)	(0.0342)			
- Import Credits	-0.0467	-0.0587	-0.118	24480	21024	13500
	(0.107)	(0.116)	(0.159)			
- Local Purchase Credits	-0.0547*	-0.0529	-0.0497	255204	250056	232380
	(0.0330)	(0.0334)	(0.0350)			
Gross Tax Liability	0.426***	0.438***	0.459***	190620	186948	174492
	(0.0416)	(0.0418)	(0.0432)			
Withholding Base	0.0310	0.0243	0.0255	278136	272592	254232
	(0.0419)	(0.0423)	(0.0450)			
Withheld Tax	0.916***	0.906***	0.938***	159624	156060	143460
	(0.0493)	(0.0495)	(0.0486)			
Withheld Tax Reclaims	0.699***	0.706***	0.714***	93060	90468	81684
	(0.0617)	(0.0619)	(0.0619)			
Compensation Requests	0.688	0.688	0.684	2232	2232	2196
	(0.584)	(0.584)	(0.587)			
Final Tax To Remit	-0.117**	-0.0818	-0.0251	167256	164268	153396
	(0.0553)	(0.0548)	(0.0553)			
Taxpayer Sales Tax Remittance	-0.0845	-0.0506	-0.00285	163548	160488	149904
	(0.0573)	(0.0568)	(0.0575)			
Total Sales Tax Remittance	0.779***	0.782***	0.811***	225756	220932	205992
	(0.0376)	(0.0380)	(0.0393)			
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXmonth FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table is similar to Table F.7, but it focuses on the balanced panel, and presents both the point estimates (columns 1-3, same as columns 1-3 in Table F.7) as well as the number of observations underlying each estimation (columns 4-6).

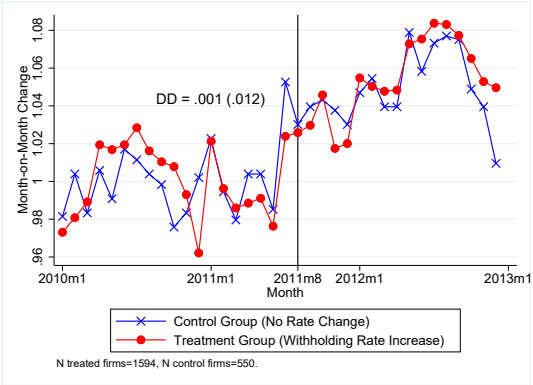
Table F.9: The Impact of Withholding: Robustness When Considering Refunds and Potential Reduction in Income Tax Remittance

	Fully-Balanced Panel			Semesterly-Balanced Panel		
	(1) Trimmed 99.9th pctile	(2) Trimmed 99th pctile	(3) Trimmed 95th pctile	(4) Trimmed 99.9th pctile	(5) Trimmed 99th pctile	(6) Trimmed 95th pctile
Total Sales Tax Remittance	0.410***	0.455***	0.708***	0.405***	0.588***	0.666***
	(0.137)	(0.0967)	(0.0698)	(0.129)	(0.0681)	(0.0606)
Total Sales Tax Remittance - Refund	0.410***	0.457***	0.711***	0.402***	0.591***	0.668***
	(0.138)	(0.0984)	(0.0705)	(0.130)	(0.0692)	(0.0613)
Total Sales Tax Remittance - Refund - IT Compensation	0.385***	0.435***	0.712***	0.376***	0.582***	0.674***
	(0.143)	(0.104)	(0.0747)	(0.135)	(0.0730)	(0.0649)
Sales Tax + Income Tax - Refund	0.310**	0.307***	0.582***	0.379***	0.431***	0.544***
	(0.140)	(0.0900)	(0.0662)	(0.128)	(0.0673)	(0.0547)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXmonth FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	257,796	252,648	235,944	332,100	325,764	303,300

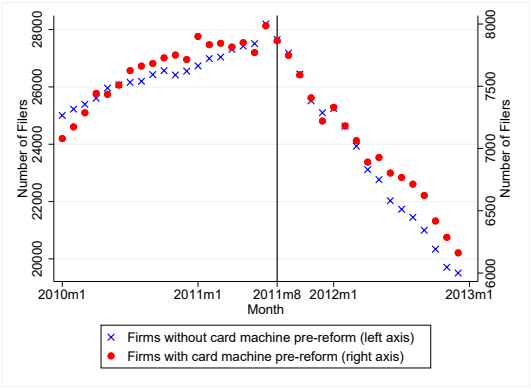
Notes: This table is similar to Table F.2, but considers a different set of outcome variables. The first row reproduces as benchmark the effect of the tax withholding-rate increase on total sales tax remittance from Table F.2. The following rows present the same estimate after accounting for refunds, income tax compensation and potential reductions in income tax remittance. Taxpayers who cannot fully deduct the tax withheld for the sales tax on their sales tax declaration, due to low tax liability, can either request a refund, or deduct the tax withheld on their income tax declaration. The latter process is referred to as compensation. Regarding the estimates on sales tax + income tax - refund, it is useful to note that total sales tax remittances are on average twice as high as income tax remittances. The estimates are consistent with no effect or even a slight increase in income tax compliance. We use OLS estimations rather than PPML for this exercise, as some of the outcome variables can take negative values, which is not allowed under PPML. We display the proportional effects, as in Table F.2.

Figure F.1: No Filing Response to Withholding

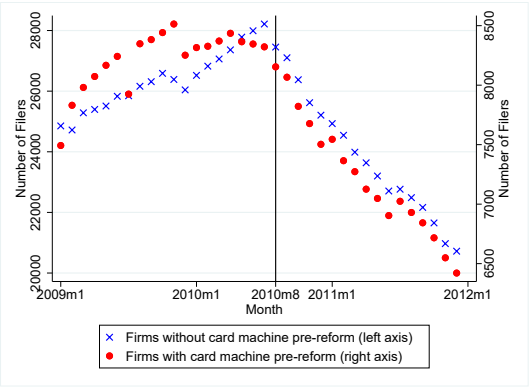
A: DiD-Estimation on Irregular Filer Panel



B: At-least-once Filers Around Reform



C: At-least-once Filers Around Placebo Reform



Notes: This figure illustrates the absence of a filing response to the August 2011 withholding-rate increase. Panel A concerns firms that file at least once per semester in the three years around the withholding reform. This means we can determine treatment status at the firm level. We drop all firms that file regularly, and estimate equation 10 on the remaining panel of irregular filers, which we balanced so that each firm appears every month. The outcome in the DiD estimation is a dummy that indicates if the firm filed a sales tax declaration in a particular month. We relax the balancing restriction in Panel B, which concerns firms that filed at least once before the reform, regardless of whether they continue to file after the reform or not. In this group, we plot the number of filers with and without a card machine prior to the reform, considering that having a card machine is a proxy for treatment status. To establishe a counterfactual, Panel C repeats this exercise, but moves the reform (and requirement that firms in the sample file at least once pre-reform) to a placebo reform date of August 2010.

G The Impact of Withholding – Mechanisms

Table G.1: Heterogeneity of Withholding Impact: Extended Control Group

	Outcome: Reported Gross Tax Liability								
	Evidence for Enforcement Perception Mechanism						No Evidence for Liquidity Mechanism		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated (Withholding Rate Increase)	0.274***	0.250***	0.231***	0.236***	0.158**	0.0269	0.250***	0.262***	0.0107
	(0.0259)	(0.0271)	(0.0280)	(0.0247)	(0.0617)	(0.0619)	(0.0321)	(0.0277)	(0.0707)
Treated X Below Median Turnover		0.258***				0.221***			0.226***
		(0.0386)				(0.0473)			(0.0493)
Treated X Mispreporter			0.166***			0.142**			0.150***
			(0.0531)			(0.0576)			(0.0549)
Treated X First-Time Withholdee				0.572***		0.564***			0.515**
				(0.189)		(0.196)			(0.202)
Treated X Reclaimer					0.129**	0.168***			0.170***
					(0.0628)	(0.0575)			(0.0587)
Treated X Below Median Profitability							0.0349		0.0412
							(0.0375)		(0.0357)
Treated X Buncher								0.0514	-0.0394
								(0.0492)	(0.0499)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CharacteristicsXMonth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	773,514	773,514	773,514	773,514	773,514	773,514	765,892	773,514	765,892

Notes: This table is identical to Table 5 but includes in the control group firms which did not use a card machine, to maximize statistical power.

Table G.2: Impact of Withholding-Rate Increase: Heterogeneity by Rate Change Size

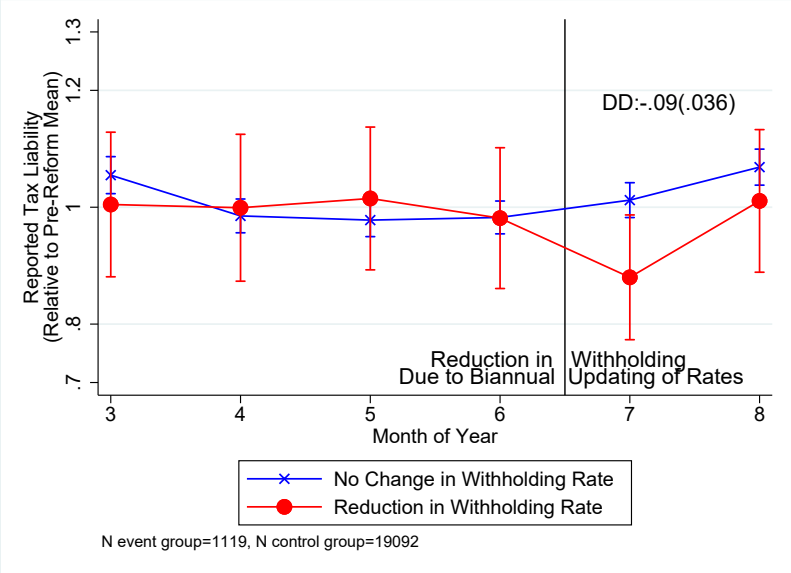
	Outcome: Reported Gross Tax Liability
	(1)
Treated X First-Time Withholdee X Big Rate Change	0.860***
	(0.242)
Treated X First-Time Withholdee X Small Rate Change	0.881***
	(0.161)
Treated X Previous Withholdee X Big Rate Change	0.274***
	(0.0363)
Treated X Previous Withholdee X Small Rate Change	0.137***
	(0.0308)
Month FE	Yes
Firm FE	Yes
CharacteristicsXMonth FE	Yes
Observations	226,372

Notes: This table is identical to Table 5, but presents the coefficient estimates on interactions between the treatment dummy (withholding-rate increase) interacted with dummies to indicate first-time withholdees and previous withholdees, and big (5-6 ppt) and small (1-3 ppt) changes in the withholding rate. We drop firms which experienced a 6 ppt change in the withholding rate, as this can be the case only among first-time withholdees. The estimates are based on a PPML estimation with firm and month fixed effects and a sector-specific linear time trend.

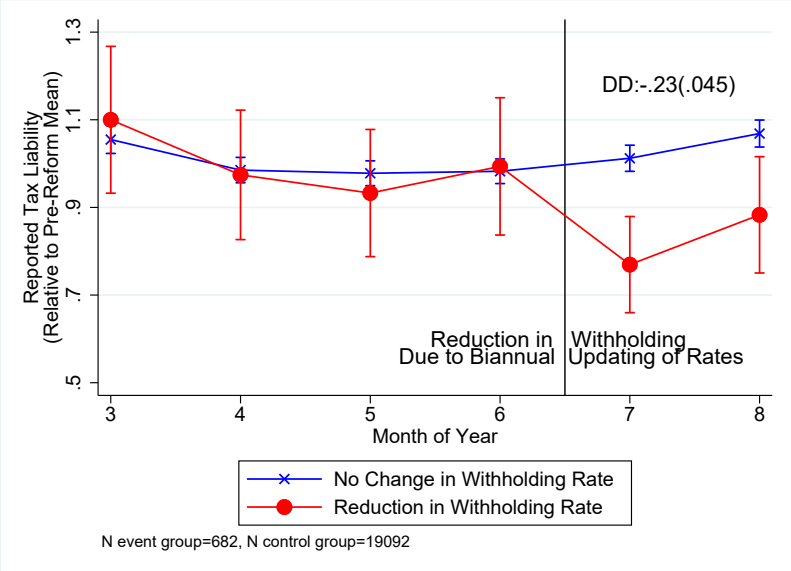
H Event Study of Withholding-Rate Reduction

Figure H.1: Event Study of Withholding-Rate Reduction

A: Small Withholding-Rate Reduction (1-2 points)



B: Large Withholding-Rate Reduction (3-4 points)



Notes: This figure is constructed as Figure 5, Panel A, except that the event groups here contain firms that experienced a decrease in their withholding rate in July, due to the biannual updating of withholding rates. The control group contains firms that experienced no change in the withholding rate.

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