The Impact of Low-Cost Intervention on Tax Compliance: Regression Discontinuity Evidence

Sanchez, Gonzalo E

Facultad de Ciencias Sociales y Humanisticas, Escuela Superior Politecnica del Litoral, ESPOL, Guayaquil, Ecuador

May 2015

The Impact of Low-Cost Intervention on Tax Compliance: Regression Discontinuity Evidence

Gonzalo E. Sánchez *

February, 2016

Abstract

This paper examines the effectiveness of low-cost tax enforcement on compliance using administrative data from Ecuador. To overcome confounding factors, I use a regression discontinuity design that exploits a discrete increase in the probability of receiving a non-compliance notification. Results indicate that the notification significantly increases taxes paid by around $1,400, or 70 percent. Additional findings suggest that this intervention also increases taxes reported in the year following it, and that some taxpayers strategically attempt to evade taxes while trying to avoid being notified. Collectively, these findings suggest that inexpensive interventions can substantially improve tax compliance in low-income countries.

Keywords: Ecuador, tax compliance, tax enforcement, tax evasion, regression discontinuity.

JEL classification: H25, H26, K42

*Facultad de Ciencias Sociales y Humanísticas, Escuela Superior Politécnica del Litoral, ESPOL (email: edsanche@espol.edu.ec). I thank the Ecuadorian Internal Revenue Service (*Servicio de Rentas Internas*-SRI) and the Center of Fiscal Studies of Ecuador (*Centro de Estudios Fiscales del Ecuador*) for their support and assistance. In particular, the access granted by the SRI to its anonymous databases and statistics was key in the realization of this project. I am especially indebted to Leonardo Orlando, César Cueva, Diana Arias, and Rolando Mantilla. I thank Mark Hoekstra, Andrew Rettenmaier, Jonathan Meer, and Jason Lindo for their advice and suggestions. Thanks also go to workshop participants at the Center of Fiscal Studies of Ecuador and the Department of Economics at Texas A&M University, for helpful comments and discussions. The results presented in this paper do not reflect the opinions of the aforementioned organizations. Any errors are my own.

1 Introduction

Tax evasion is a significant problem facing countries around the world. It imposes efficiency costs by either reducing the availability of public goods and services, or requiring higher and more distortionary taxes to meet a fixed revenue requirement. Tax evasion also creates inequality because taxpayers with the same tax liability end up with different tax burdens (Slemrod, 2007). As a result, understanding the impact of various forms of tax compliance enforcement mechanisms is required to improve the efficiency of tax systems. This issue is particularly important for low-income countries, where the size of the shadow economy is estimated to be around 35 percent of GDP, versus 17 percent for high-income countries (Schneider et al., 2010).

The main objective of this paper is to examine whether low-cost tax enforcement methods can be used to improve compliance in low-income countries. The leading challenge that must be overcome in order to do so is the selection bias that arises because enforcement usually targets taxpayers who are more likely to evade taxes. Thus, to credibly estimate the causal effect of enforcement on compliance, one must disentangle the effect of the enforcement from the effect of being the type of taxpayer who is targeted for enforcement.

This paper overcomes the selection bias by using a regression discontinuity design (RDD) that takes advantage of a discrete increase in the probability of receiving a formal notification (treatment). Specifically, I compare the behavior of taxpayers marginally selected to be sent a notification of non-compliance (because they under-reported an amount that falls just above a selection threshold) to the response of those marginally not chosen (because they under-reported an amount that falls just below a selection threshold).

The identifying assumption of this paper is that all determinants of tax compliance, other than the formal notification, are continuous across this selection threshold. This is likely to hold, as the cut-off was defined after taxpayers had reported their tax liability, was based on labor constraints limiting the delivery of the notifications, and was never announced to or known by taxpayers. Moreover, no other enforcement policy changed at the selection threshold. In addition, I show empirical evidence that supports the identifying assumption. The observed determinants of tax compliance are continuous across the threshold, there is no evidence of bunching around the cut-off, and the RDD estimates do not change significantly when additional covariates are included. As a result, I am confident that this research design distinguishes the effect of the enforcement method from the effects of other observable and unobservable factors.

In examining the effectiveness of tax compliance strategies, this paper joins the literature aimed at estimating the degree of evasion as well as the impact of different strategies on compliance. One part of the literature has used data from the Tax Compliance Measurement Program (TCMP) in the U.S. to examine the impact of aggregate audit rates on compliance (See for instance Dubin and Wilde (1988); Dubin et al. (1990); Plumley (1996); Dubin (2007)).[1]

Much of the recent research on tax compliance has come from laboratory experiments, which have the advantage of controlling for particular circumstances such as enforcement effort, tax rates, income levels, etc. For instance, Alm et al. (1993a) found that audit rules that depend on the behavior of taxpayers generate greater compliance than random audit rules.[2]

More related to this paper is the smaller literature in which randomized controlled field experiments were performed. In these studies, real taxpayers are exposed to variation in some controlled variable. For example, Blumenthal et al. (2001) study the impacts of moral appeal letters on tax compliance in Minnesota, and find that compliance was higher in the treatment groups, but the effect was not significant. Torgler (2004) studies the effect of moral suasion on tax compliance in Switzerland, and finds that a letter emphasizing the importance of compliance for the development of the community had no effect.[3]

The literature on tax compliance has focused on traditional factors such as penalties, probability of audits, and tax rates. In recent years, behavioral aspects such as social norms and moral appeals have also been studied. Nonetheless, little attention has been given to the effect of low-cost enforcement methods commonly used by tax authorities.

In that context, one innovation of this paper is the analysis of an enforcement

[1]The Tax TCMP is conducted by the U.S. Internal Revenue Service. Randomly individual income tax returns are selected and subject to an audit (Internal Revenue Service, 1996, 2007).

[2]Other studies using laboratory experiments to analyze tax compliance include Alm et al. (1992, 1993b, 1999, 2009, 2010); Bazart and Bonein (2013); Bosco and Mittone (1997); Calvet and Alm (2012); Coricelli et al. (2013); Djawadi and Fahr (2013); Friedland et al. (1978); Guala and Mittone (2005); Mittone (2006); Torgler (2002); Tan and Yim (2013), among many others.

[3]Other field experiments on tax compliance include Slemrod et al. (2001), Hasseldine et al. (2007), Kleven et al. (2011), Gemmell and Ratto (2012), Fellner et al. (2013), and Pomeranz (2013).

strategy actually applied to increase compliance. In particular, I study the causal effect of tax notifications on tax compliance. I use information corresponding to taxpayers for which under-reporting of the Income Tax Advance (ITA) in Ecuador for the fiscal year 2010 was detected. Specifically, among these taxpayers, I examine all corporations as well as those individually-owned businesses that are obligated to keep accounting records. Hereafter, the term *taxpayer* is used to refer to these two groups.

This paper also contributes to the literature by applying a quasi-experimental design to cleanly identify the effects of a low-cost tax enforcement method in a low-income country. In particular, this is the first paper, to my knowledge, that uses RDD to study tax compliance, and one of the few that analyses tax compliance in low-income countries (see for instance Carrillo et al. (2012); Fisman and Wei (2004); Olken and Singhal (2011); Kleven and Waseem (2012); Kumler et al. (2012)). Moreover, I am able to examine the persistence of the effects of this low-cost tax enforcement method, which allows the empirical analysis of an endogenous rule of enforcement (selection using a cut-off rule), instead of the traditional constant probability of audit.

Results indicate that tax notifications cause the probability of correcting the tax report to significantly increase by around 67 percentage points and the amount reported by approximately $1,400 or 70 percent. The estimated impact represents the marginal effect of sending an additional notification on tax revenues. On the other hand, the cost of this program includes printing and delivery expenses, and the value of the time spent by tax officials designing and monitoring it. Even though I do not have an accurate estimation of the marginal cost of sending an additional notification, tax officials estimate the average cost to be around $80. Arguably, the marginal cost should be less than the average cost; hence a back-of-the-envelope calculation suggests that the net marginal benefit is at least $1,300, which represents a return of around 1,500 percent.

I also find suggestive evidence that the effect persists for the following year. Treated units reported more Income Tax Advance in 2011 than the non-treated group. Moreover, those receiving the tax notification were more likely to over-report and less likely to under-report in that year. On average, and conditional on non-compliance, taxpayers under-reported less in 2011 than in 2010. Interestingly, that gap was greater for units receiving the treatment around the selection threshold. If taxpayers believe that the probability of getting a tax notification is not random, but an increasing function of the under-reported amount, then this suggests that some of them strategically attempt to evade taxes while trying to avoid being notified.

These results have significant implications for tax compliance in low-income countries. They indicate that formal notifications are effective in reducing evasion and increasing tax revenues. Moreover, the results suggest that the expansion of enforcement methods such as this could further increase tax compliance and revenues, and potentially reduce the efficiency costs and inequality created by tax evasion.

The remainder of the paper is organized as follows. Section 2 presents the institutional background. Sections 3 and 4 describe the data and research design. The results are summarized in section 5 and section 6 concludes.

2 Institutional Background

This paper analyzes taxpayers who under-reported the Income Tax Advance (ITA) in Ecuador for the fiscal year 2010.[4] In particular, among these taxpayers, I examine all corporations and only those individually-owned businesses which are obligated to keep accounting records.[5] For them, the ITA is determined as the sum of 0.4 percent of the total assets, 0.4 percent of the total taxable income, 0.2 percent of net worth, and 0.2 percent of deductible expenses.[6]

This tax is determined when taxpayers file their income tax reports for the previous fiscal year. The applicable income tax to be filed during the next fiscal period, which corresponds to the current period, is equal to the ITA or the regular income tax, whichever is greater.[7] In other words, the ITA is in practice a minimum income tax. Moreover, in the current period, taxpayers have to pay an amount equal to this tax

[4] The tax legislation pertinent for this research was in effect in the years 2010 and 2011. A few reforms have been implemented since then; however, they are not relevant for the purpose of this research.

[5] Individuals are obligated to keep accounting records if they carry out businesses and if they have yearly revenues greater than \$100,000, or yearly costs and expenses greater than \$80,000, or begin economic activities with a capital of at least \$60,000. Individuals not obligated to keep accounting records and those corporations that have contracts to explore and exploit hydrocarbons determine the ITA as 50 percent of the previous year's income tax minus withholdings corresponding to that period.

[6] There are some exemptions to this formula for financial institutions, agricultural businesses, leasing companies, new businesses, among others.

[7] The income tax for individuals is calculated using a progressive (from 5 percent to 35 percent) tax schedule. The corporate income tax for 2010 and 2011 was calculated using a flat rate of 25 percent and 24 percent respectively.

minus taxes withheld in the previous period (i.e., anticipated payment).[8] Appendix A1 presents examples for various cases. Other Latin-American countries that have taxes similar to the ITA include Argentina, Colombia, Mexico, Nicaragua, Peru, and the Dominican Republic (González, 2009).

Since the ITA is not automatically calculated when taxes are filed, the actual amount is determined by the taxpayer. To detect under-reporting, the Ecuadorian Internal Revenue Service (SRI) takes the reported variables as given and applies the corresponding formula. Thus, under-reporting is calculated as the difference between the amount reported by the taxpayer and the one determined by the tax authority. Hence, the measure of non-compliance analyzed in this paper is the result of an incorrect application of the formula to calculate the ITA. More complex methods of evasion such as under-statement of income or assets are not studied in this paper.[9]

To enforce ITA compliance, and according to Ecuadorian tax regulations, the tax authority has implemented a system of notifications sent to taxpayers for whom under-reporting is identified. In a first stage, "persuasive communications" are sent to taxpayers for whom electronic mail is available. These warnings are only informative and state the detected difference and the steps needed to correctly re-file. Then, in a second stage, written "notifications of differences" (hereafter, tax notifications) are sent to selected taxpayers who have not correctly adjusted their tax reports yet, including those who received the first communication and those who did not.[10]

This paper studies the causal effect of these tax notifications (treatment) on compliance. These notices do not imply a penalty; however, taxpayers are warned that they have under-reported the ITA and notified of the detected difference. The notification states that if the difference is justified, or the tax report is correctly adjusted

[8]The anticipated ITA is split in two equal installments to be paid in July and September of the corresponding year.

[9]There are two main ways to evade the ITA: misusing the calculation formula and under-stating the components of the formula. Arguably, the latter is riskier (accounting fraud may result in criminal prosecution, whereas, the misuse of the formula might cost accrued interest and fees), more difficult (double-accounting is necessary) and could increase the tax burden (for instance under-stating expenses increases the income tax due). Hence, taxpayers trying to evade the ITA may have incentives to misuse the formula instead of under-state its components. The next section shows that in 2010 a relatively large number of taxpayers under-reported the ITA by applying the calculation formula incorrectly.

[10]The deadline to file income tax reports in Ecuador is in March for individuals and in April for corporations. The specific day depends on the ninth digit of the taxpayer identification number. The tax authority starts sending persuasive communications to taxpayers under-reporting the ITA around June. Tax notifications are sent after that. The precise dates are unknown by the researcher.

within 20 business days of receiving the notice, no further action will be taken.[11] It also says that if the difference is not justified or corrected in time, the tax authority will rectify the value of the tax and will send the taxpayer a bill to be paid immediately according to the law.[12]

Written tax notifications are less expensive than intensive enforcement methods, such as audits. Nevertheless, they still represent a cost. Hence, given the resource constraints, not all taxpayers who under-report the ITA are sent these communications. The Department of Control of the SRI selects the taxpayers who are scheduled to be sent tax notifications. The selection is done for each of the 24 provinces of Ecuador individually. In each province, taxpayers are ranked by their under-reported amount. Then, the number of chosen taxpayers is determined as a function (unknown by the researcher) of the number of tax officials available in each province. Specifically, for each province, only the taxpayers whose under-reported taxes are greater than a given amount (selection threshold) are selected to be sent the notification.

It is important to mention that not all the selected taxpayers receive the tax notification. It is possible that some of them are not found by the delivery person. On the other hand, it is also possible that taxpayers not selected are sent the notification anyway. However, as the results section shows, the probability of receiving the notification increases discontinuously at the selection threshold.

3 Data

The dataset used in this paper includes business-level observations corresponding to the total number of taxpayers for whom the tax authority detected under-reporting of the ITA 2010 before the selection process for the tax notifications (pre-treatment ITA 2010).

The data were provided by the Ecuadorian Tax Authority, specifically by its Tax Control Department and its Center of Fiscal Studies. These data consist of 39,223

[11]If the tax notification is received after the deadline to pay the first installment, interest is accrued.

[12]The Ecuadorian tax regulations state that this bill will include accrued interest and a penalty equal to 20 percent of the value of the ITA. There is an additional 20 percent fine if the taxpayer did not determine this tax at all. The monthly interest rate on unpaid taxes for the third quarter of 2010 was 1.021 percent. This interest rate is calculated as 1.5 times the 90-Day Loan Reference Rate determined by the Central Bank of Ecuador.

observations (around 7 percent of the total number of corporations and individually-owned businesses obligated to keep accounting records).[13]

To avoid confounding under-reporting with rounding, I restrict the sample to taxpayers with under-reported pre-treatment ITA 2010 of more than one dollar (37,249 observations). Also, observations that belong to the 99 percentile of the continuous outcome variables (introduced below) were trimmed out. These "outliers" drive the local averages up in a manner that would not allow readers to distinguish local effects graphically. These changes facilitate the presentation of graphical evidence using outcome variables directly, instead of logarithmic transformations or estimated regression residuals.

Importantly, these changes do not bias the estimators of the effect of the intervention since the probability of being an "outlier" is not correlated with the treatment variable. Using regression models similar to those utilized to estimate the main results of this paper (presented in the next section), I found, across various specifications, no discontinuous change in the likelihood of being an "outlier" at the selection threshold. In addition, the results of this paper are robust to these changes. The resulting sample includes 36,457 observations.

Table 1 shows the frequencies of the data by province. Around 62 percent of the observations belong to the two biggest provinces of Ecuador, Guayas and Pichincha. Table 1 also shows that 5,028 (13.79 percent) taxpayers were chosen from the sample (using the selection thresholds explained before) nationally to be sent the tax notification and that 4,822 (13.23 percent) actually received them (were treated). This discrepancy occurs because the selection process was not perfectly implemented as explained in the previous section.

Panel A of Table 2 shows summary statistics of the outcome variables. The first variable is binary and takes the value of one is the taxpayer corrected or justified the detected difference by the end of 2010 and zero otherwise. This variable is used to measure the effect of the enforcement method on compliance.[14] Table 2 shows that around 23 percent of the taxpayers corrected their reports or justified the differences

[13] All the calculations of compliance were produced by the SRI.

[14] For the taxpayers that received the tax notification a variable that specifies if they corrected or justified the detected difference is used. For the taxpayers that did not receive this communication, I have information on whether they re-filed their tax report or not by the end of the year. In some cases there is still a difference between the new tax report and the amount estimated by SRI. If that difference is less than one dollar, I consider that the taxpayer rectified the tax report.

by the end of the year.

The second outcome variable represents the post-treatment measure of the reported ITA in 2010.[15] In particular, this variable is the dollar amount of the ITA 2010, reported along with the income tax report corresponding to that year (filed in 2011, see the previous section).[16] This variable is used to analyze the effect of the enforcement method on reported taxes. Its mean is US$ 1,804 with a standard deviation of US$ 5,131.

To analyze the effects of the treatment for the following year, I use data on the reported ITA 2011. To avoid confounding the effect under analysis with other enforcement programs in 2011, I consider the last report filed by the taxpayer before June 2011.[17] This variable has a mean of US$ 2,923 with a standard deviation of US$ 6,907.

The dataset also includes the ITA 2011 calculated by the tax authority. This variable was used to calculate under-reporting and over-reporting in 2011. Panel A of Table 2 shows that 60 percent of the taxpayers in the sample under-reported, and that 28 percent over-reported in 2011. The median under-reported amount is US$ 522 and the median over-reported amount is US$ 53.[18]

Additional covariates are included in some specifications to reduce the sample variability of the estimates as suggested by Lee and Lemieux (2010). Following the literature on the determinants of tax compliance, additional covariates include measures of the size of the business (total assets, taxable income, net worth deductible costs, and expenses), characteristics of the business (years of operation, special taxpayer indicator, indicator for corporations, economic activity fixed effects, and province fixed effects), and characteristics of the legal representative (gender, age, and level of education).[19] All these variables correspond to the pre-treatment period.[20]

Panel B of Table 2 shows summary statistics for the covariates. Average taxable

[15] This variable includes changes to the ITA 2010, if any, made by the taxpayer after the treatment period.

[16] Since 2000 the American Dollar is the official currency in Ecuador.

[17] As noted before, the tax authority starts sending communications to taxpayers under-reporting the ITA around June of each year.

[18] I code under-reporting (over-reporting) as 1 if there is a positive (negative) difference between the ITA reported by the taxpayer and the one calculated by the tax authority of more than one dollar.

[19] Special taxpayers are those required to withhold taxes from other taxpayers.

[20] Very comprehensive literature reviews on the determinants of tax compliance can be found in Andreoni et al. (1998); Slemrod (1992) and Torgler (2007).

income is US$ 422,448, with a standard variation of US$ 891,658. The other variables representing the size of the business are also presented in the table. In addition, the average age of the legal representatives is 48 years. Approximately 28 percent of them are female, and 45 percent have a college education. The average years of operation of the businesses is 12.7 years. Around 5 percent of them are special taxpayers, and 57 percent are corporations.

4 Research Design

I use regression discontinuity design (RDD) to estimate the causal effect of tax notifications on compliance. By taking advantage of a discrete increase in the probability of receiving these notifications, this paper compares the response of taxpayers marginally selected to be sent the notifications (because their under-reported amount falls just above a selection threshold) to the response of those marginally not chosen (because their under-reported amount falls just below a selection threshold).

To apply this design, I use the selection thresholds or cut-offs for each province to define the running variable. Since the selection cut-offs vary across provinces, the running variable is centered (detected difference minus the cut-off in each province) and standardized. Hence, the running variable is defined as standard deviations away from the cut-offs.

The identifying assumption of the RDD in this context is that all determinants of the outcome variables other than the tax notifications are continuous across the threshold. Under that assumption, any discontinuity in outcome variables at the cut-off is properly interpreted as the effect of the tax enforcement strategy, rather than as the effect of other observable (income, assets, years of operation, etc.) or unobservable (knowledge of regulations, tax evasion behavior, other tax enforcements strategies, etc.) determinants of tax compliance.[21] Consequently, under the identifying assumption, this design produces a consistent estimation of the causal effect of tax the notifications.

Since the selection process was not implemented perfectly (as explained in the

[21] Among the unobservable enforcement strategies is the persuasive notification explained in the previous section. There is no information about which taxpayers actually receive this communication; however it was sent to taxpayers for whom electronic mail was available. In that sense, it is difficult to believe that the probability of receiving the persuasive notification changed discontinuously at the selection threshold for the tax notifications.

previous section), an estimation of the discontinuity on the probability of treatment (first stage) is needed. I estimate it by using a polynomial regression as follows:

$$treated = \alpha_1 + f_l(d) + \beta_1(above) + f_r((above) * d) + u \tag{1}$$

In equation (1) *treated* is a binary variable equal to one if the taxpayer received the tax notification and zero otherwise; d is the running variable as defined before; f_l and f_r represent polynomial functions estimated to the left and to the right of the cut-off point respectively; *above* is a binary variable equal to one if the centered running variable positive and zero otherwise; and u is the error term.

The discontinuities on the outcome variables are estimated as follows:

$$outcome = \alpha_2 + h_l(d) + \beta_2(above) + h_r((above) * d) + e \tag{2}$$

In equation (2) h_l and h_r represent polynomial functions estimated to the left and to the right of the cut-off point respectively; e is the error term; and the other variables are the same as in equation (1).

Hence, β_1 is the estimator of the discontinuity of the probability to receive the treatment and β_2 is the estimator of the discontinuity in the outcome variable. As discussed before, the jump in the probability of treatment is less than 100 percent. Therefore, the discontinuities of the outcome variables represent the intent-to-treat (ITT) effect. Thus, they have to be re-weighted by the treatment discontinuity. Following Hahn et al. (2001), I utilize a Fuzzy Regression Discontinuity Design (FRD) and apply Two-Stage Least Squares (2SLS) to estimate the Local Average Treatment Effect (LATE) of the enforcement program. Robust standard errors were used for all the specifications.

This identification strategy allows the estimation of a local effect that holds only for those units around the selection threshold. Assuming that the effect of the treatment is heterogeneous across units, the FRD identifies the effect for compliers. In other words, the effect for those taxpayers who were treated because the amount they under-reported was above the selection threshold, and would not have been treated if the threshold were higher.

As it is well known in the RDD literature, it is desirable to use data close to the cut-off point to avoid the potential bias of estimation of discontinuities with large bandwidths. However, estimations with small bandwidths could produce imprecise estimates. Hence, there is a tradeoff between bias and precision when selecting the bandwidth. To address this issue, I report regression results using bandwidths of 1, 0.5, and 0.25 standard deviations, with and without additional predetermined control variables.

5 Results

5.1 Testing the Identifying Assumption

As discussed before, the identifying assumption in this paper is that all determinants of tax compliance, other than the enforcement method, are continuous across the selection threshold. This assumption will fail if taxpayers were able to manipulate the side of the threshold on which they fall, or if other tax policy or enforcement method changed at the cut-off. That is arguably unlikely for a number of reasons.

As described before, the selection for treatment is implemented after taxes are filed, which means there is no way for taxpayers to know beforehand where they are relative to the cut-off. Moreover, the selection method (not only the selection threshold) is only known by tax officials and determined as a function of the availability of tax officials in each province. Furthermore, the tax authority did not change anything else at the cut-off level. That is, the cutoff was only used for this intervention, and not for others.

Importantly, the empirical evidence is consistent with the lack of manipulation around the selection threshold. Figure 1 shows that there is no bunching in the distribution around the cut-off that would suggest that taxpayers can control where they are relative to it. I also use the density test suggested in McCrary (2008) and fail to reject the null hypothesis of continuity of the density function at the threshold.[22]

Furthermore, the observed pre-treatment covariates are locally balanced around the selection threshold. Appendix A2 shows scatter plots of local averages of the available pre-treatment covariates and the running variable along with fitted values from a polynomial regression model, flexibly estimated on each side of the cut-off point. It is difficult to see discontinuities that may suggest that covariates are unbalanced on the two sides of the threshold. Moreover, for each variable, discontinuities at the

[22]The statistic found is equal to -0.06 (implying a log discontinuity in the discontinuity of 6 percent) that is not significant (t-stat of -0.9).

cut-off point were estimated. Appendix A2 shows that they cannot be statistically distinguished from zero. For instance, the estimated discontinuity for the variable taxable income is less than 2 percent and it is not significant.

Finally, as explained below, the inclusion of additional covariates did not significantly change the estimated parameters, but reduced its standard errors. Hence, the empirical evidence suggests that the identifying assumption holds.

5.2 Discontinuity in the probability of treatment

I begin by estimating the discontinuity in the probability of receiving the tax notification. Figure 2, which takes the same form as those figures after it, shows the probability of treatment on the vertical axis, and the running variable on the horizontal axis. I use open circles to represent local averages of the dependent variable, and solid lines to represent a flexible polynomial of the running variable fitted using a bandwidth of one standard deviation around the selection threshold.[23] The order of the polynomial was selected among linear, quadratic and cubic orders, using the Akaike information criterion (AIC) as selection method.

The AIC was calculated as:

$$\text{AIC} = Nln(\hat{\sigma}^2) + 2p$$

where $\hat{\sigma}^2$ is the mean squared error of the corresponding regression model (equation (1) or (2)), and p is the number of parameters in the regression. The selected order of the polynomial is the one that produces the lowest AIC.

Table 3, like all tables following it, shows regression estimates for different combinations of bandwidths and order polynomials. The preferred specifications were selected using the AIC statistic (among linear, quadratic, and cubic orders) for the reduced form models that include additional covariates. These regressions include additional covariates because they help to improve precision and to reduce small sample biases.

Figure 2 shows that there is a large change in the probability of receiving the tax notification at the selection threshold. Table 3 shows the corresponding regression results obtained using equation (1) for different specifications. It confirms the graphical evidence, and shows that the discontinuity in the probability of receiving

[23] The width of the bin used to calculate the local averages is 0.1 standard deviations. Similar plots were obtained when using different widths.

the treatment, or first stage, is around 75 percentage points. It is important to note that the coefficients are very similar across specifications and all are significant at the 1 percent level. As expected, the inclusion of additional covariates reduces the standard errors of the estimates.

5.3 Effects on compliance and reported taxes

To analyze the discontinuities in outcome variables, I use figures with the same features as Figure 2. The regression results in Tables 4 through 6 present estimates for the intent-to-treat (ITT) and local average treatment effect (LATE).

The first outcome variable is used to study the effect of the enforcement method on the probability of compliance (correcting or justifying the detected difference). It is measured by a binary variable as explained in the previous section.

Figure 3 shows that the probability of compliance changes significantly at the selection threshold. Table 4 shows the corresponding regression results. The estimated discontinuity, or intent-to-treat effect, is around 50 percentage points, while the LATE is approximately 67 percentage points. All the coefficients are significant at the 1 percent level. The point estimates are robust across specifications, and the inclusion of additional covariates does not significantly change them, but reduces their standard errors. These findings imply that tax notifications are effective in improving compliance.

To study the effect of the program on reported taxes (i.e., taxes actually paid), I use the post-treatment ITA 2010. Figure 4 shows that the estimated discontinuity for this variable seems to be large. Table 5 complements the graphical evidence with regression results. The coefficients estimated in regressions that include additional covariates are larger, but not significantly different, than those which do not. As expected, the standard errors in the regressions that include covariates are smaller. The preferred estimates for the discontinuity range from around $1,000 to $1,400, and the effect adjusted by the treatment discontinuity ranges from approximately $ 1,360 to $1,860. These estimates are significant at the 1 percent level.

There are 1,524 taxpayers in the dataset (around 3.8 percent) not reporting the post-treatment ITA 2010, 51 of which received the tax notifications. The main reason for attrition is that taxpayers stopped economic activities. This could be problematic if the tax notification changes the probability of attrition and modifies the composition of those remaining in the sample. For instance, assuming that the tax notification increases the probability of attrition, if those getting out of business would have had the smallest reported taxes had they remained, then my estimates could overstate the impact of the notification.

To address this potential problem, I take two steps. First, I explicitly test whether there is a discontinuity in the likelihood of attrition at the cut-off. Results presented in Panel A of Table 7 indicate that the LATE estimates are small and statistically indistinguishable from zero, which suggests that attrition is unlikely to bias the results.

In addition, I perform a bounding analysis similar to the one used by Lindo et al. (2010), who adapted the trimming procedure suggested by Lee (2009) to a regression discontinuity design using a bootstrap method.

Specifically, suppose that the tax notification increases the probability of attrition, then I estimate the lower (upper) bound of the estimated impact assuming that receiving the tax notification causes taxpayers to stop economic activities who would have reported the least (most) if they had remained in business. Then, to make groups to the both sides of the cut-off comparable, I drop the taxpayers with the least (most) reported taxes from the units to the left of the selection threshold. I use the estimated impact of the notification on the probability of attrition to calculate the share of taxpayers who needs to be trimmed out.[24]

Finally, I estimate the impact for this modified sample. Panel A of Table 7 shows the LATE estimates (bootstrapped standard errors in parenthesis) that correspond to the preferred specification for each bandwidth. Both the lower and upper bounds are statically indistinguishable from the RDD estimates. These findings support the hypothesis that attrition does not bias the estimated impact of the tax notification.

The estimated impact in the post-treatment ITA 2010 represents the marginal effect of sending an additional notification on taxes reported. On the other hand, the cost of this enforcement program includes printing and delivery expenses, and the value of the time spent by tax officials designing and monitoring it. Even though I do not have an accurate estimate of the marginal cost of sending an additional notification, calculations by tax officials estimate the average cost at around $80.

[24]Following Lindo et al. (2010), in any bootstrap replication in which the estimated change in the probability of attrition is negative, taxpayers with the highest (lowest) reported taxes from the group to the right of the cut-off are dropped when estimating the lower (upper) bound.

The marginal cost should arguably be less than the average cost; hence a back-of-the-envelope estimation of the marginal benefit brings about a lower-bound of around $1,300 or 1,500 percent.

5.4 Subsequent effects

One advantage of my data is that they also enable me to test whether the effects of this intervention change longer-term behavior as well. Using data on the reported ITA corresponding to 2011, I analyze the effects of the treatment (tax notifications sent in 2010) for the year following the intervention.

There is suggestive evidence that the effect of the treatment persists for the next period. Figure 5 shows that there is a discontinuity in reported ITA 2011 at the selection threshold. Table 6 presents the corresponding regression results. Most of the estimates are not significant in the regressions without additional covariates. On the other hand, the coefficients in the regressions with additional covariates are significant at 1 percent or 5 percent level. The changes in the significance levels seem to be driven by reduction in standard errors rather than by alterations in the point estimates. The preferred estimates for the intent-to-treat effect range between $394 and $575, whereas the reweighted coefficients range from $506 to $ 768. Thus, the evidence suggests persistence of the treatment effect, but in a reduced magnitude.

As for the ITA 2010, there is attrition for the reported ITA 2011. To rule out bias caused by attrition, I follow the same steps used for the reported ITA 2010 in the previous sub-section. Panel B of Table 7 shows that there is no evidence of impact of the tax notification on the probability of attrition. Moreover, the estimated upper and lower bounds are both statistically indistinguishable from the RDD estimates. These findings suggest that attrition does not bias the estimates of the impact of the tax notification.

To better understand the mechanisms that explain the persistence of the tax notifications effect, I use three variables. The first is an indicator for under-reporting in 2011, as explained in Section 3. Figure 6 shows that there is a discontinuous reduction in the likelihood of under-reporting at the threshold point. Panel A of Table 8 complements the graphical evidence and shows regression estimates. In particular, the coefficients in the preferred specifications reweighted by the discontinuity on treatment probability range from -7.7 percent to -11.6 percent and are significant at the 1 percent and 5 percent levels. The regressions which do not control for additional covariates produced similar coefficients. The estimated effect can be interpreted as the extensive margin deterrence effect for the year following the enforcement program.

I also analyze an indicator for over-reporting in 2011. Figure 7 shows that the probability of over-reporting jumps up at the cut-off point. The corresponding estimates in Panel B of Table 8 appear to change moderately across the different order polynomials and bandwidths. Nevertheless, there is suggestive evidence of a small positive effect of the tax notifications on the probability of over-reporting. Specifically, the LATE estimated for the preferred specifications are between 4.7 percentand 10.1 percent.

The estimated effect could be explained if taxpayers are willing to over-report in an attempt to reduce the probability of being notified or audited (Andreoni et al., 1998). In the context of this article, those taxpayers who received the tax notification in 2010 might expect a higher risk of being audited or being part of an enforcement program than those who did not. This could happen if taxpayers believe that the tax authority follows a conditional future audit rule in which past non-compliers will be audited more frequently in the future (Alm et al., 1993a). Hence, some of them over-report to reduce the probability of these undesirable events.

The three previous results imply the absence of the "bomb crater effect" introduced by Guala and Mittone (2005) and Mittone (2006). [25] The authors found in experimental settings that after an audit, evasion remained high for a few rounds and then decreased. If audits rules are believed to be random, the "bomb crater effect" can be explained by the "gambler's fallacy effect" (misperception of probabilities). In this case, the assumption that a random audit is less likely to occur because it recently happened (Kirchler, 2007).

In the context of this study, the absence of the "bomb crater effect" in the results can be explained by the perception of endogenous audit rules such as the conditional future audit rule described above or because of the absence of the "gambler's fallacy effect".

The last outcome variable is the difference between the under-reported amount in 2011 and 2010, as defined in Section 3. On average, this variable is negative (taxpayers

[25]The authors derived this term from the First World War: troops under enemy fire hid in craters of recent explosions because they believed it to be very unlikely that two bombs will fall exactly in the same spot in a short period of time.

under-reported less in 2011 with respect to 2010). Interestingly, that gap jumps discontinuously at the cut-off point (Figure 8). Table 8 presents the regression results, which appear to change to some extent across specifications. Nonetheless, there seems to be evidence of a negative effect on the outcome variable. The coefficients for the LATE estimated in preferred regressions are between -$1,041 and -$476. One of them is not significant at the 10 percent level (0.5 std. dev. bandwidth) and the other two are significant at the 5 percent and 10 percent levels. These estimates are close to those obtained from regressions without additional covariates.

If taxpayers believe that the probability of getting a notification is an increasing function of the under-reported amount, then these findings suggest that some taxpayers strategically attempt to evade taxes while trying to avoid being notified.

As pointed out by Phillips (2011), it is reasonable to think that taxpayers do not face a constant likelihood of non-compliance detection. Instead, that probability most likely depends on how large non-compliance is. This arises from targeted compliance enforcement methods that focus on taxpayers who are most likely to be non-compliers and to those who have the greater expected non-compliance amount.

Especially interesting is the cut-off rule studied by Alm et al. (1993a) using a laboratory experiment. Under this rule, the tax authority announces that any taxpayer who reports less than the cut-off level will be audited with certainty.

As explained before, the enforcement method studied in this paper follows a cut-off rule; however, taxpayers do not know the cut-off. Hence, is it reasonable to believe that taxpayers who received the tax notification in 2010 think that they were treated because they under-reported “too much”. Following that logic, those taxpayers who wanted to under-report in 2011, and received the tax notification in 2010, would reduce the under-reported amount significantly, expecting to fall below the selection threshold and consequently avoid being notified in 2011. In other words, taxpayers perceive enforcement to be a function of compliance behavior. The results in panel C of Table 8 suggest that kind of behavior.

In summary, the findings in this subsection suggest that the treatment changed taxpayers’ behavior one year following the intervention. Treated taxpayers were more likely to over-report and less likely to under-report. Also, treated taxpayers who under-reported in 2011, under-reported an amount significantly lower than in 2010, presumably to avoid being notified.

Collectively, these results suggest that some taxpayers perceive enforcement to be endogenously determined as a function of compliance, and act accordingly to reduce the tax burden and/or the probability of being targeted for enforcement. That behavior is consistent with theories that explain tax evasion with economics-of-crime type of models, first applied to tax compliance by Allingham and Sandmo (1972).

6 Conclusions

This paper estimates the impact of tax notifications on compliance and tax revenues in Ecuador. I overcome confounding factors by using a regression discontinuity design that takes advantage of a discrete increase in the probability of receiving a non-compliance notification. The results indicate that the intervention causes the probability of compliance to increase by around 67 percentage points. Also, the treatment causes taxes reported to increase by approximately $1,400, which implies a net benefit of at least $1,300 (a return of 1,500 percent) for the marginal tax notification.

I also find suggestive evidence that the effect of the intervention persists. Around the cut-off, treated taxpayers reported more taxes in the year following the intervention (2011) than the non-treated group.

In addition, those receiving the treatment were less likely to under-report, which can be interpreted as the deterrence effect of the intervention. Moreover, the tax notification caused an increase in the probability to over-report, which can be explained if some taxpayers pay more taxes than what is due in an attempt to reduce the probability of being notified or audited.

Additional results show that on average, taxpayers under-reported less in 2011 than in 2010, and that the gap was greater for treated taxpayers around the cut-off. If taxpayers believe that the probability of receiving a notification is an increasing function of the under-reported amount, then these findings imply that some taxpayers strategically attempt to evade taxes while trying to avoid being notified.

These results suggest that some taxpayers believe that enforcement is a function of compliance, and act strategically to reduce the tax burden and/or the probability of being targeted for enforcement. That behavior is consistent with theories that explain tax evasion with economics-of-crime type of models.

These findings indicate that inexpensive tax compliance interventions can be used effectively by tax authorities in low-income countries. This is important since tax evasion is a particularly large problem in these countries, and since they likely have less means and capability to pursue other, costlier, compliance strategies. Thus, while it is difficult to know the extent to which the results found in his paper extend to countries with different tax systems, the results suggest that there may well be scope for low-income countries to reduce the inefficiencies and inequities caused by tax evasion by utilizing low-cost compliance strategies such as the one studied in this paper.

References

Allingham, M. G. and A. Sandmo (1972) "Income tax evasion: a theoretical analysis," *Journal of Public Economics*, Vol. 1, No. 34, pp. 323 – 338.

Alm, J., T. Cherry, M. Jones, and M. McKee (2010) "Taxpayer information assistance services and tax compliance behavior," *Journal of Economic Psychology*, Vol. 31, No. 4, pp. 577–586.

Alm, J., M. B. Cronshaw, and M. McKee (1993a) "Tax compliance with endogenous audit selection rules," *Kyklos*, Vol. 46, No. 1, pp. 27–45.

Alm, J., B. Jackson, and M. McKee (1992) "Institutional uncertainty and taxpayer compliance," *The American Economic Review*, Vol. 82, No. 4, pp. 1018–1026.

——— (1993b) "Fiscal exchange, collective decision institutions, and tax compliance," *Journal of Economic Behavior & Organization*, Vol. 22, No. 3, pp. 285–303.

——— (2009) "Getting the word out: Enforcement information dissemination and compliance behavior," *Journal of Public Economics*, Vol. 93, No. 3, pp. 392–402.

Alm, J., G. H. McClelland, and W. D. Schulze (1999) "Changing the social norm of tax compliance by voting," *Kyklos*, Vol. 52, No. 2, pp. 141–171.

Andreoni, J., B. Erard, and J. Feinstein (1998) "Tax compliance," *Journal of Economic Literature*, Vol. 36, No. 2, pp. 818–860.

Bazart, C. and A. Bonein (2013) "Reciprocal relationships in tax compliance decisions," *Journal of Economic Psychology*, Forthcoming.

Blumenthal, M., C. Christian, and J. Slemrod (2001) “Do normative appeals affect tax compliance? Evidence from a controlled experiment in Minnesota,” *National Tax Journal*, Vol. 54, No. 1, pp. 125–138.

Bosco, L. and L. Mittone (1997) “Tax evasion and moral constraints: some experimental evidence,” *Kyklos*, Vol. 50, No. 3, pp. 297–324.

Calvet, R. and J. Alm (2012) “Empathy, sympathy, and tax compliance,” *Journal of Economic Psychology*, Forthcoming.

Carrillo, P., M. S. Emran, and A. Rivadeneira (2012) “Do cheaters bunch together? Profit taxes, withholding rates and tax evasion,” Working Paper, George Washington University.

Coricelli, G., E. Rusconi, and M. C. Villeval (2013) “Tax evasion and emotions: An empirical test of re-integrative shaming theory,” *Journal of Economic Psychology*, Forthcoming.

Djawadi, B. M. and R. Fahr (2013) “The impact of tax knowledge and budget spending influence on tax compliance,” *IZA Discussion Paper*, Vol. 7255.

Dubin, J. A. (2007) “Criminal investigation enforcement activities and taxpayer noncompliance,” *Public Finance Review*, Vol. 35, No. 4, pp. 500–529.

Dubin, J. A., M. J. Graetz, and L. L. Wilde (1990) “The effect of audit rates on the federal individual income tax, 1977-1986,” *National Tax Journal*, Vol. 43, No. 4, pp. 395–409.

Dubin, J. A. and L. L. Wilde (1988) “An empirical analysis of federal income tax auditing and compliance,” *National Tax Journal*, Vol. 41, No. 1, pp. 61–74.

Fellner, G., R. Sausgruber, and C. Traxler (2013) “Testing enforcement strategies in the field: Legal threat, moral appeal, and social information,” *Journal of the European Economic Association*, Forthcoming.

Fisman, R. and S. J. Wei (2004) “Tax rates and tax evasion: Evidence from missing imports in China,” *Journal of Political Economy*, Vol. 112, No. 2, pp. 471–496.

Friedland, N., S. Maital, and A. Rutenberg (1978) “A simulation study of income tax evasion,” *Journal of Public Economics*, Vol. 10, No. 1, pp. 107–116.

Gemmell, N. and M. Ratto (2012) "Behavioral responses to taxpayer audits: evidence from random taxpayer inquiries," *National Tax Journal*, Vol. 65, No. 1, pp. 33–58.

González, D. (2009) *La política tributaria heterodoxa en los países de América Latina*: Cepal.

Guala, F. and L. Mittone (2005) "Experiments in economics: External validity and the robustness of phenomena," *Journal of Economic Methodology*, Vol. 12, No. 4, pp. 495–515.

Hahn, J., P. Todd, and W. Van der Klaauw (2001) "Identification and estimation of treatment effects with a regression-discontinuity design," *Econometrica*, Vol. 69, No. 1, pp. 201–209.

Hasseldine, J., P. Hite, S. James, and M. Toumi (2007) "Persuasive Communications: Tax Compliance Enforcement Strategies for Sole Proprietors," *Contemporary Accounting Research*, Vol. 24, No. 1, pp. 171–194.

Internal Revenue Service (1996) "Federal tax compliance research: Individual income tax gap estimates for 1985, 1988, and 1992," Publication 1415 (Rev. 4-96), Washington, DC.

——— (2007) "Reducing the federal tax gap: A report on improving voluntary compliance."

Kirchler, E. (2007) *The economic psychology of tax behaviour*: Cambridge University Press.

Kleven, H. J., M. B. Knudsen, C. T. Kreiner, S. Pedersen, and E. Saez (2011) "Unwilling or unable to cheat? Evidence from a tax audit experiment in Denmark," *Econometrica*, Vol. 79, No. 3, pp. 651–692.

Kleven, H. J. and M. Waseem (2012) "Behavioral responses to notches: Evidence from pakistani tax records," Working Paper Series, London School of Economics.

Kumler, T. J., E. A. Verhoogen, and J. Frias (2012) "Enlisting workers in monitoring firms: Payroll tax compliance in Mexico," Columbia University Department of Economics Discussion Papers, 96.

Lee, D. S. (2009) "Training, wages, and sample selection: Estimating sharp bounds on treatment effects," *The Review of Economic Studies*, Vol. 76, No. 3, pp. 1071–1102.

Lee, D. S. and T. Lemieux (2010) "Regression discontinuity designs in Economics," *Journal of Economic Literature*, Vol. 48, No. 2, pp. 281–355.

Lindo, J. M., N. J. Sanders, and P. Oreopoulos (2010) "Ability, Gender, and Performance Standards: Evidence from Academic Probation," *American Economic Journal: Applied Economics*, Vol. 2, No. 2, pp. 95–117.

McCrary, J. (2008) "Manipulation of the running variable in the regression discontinuity design: A density test," *Journal of Econometrics*, Vol. 142, No. 2, pp. 698–714.

Mittone, L. (2006) "Dynamic behaviour in tax evasion: An experimental approach," *The Journal of Socio-Economics*, Vol. 35, No. 5, pp. 813–835.

Olken, B. A. and M. Singhal (2011) "Informal taxation," *American Economic Journal: Applied Economics*, Vol. 3, No. 4, pp. 1–28.

Phillips, M. D. (2011) "Reconsidering the deterrence paradigm of tax compliance," in *2011 IRS-TPC Research Conference: New Perspectives on Tax Administration*, pp. 99–106, Internal Revenue Service.

Plumley, A. H. (1996) "The determinants of individual income tax compliance: estimating the tmpacts of Tax policy, enforcement, and IRS responsiveness," Internal Revenue Service, Publication 1916 (Rev. 11-96), Washington, DC.

Pomeranz, D. (2013) "No Taxation without Information: Deterrence and Self-Enforcement in the Value Added Tax," Working Paper 19199, National Bureau of Economic Research.

Schneider, F., A. Buehn, and C. E. Montenegro (2010) "Shadow economies all over the world: New estimates for 162 Countries from 1999 to 2007," The World Bank: Policy Research Working Paper, 5356.

Slemrod, J. (1992) *Why people pay taxes: Tax compliance and enforcement*: University of Michigan Press Ann Arbor, MI.

——— (2007) "Cheating ourselves: the economics of tax evasion," *The journal of economic perspectives*, Vol. 21, No. 1, pp. 25–48.

Slemrod, J., M. Blumenthal, and C. Christian (2001) "Taxpayer response to an increased probability of audit: Evidence from a controlled experiment in Minnesota," *Journal of Public Economics*, Vol. 79, No. 3, pp. 455–483.

Tan, F. and A. Yim (2013) "Can strategic uncertainty help deter tax evasion? An experiment on auditing rules," *Journal of Economic Psychology*, Forthcoming.

Torgler, B. (2002) "Speaking to theorists and searching for facts: Tax morale and tax compliance in experiments," *Journal of Economic Surveys*, Vol. 16, No. 5, pp. 657–683.

——— (2004) "Moral suasion: An alternative tax policy strategy? Evidence from a controlled field experiment in Switzerland," *Economics of Governance*, Vol. 5, No. 3, pp. 235–253.

——— (2007) *Tax compliance and tax morale: a theoretical and empirical analysis*: Edward Elgar Publishing.

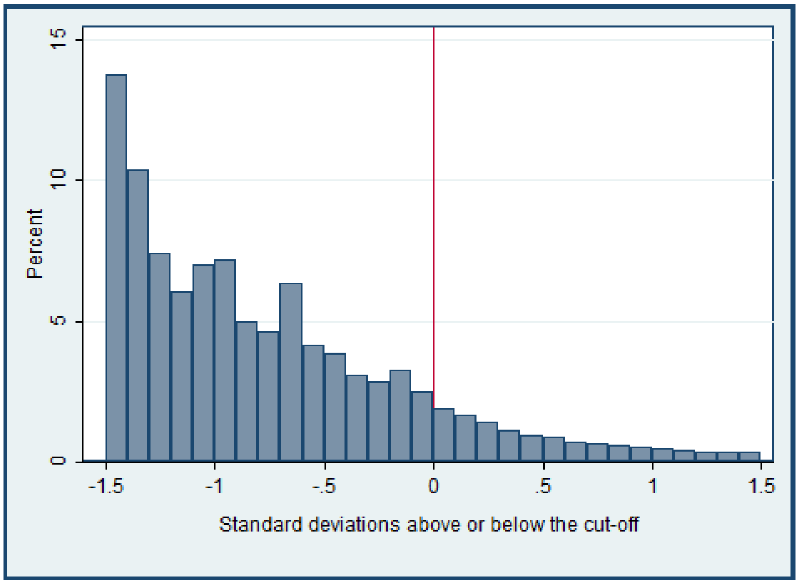


Figure 1: Histogram of the running variable

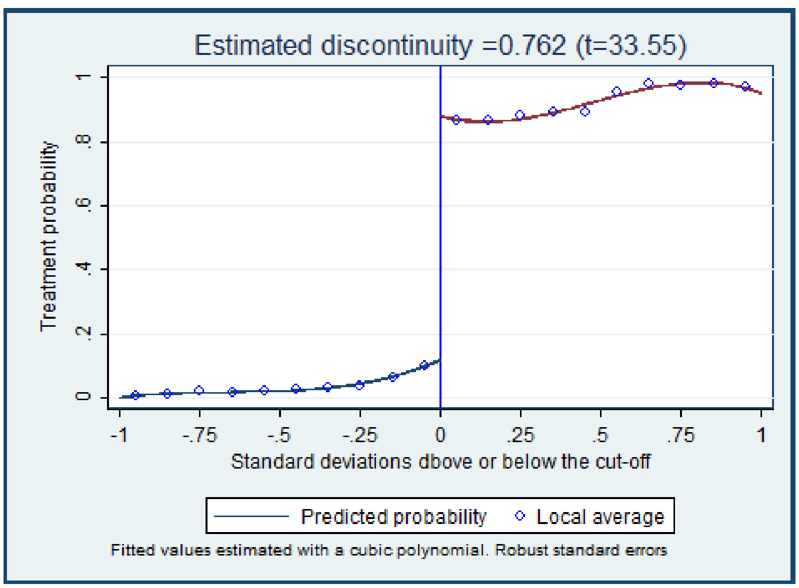


Figure 2: Discontinuity in the probability of receiving the tax notification (First Stage)

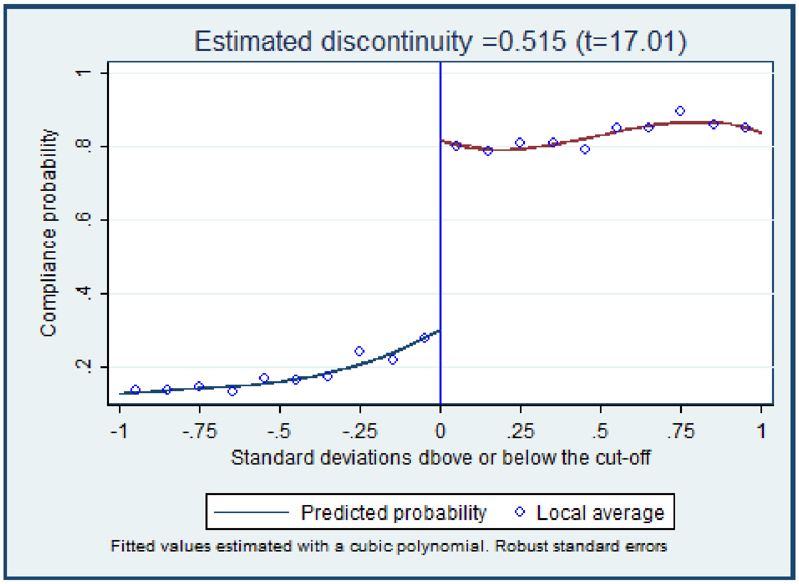


Figure 3: Discontinuity in the probability of compliance

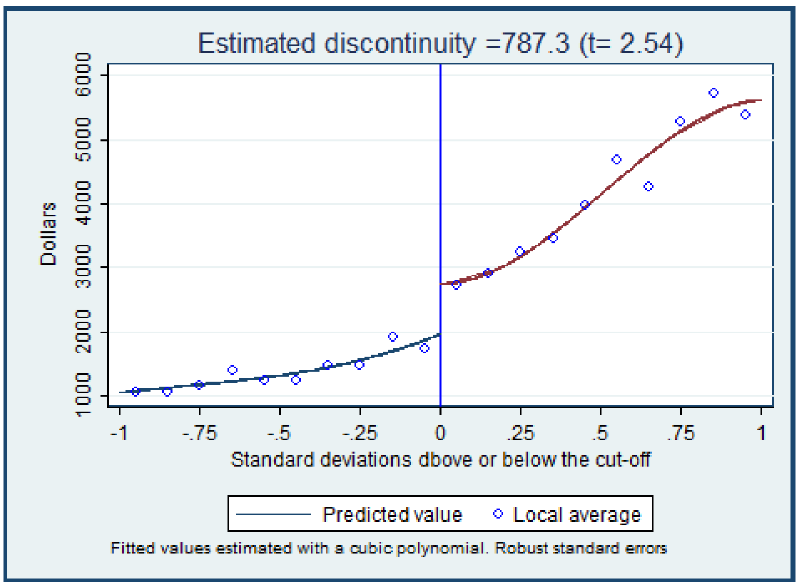


Figure 4: Discontinuity in reported taxes (Post-treatment ITA 2010)

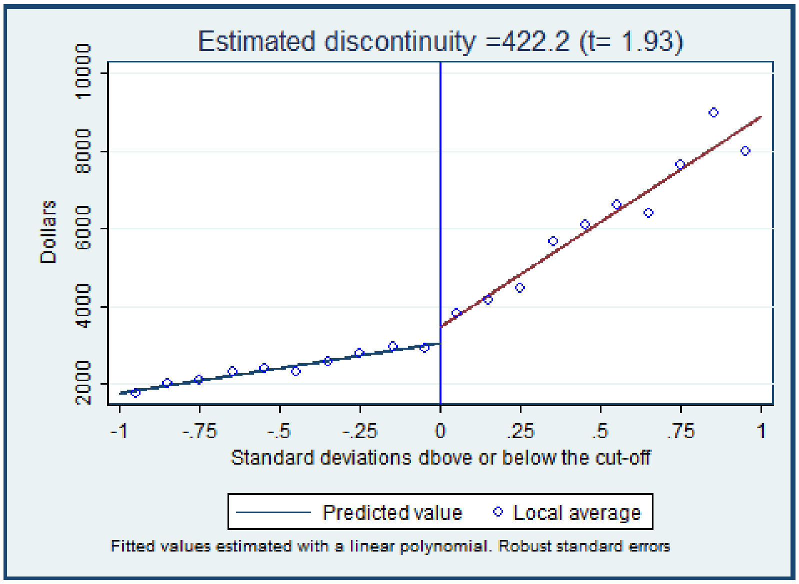


Figure 5: Persistence effect (Discontinuity in reported taxes - ITA 2011)

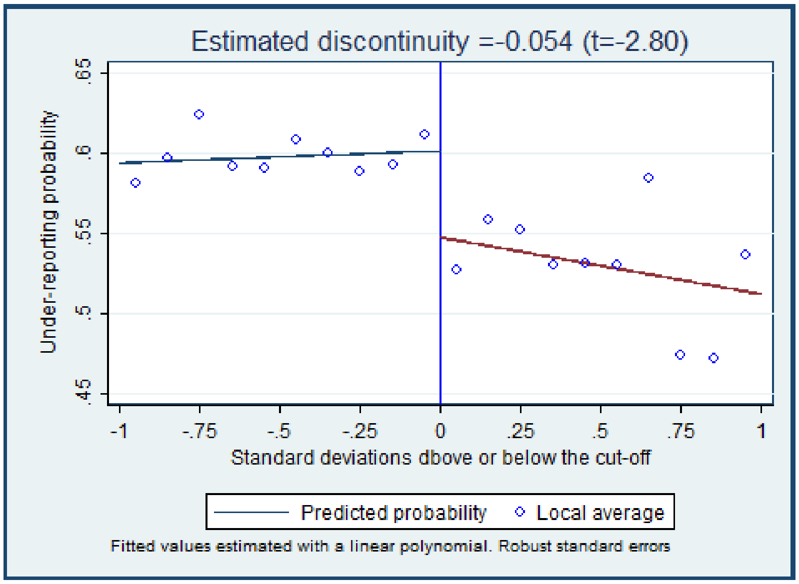


Figure 6: Persistence effect (Discontinuity in the probability of under-reporting taxes - ITA 2011)

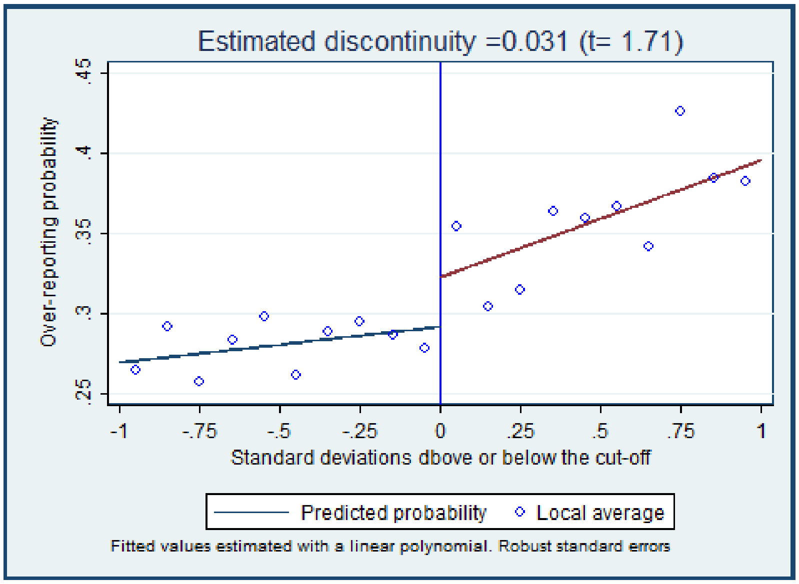


Figure 7: Persistence effect (Discontinuity in the probability of over-reporting taxes - ITA 2011)

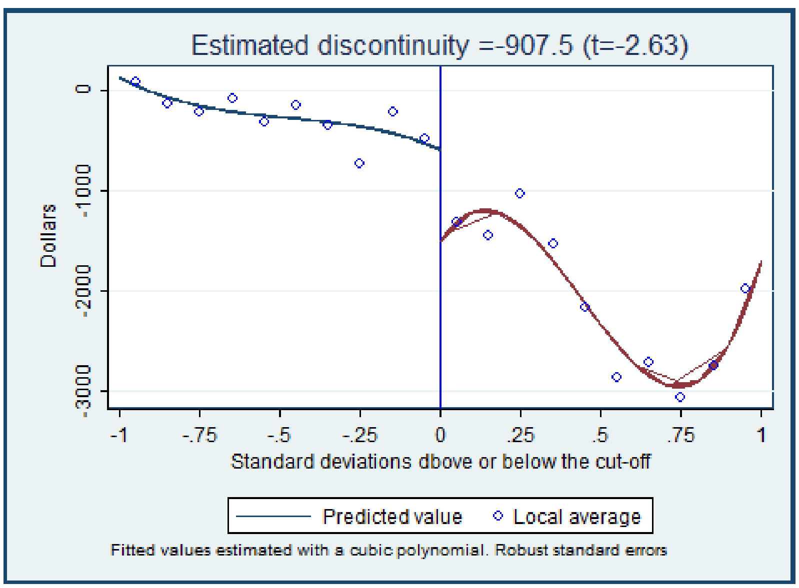


Figure 8: Persistence effect (Discontinuity in the difference between under-reported ITA 2011 and 2010

Table 1: Frequencies by Province

Province	Under-reported ITA 2010		Selected to receive the tax notification	Received the tax notification	Corrected or justified differences
	(Count)	(Percent)	(Count)	(Count)	(Count)
Azuay	2,225	6.10	448	436	651
Bolívar	123	0.34	60	60	76
Carchi	380	1.04	110	109	87
Cañar	322	0.88	160	92	91
Chimborazo	697	1.91	279	277	364
Cotopaxi	509	1.40	41	41	69
El Oro	1,586	4.35	295	291	366
Esmeraldas	550	1.51	133	130	121
Galápagos	145	0.40	12	12	19
Guayas	12,385	33.97	928	879	2,182
Imbabura	865	2.37	121	121	284
Loja	724	1.99	390	376	403
Los Ríos	630	1.73	35	34	55
Manabí	1,927	5.29	269	267	356
Morona S.	169	0.46	79	46	49
Napo	135	0.37	71	71	86
Orellana	262	0.72	87	87	129
Pastaza	112	0.31	41	41	43
Pichincha	10,224	28.04	996	988	2,239
Sta. Elena	285	0.78	42	41	56
Sto. Domingo	715	1.96	106	106	140
Sucumbíos	256	0.70	149	144	151
Tungurahua	1,121	3.07	129	129	183
Zamora Ch.	110	0.30	47	44	46
Total	36,457	100	5,028	4,822	8,246

Source: Author calculations and SRI.

Table 2: Summary Statistics

	Mean	Standard deviation
Panel A. Outcome variables		
Corrected or justifed difference (binary)	0.23	0.42
Post-treatment reported ITA 2010 (US$)	1,803.52	5,131.16
Reported ITA 2011 (US$)	2,923.13	6,907.22
Under-reporting ITA 2011 (binary)	0.60	0.49
Over-reporting ITA 2011 (binary)	0.28	0.45
Difference under-reported ITA (2011 minus 2010) (US$) [a]	-494.80	3,509.21
Panel B. Covariates		
Assets (US$ in thousands)	197.51	472.43
Taxable income (US$ in thousands)	422.45	891.66
Deductible costs and expenses (US$ in thousands)	407.50	867.84
Net worth (US$ in thousands)	95.45	305.17
Age of legal representative (years)	48.12	12.41
Female legal representative (binary)	0.28	0.45
Legal representative has college education (binary)	0.45	0.50
Special taxpayer (binary) [b]	0.05	0.22
Corporation (binary)	0.57	0.49
Years of operation (years)	12.78	8.94

Notes: All the covariates correspond to the pre-treatment period.
[a] Conditional on under-reporting in 2011.
[b] Special taxpayers are those required to withhold taxes from other taxpayers.
Source: Author calculations and SRI.

Table 3: Regression discontinuity estimates of the discontinuity in the probability of receiving the tax notification (First Stage)

	Order of polynomial	Discontinuity (1)	Discontinuity (2)
Panel A. Bandwidth: 1 std. dev.			
	Cubic [a]	0.762***	0.750***
		(0.023)	(0.019)
	Quadratic	0.755***	0.762***
		(0.017)	(0.014)
	Linear	0.779***	0.804***
		(0.011)	(0.009)
Controls		No	Yes
Observations		14,340	14,260
Panel B. Bandwidth: 0.5 std. dev.			
	Cubic	0.752***	0.761***
		(0.032)	(0.026)
	Quadratic [a]	0.746***	0.732***
		(0.025)	(0.020)
	Linear	0.766***	0.773***
		(0.016)	(0.013)
Controls		No	Yes
Observations		6,084	6,052
Panel C. Bandwidth: 0.25 std. dev.			
	Quadratic [a]	0.771***	0.786***
		(0.034)	(0.028)
	Linear	0.738***	0.745***
		(0.023)	(0.019)
Controls		No	Yes
Observations		3,120	3,105

Notes: The bandwidths are standard deviations above or below the cut-off. Robust Standard Errors in parentheses; * p<0.1; ** p<0.05; *** p<0.01.
[a] Preferred order polynomial for each bandwith selected using the Akaike Information Criterion (AIC) calculated for regressions that include additional covariates.
Source: Author calculations and SRI.

Table 4: Regression discontinuity estimates of the effect of the tax notification in the probability of compliance (ITA 2010)

Treatment effect	Intent-to treat effect		Effect of the notification (LATE)	
	(1)	(2)	(3)	(4)
Panel A. Bandwidth: 1 std. dev.				
Cubic polynomial [a]	0.515***	0.505***	0.675***	0.673***
	(0.030)	(0.028)	(0.032)	(0.032)
Quadratic polynomial	0.505***	0.520***	0.669***	0.683***
	(0.023)	(0.021)	(0.024)	(0.024)
Linear polynomial	0.541***	0.563***	0.694***	0.700***
	(0.015)	(0.014)	(0.016)	(0.015)
Controls	No	Yes	No	Yes
Observations	14,340	14,260	14,340	14,260
Panel B. Bandwidth: 0.5 std. dev.				
Cubic polynomial	0.509***	0.521***	0.676***	0.685***
	(0.043)	(0.039)	(0.046)	(0.046)
Quadratic polynomial [a]	0.494***	0.483***	0.663***	0.660***
	(0.033)	(0.030)	(0.035)	(0.036)
Linear polynomial	0.520***	0.533***	0.678***	0.689***
	(0.022)	(0.019)	(0.023)	(0.022)
Controls	No	Yes	No	Yes
Observations	6,084	6,052	6,084	6,052
Panel C. Bandwidth: 0.25 std. dev.				
Quadratic polynomial	0.485***	0.511***	0.629***	0.650***
	(0.046)	(0.042)	(0.049)	(0.047)
Linear polynomial [a]	0.496***	0.510***	0.672***	0.685***
	(0.031)	(0.028)	(0.034)	(0.033)
Controls	No	Yes	No	Yes
Observations	3,120	3,105	3,120	3,105

Notes: The bandwidths are standard deviations above or below the cut-off. Robust Standard Errors in parentheses; * p<0.1; ** p<0.05; *** p<0.01. The additional covariates are defined in the text. LATE is estimated using Two-stage Least Squares.
[a] Preferred order polynomial for each bandwith selected using the Akaike Information Criterion (AIC) calculated for the reduced form regressions that include additional covariates.
Source: Author calculations and SRI.

Table 5: Regression discontinuity estimates of the effect of the tax notification in reported taxes (ITA 2010 (U.S. Dollars))

Treatment effect	Intent-to treat effect		Effect of the notification (LATE)	
	(1)	(2)	(3)	(4)
Panel A. Bandwidth: 1 std. dev.				
Cubic polynomial [a]	787.28**	1,013.75***	1,036.75**	1,356.11***
	(309.88)	(200.05)	(405.46)	(269.28)
Quadratic polynomial	637.57***	696.00***	845.51***	913.85***
	(239.10)	(158.46)	(315.19)	(208.34)
Linear polynomial	698.38***	835.01***	896.28***	1,038.20***
	(169.47)	(119.48)	(216.05)	(148.69)
Controls	No	Yes	No	Yes
Observations	14,061	13,982	14,061	13,982
Panel B. Bandwidth: 0.5 std. dev.				
Cubic polynomial [a]	1,173.22***	1,406.41***	1,563.56***	1,859.55***
	(425.81)	(260.25)	(564.00)	(350.20)
Quadratic polynomial	820.33**	923.60***	1,105.33**	1,267.52***
	(321.51)	(201.31)	(430.04)	(277.75)
Linear polynomial	623.39***	741.50***	814.14***	959.79***
	(220.08)	(142.21)	(285.39)	(184.16)
Controls	No	Yes	No	Yes
Observations	5,995	5,963	5,995	5,963
Panel C. Bandwidth: 0.25 std. dev.				
Quadratic polynomial	1,343.79***	1,367.99***	1,749.16***	1,753.07***
	(450.28)	(278.06)	(582.91)	(361.80)
Linear polynomial [a]	837.21***	1,072.55***	1,138.16***	1,443.01***
	(292.71)	(189.44)	(394.89)	(256.12)
Controls	No	Yes	No	Yes
Observations	3,074	3,059	3,074	3,059

Notes: The bandwidths are standard deviations above or below the cut-off. Robust Standard Errors in parentheses; * p<0.1; ** p<0.05; *** p<0.01. The additional covariates are defined in the text. LATE is estimated using Two-stage Least Squares.
[a] Preferred order polynomial for each bandwith selected using the Akaike Information Criterion (AIC) calculated for the reduced form regressions that include additional covariates.
Source: Author calculations and SRI.

Table 6: Regression discontinuity estimates of the persistence of the effect of the tax notification in reported taxes (ITA 2011 (U.S. Dollars))

Treatment effect	Intent-to treat effect		Effect of the notification (LATE)	
	(1)	(2)	(3)	(4)
Panel A. Bandwidth: 1 std. dev.				
Cubic polynomial	477.61	653.87***	626.47	866.29***
	(392.55)	(251.29)	(512.98)	(333.76)
Quadratic polynomial	439.10	399.02**	580.91	520.77**
	(307.50)	(197.11)	(405.34)	(257.34)
Linear polynomial [a]	422.17*	539.15***	541.83*	668.39***
	(218.39)	(143.85)	(279.28)	(178.43)
Controls	No	Yes	No	Yes
Observations	13,697	13,622	13,697	13,622
Panel B. Bandwidth: 0.5 std. dev.				
Cubic polynomial	638.25	665.25**	847.87	870.84**
	(535.26)	(320.62)	(708.15)	(421.44)
Quadratic polynomial	679.62*	607.99**	913.60*	825.86**
	(406.26)	(252.19)	(543.26)	(343.40)
Linear polynomial [a]	310.51	394.30**	404.60	506.56**
	(283.07)	(175.58)	(367.76)	(225.66)
Controls	No	Yes	No	Yes
Observations	5,854	5,824	5,854	5,824
Panel C. Bandwidth: 0.25 std. dev.				
Quadratic polynomial	771.02	464.28	995.74	587.39
	(569.90)	(333.88)	(732.90)	(423.41)
Linear polynomial [a]	456.60	575.37**	619.88	767.99**
	(375.46)	(235.39)	(507.75)	(314.69)
Controls	No	Yes	No	Yes
Observations	2,997	2,983	2,997	2,983

Notes: The bandwidths are standard deviations above or below the cut-off. Robust Standard Errors in parentheses; * p<0.1; ** p<0.05; *** p<0.01. The additional covariates are defined in the text. LATE is estimated using Two-stage Least Squares.
[a] Preferred order polynomial for each bandwith selected using the Akaike Information Criterion (AIC) calculated for the reduced form regressions that include additional covariates.
Source: Author calculations and SRI.

Table 7: Regression discontinuity estimates of the effect (LATE) of the tax notification on the probability of attrition and on reported taxes with bounds analysis

Bandwidth (standard deviations)	1	0.5	0.25
Panel A. Dependent variable:			
Reported post-treatment ITA 2010 - US$			
Probability of attrition	0.012	0.012	0.003
	(0.013)	(0.018)	(0.013)
RDD estimate	1,356.11***	1,859.55***	1,443.01***
	(269.28)	(350.20)	(256.12)
Lower bound	1,339.78	1,822.29	1,428.21
	(273.90)	(344.22)	(264.72)
Upper bound	1,493.59	2,022.95	1,511.14
	(326.31)	(374.14)	(272.11)
Order of polynomial	Cubic	Cubic	Linear
Observations	14,061	5,963	3,059
Panel B. Dependent Variable: Reported ITA 2011 - US$			
Probability of attrition	0.001	0.002	-0.007
	(0.009)	(0.013)	(0.019)
RDD estimate	668.39***	506.56**	767.99**
	(178.43)	(225.66)	(314.69)
Lower bound	660.00	492.54	769.10
	(181.25)	(229.58)	(323.73)
Upper bound	709.77	604.59	867.80
	(204.32)	(242.83)	(315.87)
Order of polynomial	Linear	Linear	Linear
Observations	13,622	5,824	2,983

Notes: The bandwidths are standard deviations above or below the cut-off. Robust Standard Errors in parentheses; * p<0.1; ** p<0.05; *** p<0.01. The order polynomial for each bandwidth is the preferred one selected using the Akaike Information Criterion (AIC) calculated for the reduced form regressions that include additional covariates. All regressions include the additional covariates defined in the text. LATE is estimated using Two-stage Least Squares. Bounds standard errors based on 500 bootstrapped samples.
Source: Author calculations and SRI.

Table 8: Regression discontinuity estimates of the persistence of the effect (LATE) of the tax notification

Bandwidth (standard deviations)	1	0.5	0.25
Panel A. Dependent variable: under-reporting ITA 2011 (binary)			
RDD estimate	-0.077***	-0.089***	-0.116**
	(0.024)	(0.034)	(0.050)
Order of Polynomial	Linear	Linear	Linear
Observations	13,622	5,824	2,983
Panel B. Dependent variable: over-reporting ITA 2011 (binary)			
RDD estimate	0.047**	0.049	0.101**
	(0.023)	(0.032)	(0.047)
Order of Polynomial	Linear	Linear	Linear
Observations	13,622	5,824	2,983
Panel C. Dependent variable: difference Under-reported ITA (2011 minus 2010) (US$) [a]			
RDD estimate	-1,041.35**	-475.70	-790.21*
	(443.35)	(292.94)	(415.67)
Order of Polynomial	Cubic	Linear	Linear
Observations	7,962	3,386	1,739

Notes: The bandwidths are standard deviations above or below the cut-off . Robust Standard Errors in parentheses;* p<0.1; ** p<0.05; *** p<0.01. The order polynomial for each bandwidth is the preferred one selected using the Akaike Information Criterion (AIC) calculated for the reduced form regressions that include additional covariates. All regressions include the additional covariates defined in the text. LATE is estimated using Two-stage Least Squares.

[a] Conditional on under-reporting in 2011.

Source: Author calculations and SRI.

A Examples of application of the Income tax advance

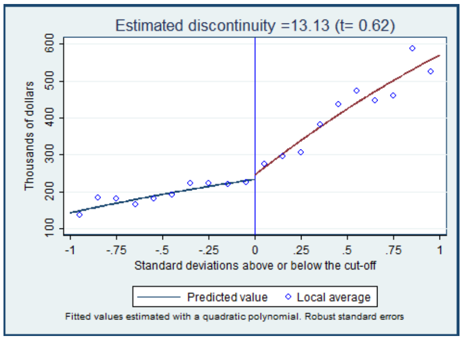
The table below shows three examples of the Income Tax (IT) reports in Ecuador for the fiscal year 2010. As discussed in the text, if the ITA is greater than the Incurred IT, the former becomes the relevant IT that the taxpayer has to file.

Table 9: Examples of application of the Income tax advance

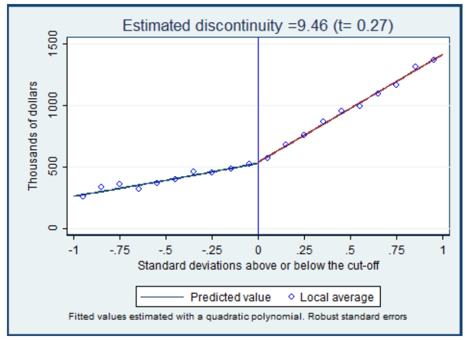
Examples	**Case 1**	**Case 2**	**Case 3**
(A) ITA (2010)	100	100	100
(B) IT Withheld (2009)	70	70	70
(C) Anticipated Payment (A-B)	30	30	30
(D) Incurred IT (2010)	120	80	80
(E) IT Withheld (2010)	50	50	85
ITA filed in 2010			
(F) Greater btw A and D	120	100	100
(G) Taxes Due (F-C-E)	40	20	-15

Source: Author calculations and SRI.

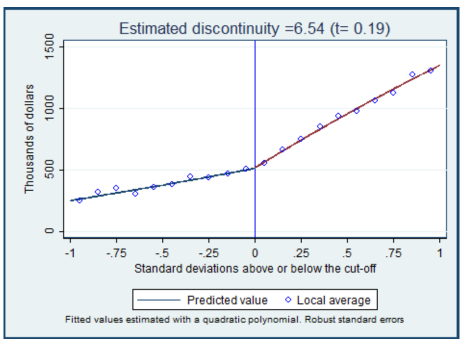
B Identifying assumption validity: inspecting covariates



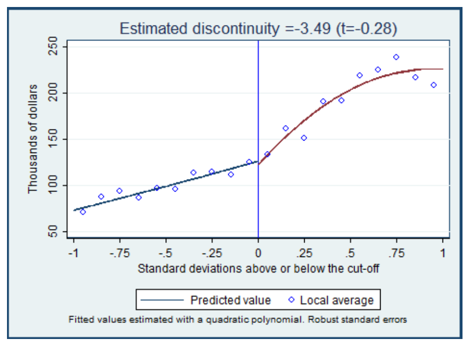
(a) Total assets



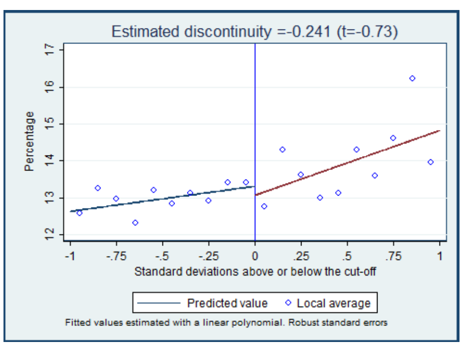
(b) Taxable income



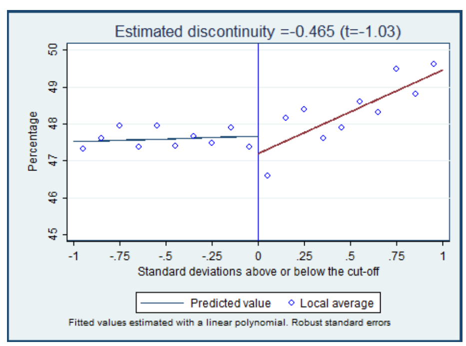
(c) Deductable costs and expenses



(d) Net worth

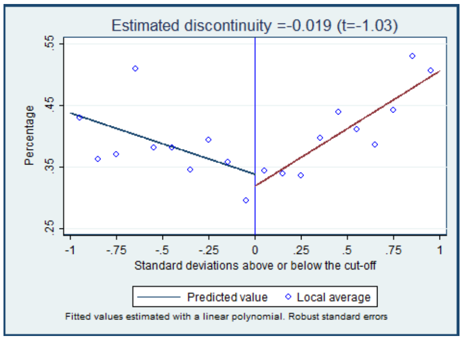


(e) Years of operation

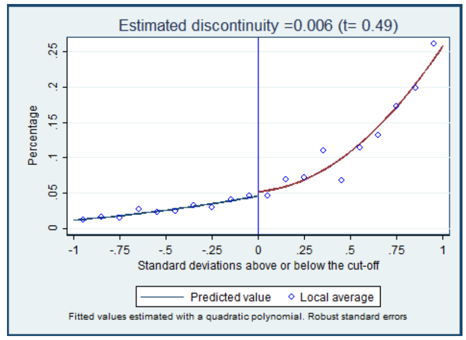


(f) Age of legal representative

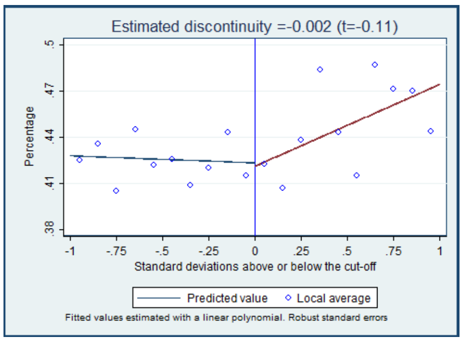
Figure 9: Discontinuities of covariates at the selection threshold



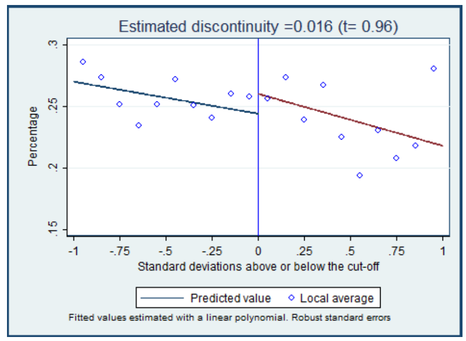
(a) Corporation (binary)



(b) Special taxpayer (binary)[a]



(c) Legal representative college education (binary)



(d) Female legal representative (binary)

Figure 10: Discontinuities of covariates at the selection threshold (continued...)

Note: [a] Special taxpayers are required to withhold taxes from other taxpayers.