

THREE ESSAYS ON IMPACT EVALUATION OF PUBLIC POLICIES

A Dissertation

by

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ABSTRACT

This dissertation analyzes the impact of three public policies. Each essay attempts to identify the effects of a specific public policy using different methods. The first studies the effect of low-cost intervention on tax compliance. 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. These findings indicate that inexpensive tax compliance interventions can be used effectively by tax authorities in low-income countries.

The second essay studies data collected in a field experiment that provides information to households to promote conservation of electricity. Households received one of three different information interventions: (1) make a price notch salient, (2) make a social comparison, or (3) do both. Results corresponding to households with historical consumption above the notch indicate that the social comparison information reduces consumption by around 1%, and that the price salience information effect is not statistically significant. These findings imply that the social comparison treatment was more effective in promoting conservation. However, there is also suggestive evidence that the effect of the price salience treatment exists for households who were just above the notch, whereas the effect of the social comparison is significant for both households who were just above and well above the notch. The results suggest that similar interventions could be used in longer term projects to promote conservation and reduce the fiscal burden of electricity subsidies.

The last essay examines the effect of the Arizona Immigration Law of 2010 (SB 1070) on the noncitizen Hispanic state population. Results indicate that this bill

produced a significant reduction in the proportion of Hispanic noncitizens living in Arizona estimated to be between 10% and 16%. However, this effect lasted less than one year, as the evidence suggests that it vanishes after a few months. The findings imply that the response of the undocumented population facing higher risk of deportation is to quickly move out. The findings also suggest that when that risk diminishes, the undocumented population tends to increase.

DEDICATION

To my mother

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1. INTRODUCTION

Measuring the impact of public policies is very important for policymakers and academics. For the former group, an accurate policy evaluation helps to take better decisions and to reassign resources. For the latter group, studying the impact of public policies increases the understanding of economic systems. In the recent years, empirical economics has experienced what Angrist and Pischke (2010) call a “credibility revolution”. This revolution has impacted the research methods used to evaluate policies, emphasizing the importance of the correct identification of treatment effects. In this context, the main objective of this dissertation is to identify the effects of three public policies using state-of-the-art methods.

The first essay studies tax evasion, which is a significant problem facing countries around the world that imposes efficiency costs and creates inequality. 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, Buehn and Montenegro, 2010).

The main objective of the first essays 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. To overcome this type of selection bias I use a regression discontinuity design (RDD) that takes advantage of a discrete increase in the probability of receiving a formal notification (treatment). In particular, 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.

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. I also find suggestive evidence that the effect persists for the following year.

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 second essay explores the effects of information intervention as incentives to promote conservation of energy, which has become a significant matter in public policy in recent years. Traditionally, economic or price measures have been considered the first-best policies to induce conservation. However, there is mixed evidence that these non-linear incentives alter behavior in the energy sector. In contrast, recent research has shown that non-pecuniary strategies can be used to influence behavior in a variety of areas. In the energy domain, the use of social comparisons is increasingly seen as a powerful policy instrument. The alternative policy instrument – prices – can face large political constraints. Policymakers and regulators are generally reluctant to set prices at the true marginal social cost for utilities such as electricity. In fact, in many developing countries energy prices are set far below the market price, and such government subsidy schemes contribute to government debt. In that context, an important question is whether information interventions can be used in developing countries to induce conservation of energy, and therefore reduce funds allocated to subsidies. Despite the growing research on the effects of both non-linear

incentives and social comparisons on conservation, little is known about the relative efficacy of these instruments. In that context, the main goal of the second essay is to identify the relative magnitudes of these two incentives in a developing country. To reach this goal, a large scale randomized controlled trial was implemented in collaboration with the Quito Electric Company (*Empresa Eléctrica Quito-EEQ*). In the field experiment, informational letters were attached to the electricity bills of randomly selected households in March 2014.

Typically it is very difficult to find utilities that have large changes in tariffs for small changes in consumption. However, households in Ecuador, and in Quito in particular, face a tariff with large changes in the total electricity bill for an additional kWh of monthly consumption (i.e. notches). These notches do not appear to induce a consumption reduction because there is no evidence of discontinuity of the distribution before the notches or bunching around them in historical consumption data. This might be evidence of the lack of salience of the complex electricity tariff in Quito.

In the randomized controlled trial the effects of three information interventions are explored. The first treatment makes the most important notch (the 111th kWh consumed increases the total electricity bill by around 40%) salient. The second makes a social comparison. Finally, the two pieces of information together are used as a third treatment to test for additive effects.

For the households with historical consumption above the notch, I find that the social comparison treatment reduces consumption by 1%, and it is statistically significant. On the other hand, the price salience treatment estimate is approximately one third of that of the social comparison, and it is not statistically significant. However, there is suggestive evidence that the effect of the price salience treatment exists only for households who were just above the notch, whereas the effect of the

social comparison is significant for both households who were just above and well above the notch. Importantly, none of the treatments have a significant effect for households historically consuming below the notch. Hence, there is no evidence of a “boomerang” effect created by the information intervention. These results contribute to the understanding of the capacity of information interventions to promote conservation of energy in the context of a developing country. The larger effect of the social comparison treatment suggests that similar interventions could be used in longer term projects to induce conservation and reduce the fiscal burden of electricity subsidies. The findings for the price notch salience treatment support previous literature finding that non-linear price incentives are not effective in reducing consumption of electricity. However, results also suggest that information that makes salient non-linear price schemes might be effective if targeted to the population with consumption levels just above price notches.

The last essays studies immigration legislation and its consequences, which are among the most important topics in the public policy debates in the U.S. In recent years a series of measures at the federal and state level have modified the immigration system. In that context, the main objective of the essays is to estimate the effect of state immigration legislation on the stock of undocumented population. To reach this goal, I study the Arizona Senate Bill 1070 enacted in 2010 (SB 1070), which is arguably one of the most strict state immigration bills passed in recent years.

I estimate the effect of this law on the composition of the population Arizona, specifically, in the proportion of noncitizen Hispanics living in that state. In order to get a consistent estimate of this effect, I use state-level aggregate data from the Current Population Survey (CPS) and the synthetic control method proposed in Abadie, Diamond and Hainmueller (2010) to calculate a counterfactual for Arizona.

In that sense this paper follows closely Bohn, Lofstrom and Raphael (2014), which

estimates the impact of the Arizona's 2007 Legal Arizona Workers Act (LAWA) using synthetic control methods, and finds that this law caused a significant reduction in the proportion of the Hispanic noncitizen population in Arizona.

One contribution of this paper is that it assess the flexibility of the illegal population to move as a response to changing conditions. In that sense, this paper joins the small, but growing literature, studying the response of undocumented individuals to unfavorable conditions (See for instance, Cadena and Kovak (2013), Watson (2013)).

Results indicate that the Arizona Immigration Law produced a statistically significant reduction in the proportion of noncitizen Hispanics in Arizona between 10% and 16%. However, this effect lasted less than one year, as the evidence suggests that it vanishes after a few months.

2. THE IMPACT OF LOW-COST INTERVENTION ON TAX COMPLIANCE: REGRESSION DISCONTINUITY EVIDENCE

2.1 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, Buehn and Montenegro, 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, Graetz and Wilde (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, Cronshaw

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

and McKee (1993) 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, Christian and Slemrod (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 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

²Other studies using laboratory experiments to analyze tax compliance include Alm, Jackson and McKee (1992, 1993); Alm, McClelland and Schulze (1999); Alm, Jackson and McKee (2009); Alm, Cherry, Jones and McKee (2010); Bazart and Bonein (2014); Bosco and Mittone (1997); Christian and Alm (2014); Djawadi and Fahr; Friedland, Maital and Rutenberg (1978); Guala and Mittone (2005); Mittone (2006); Torgler (2002); Tan and Yim (2014), among many others.

³Other field experiments on tax compliance include Slemrod, Blumenthal and Christian (2001), Hasseldine, Hite, James and Toumi (2007), Kleven et al. (2011), Gemmell and Ratto (2012), Fellner, Sausgruber and Traxler (2013), and Pomeranz (2013).

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, Pomeranz and Singhal (2014); Kumler, Verhoogen and Frias (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 reported taxes.

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.

2.2 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 minus taxes withheld in the previous period (i.e., anticipated payment).⁸ Table B.1 presents examples for various cases. Other Latin-American countries that have

⁴The 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.

⁵Individuals 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.

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

⁷The 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.

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

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

⁹There 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.

¹⁰The 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.

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 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.

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

¹²The 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.

2.3 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 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.

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

Table B.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 B.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 B.2 shows summary statistics of the outcome variables. The first variable is binary and takes the value of one if 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 B.2 shows that around 23 percent of the taxpayers corrected their reports or justified the differences 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

¹⁴For 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.

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

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

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 B.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 B.3 shows summary statistics for the covariates. Average taxable 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

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

¹⁸I 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.

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

²⁰Very comprehensive literature reviews on the determinants of tax compliance can be found in Andreoni, Erard and Feinstein (1998); Slemrod (1992) and Torgler (2007).

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.

2.4 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 identi-

²¹Among 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.

fying 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 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:

$$treated = \alpha_1 + f_l(d) + \beta_1(above) + f_r((above) * d) + u \quad (2.1)$$

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:

$$outcome = \alpha_2 + h_l(d) + \beta_2(above) + h_r((above) * d) + e \quad (2.2)$$

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, Todd and Van der Klaauw (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.

2.5 Results

2.5.1 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 A.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. Figures A.2 through A.11 show 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 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.

²²The 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).

2.5.2 Discontinuity in the Probability of Treatment

I begin by estimating the discontinuity in the probability of receiving the tax notification. Figure A.12, 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:

$$AIC = N\ln(\hat{\sigma}^2) + 2p$$

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 B.4, 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 A.12 shows that there is a large change in the probability of receiving the tax notification at the selection threshold. Table B.4 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

²³The 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.

2.5.3 Effects on Compliance and Reported Taxes

To analyze the discontinuities in outcome variables, I use figures with the same features as Figure A.12. The regression results in Tables B.5 through B.7 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 A.13 shows that the probability of compliance changes significantly at the selection threshold. Table B.5 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 use the post-treatment ITA 2010. Figure A.14 shows that the estimated discontinuity for this variable seems to be large. Table B.6 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 B.8 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, Sanders and Oreopoulos (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 B.8 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.

2.5.4 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 A.15 shows that there is a discontinuity in reported ITA 2011 at the selection threshold. Table B.7 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

²⁴Following Lindo, Sanders and Oreopoulos (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.

caused by attrition, I follow the same steps used for the reported ITA 2010 in the previous sub-section. Panel B of Table B.8 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 A.16 shows that there is a discontinuous reduction in the likelihood of under-reporting at the threshold point. Panel A of Table B.9 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 A.17 shows that the probability of over-reporting jumps up at the cut-off point. The corresponding estimates in Panel B of Table B.9 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 percent and 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, Erard

and Feinstein, 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, Cronshaw and McKee, 1993). 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 under-reported less in 2011 with respect to 2010). Interestingly, that gap jumps discontinuously at the cut-off point (Figure A.18). Table B.9 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 coeffi-

²⁵The 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 improbable that two bombs will fall precisely in the same crater in a short period of time.

cients 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 (2014), 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, Cronshaw and McKee (1993) 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 B.9 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 introduced to the tax compliance literature by Allingham and Sandmo (1972).

2.6 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

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.

3. PRICE SALIENCE AND SOCIAL COMPARISONS AS POLICY INSTRUMENTS: EVIDENCE FROM A FIELD EXPERIMENT IN ENERGY USAGE

3.1 Introduction

Energy conservation has become a significant matter in public policy in recent years, mainly because of the volatile production costs and increasing climate change concerns. Consequently, it is important to understand the effects of policy instruments aimed to reduce electricity consumption. Traditionally, economic or price measures have been considered the first-best policies to address this issue. In that sense, policymakers often use non-linear financial incentives in order to induce individuals to change behavior as part of energy and social policy. For example, electric, natural gas, and water utilities generally set tariffs that are a non-linear function of consumption. The marginal price of one additional unit of consumption discretely rises at specific levels of consumption. Likewise in social policy, the retirement benefits from Social Security discretely rise at certain ages of retirement. Also, discrete changes in the marginal income tax induced by the Earned Income Tax Credit (EITC) could cause lower income households to work more hours. The policy motivation behind such non-linear schemes is that the discrete change in marginal incentives will induce individuals to change behavior.

However, there is mixed evidence that these non-linear incentives alter behavior in the energy sector. (e.g. see Reiss and White (2005), Borenstein (2009, 2012), and Wolak (2011), Ito (2014)). Several explanations exist for the limited behavioral response. Individuals may be unaware of the non-linear incentives or face choice frictions that prevent them from changing behavior. Alternatively, even after ac-

counting for optimization frictions, individuals may be inelastic with respect to the incentive. Some existing field experiments have used information interventions to make the non-linear incentives more salient in a variety of domains.¹

In contrast, recent research has shown that non-pecuniary strategies can be used to influence behavior in a variety of areas.² Social comparisons have been particularly effective to encourage environmental conservation. (e.g. see Schultz et al. (2007), Nolan et al. (2008), Allcott (2011b), Ferraro and Price (2013), and Costa and Kahn (2013)).

These non-pecuniary strategies to induce conservation are actually frequently used by policymakers. In the energy domain, the use of social comparisons is increasingly seen as a powerful policy instrument. The alternative policy instrument – prices – can face large political constraints. Policymakers and regulators are generally reluctant to set prices at the true marginal social cost for utilities such as electricity, water, and natural gas. In fact, in many developing countries energy prices are set far below the market price, and such government subsidy schemes contribute to government debt. In Ecuador for instance, the electricity subsidies accounted for approximately 2% of the public sector expenses in 2010. According to the Ecuadorian Ministry Coordinator of Production, Employment and Competitiveness (MCPEC), the cost of electricity to consumers would increase around 27% if these subsidies were removed (MCPEC, 2010). In that context, an important question is whether information interventions can be used in developing countries to induce conservation of energy, and therefore reduce funds allocated to subsidies.

Despite the growing research on the effects of both non-linear incentives and so-

¹These field experiments present a mixed record on the ability of salience interventions to change behavior (e.g. see Liebman and Luttmer (2011), Chetty and Saez (2013), Kahn and Wolak (2013), and Jessoe and Rapson (2014)).

²For instance, retirement savings, (Duflo and Saez, 2003), coordination games (Eckel and Wilson, 2007), provision of public goods (Harper, Konstan and Li, 2010), and charitable giving (Meer, 2011).

cial comparisons on conservation, little is known about the relative efficacy of these instruments.³ In that context, the main goal of this paper is to identify the relative magnitudes of these two incentives in a developing country. To reach this goal, we partnered with the Quito Electric Company (*Empresa Eléctrica Quito-EEQ*) to implement a large scale randomized controlled trial. In our field experiment, informational letters were attached to the electricity bills of randomly selected households in March 2014. To our knowledge, this is the first field experiment in the energy sector that jointly studies non-linear incentives and social comparisons in an effort to estimate their relative effects.

Substantial price variability is important to test for the effects of pecuniary incentives on conservation. Typically it is very difficult to find utilities that have large changes in tariffs for small changes in consumption. However, households in Ecuador, and in Quito in particular, face a tariff with large changes in the total electricity bill for an additional kWh of monthly consumption (i.e. notches). These notches do not appear to induce a consumption reduction because we find no evidence of discontinuity of the distribution before the notches or bunching around them in historical consumption data. This might be evidence of the lack of salience of the complex electricity tariff in Quito. Since it has been shown that salience is an important determinant of economic decisions (Finkelstein, 2009; DellaVigna and Pollet, 2009; Chetty, Looney and Kroft, 2009), and since random assignment of prices in real markets is most likely impractical, then making the notches salient is an ideal exercise to test for the effects of non-linear price schedules on conservation.

In our randomized controlled trial we explore the effects of three information

³ An important contribution is Reiss and White (2008). Using longitudinal data from the 2000-2001 California electricity crisis, the authors find significant demand responses to a large and unanticipated price shock, and a voluntary conservation campaign. However, the paper does not offer an elaborate discussion on the relative efficacy and recognizes the necessity of further research on the matter.

interventions. The first treatment makes the most important notch (the 111th kWh consumed increases the total electricity bill by around 40%) salient. The second makes a social comparison. Finally, we use the two pieces of information together as a third treatment to test for additive effects.

For the households with historical consumption above the notch, we find that the social comparison treatment reduces consumption by approximately 1.36 kWh/month (around 1%), and it is significant at the 5% level. On the other hand, the price salience treatment estimate is approximately one third of that of the social comparison, and it is not statistically significant at standard levels. However, we also find suggestive evidence that the effect of the price salience treatment exists only for households who were just above the notch, whereas the effect of the social comparison is significant for both households who were just above and well above the notch. These heterogeneous effects for the price notch salience treatment might explain its relatively small overall treatment effect.

The estimate for the treatment that combines price salience and social comparison information falls between the other two estimates and is not significant. The relatively small effect of it suggests that the price salience information does not strengthen the impact of the social comparison, and might even weaken it. This could be explained if the extra information reduces the probability that the customer reads and understands the message. Importantly, we find that none of the treatments have a significant effect for households historically consuming below the notch. Hence, we find no evidence of a “boomerang” effect created by the information intervention. Also, and consistent with previous literature, we find that the effect of our interventions diminishes over time.

We believe that these results contribute to the understanding of the capacity of information interventions to promote conservation of energy in the context of a de-

veloping country. The larger effect of the social comparison treatment suggests that similar interventions could be used in longer term projects to induce conservation and reduce the fiscal burden of electricity subsidies. Our findings for the price notch salience treatment support previous literature finding that non-linear price incentives are not effective in reducing consumption of electricity. However, our results also suggest that information that makes salient non-linear price schemes might be effective if targeted to the population with consumption levels just above price notches.

3.2 Institutional Background

The Quito Electric Company serves approximately 750,000 residential costumers, 65% of which are located in the Metropolitan District of Quito; the rest belong to nearby *cantones* (political classification similar to counties in the U.S). Table B.10 shows summary statistics for the Metropolitan District of Quito corresponding to 2013. Consumption is relatively low and steady across months, with the average ranging between 137 and 153 kWh, and the median between 123 and 130 kWh.

This is likely due to the moderate climate that shows small variability in temperature throughout the year.⁴ The absence of extremely hot or cold temperatures implies that households generally do not use air conditioners or heaters, and that the main electricity usage comes from refrigerators, televisions and lightning (Table B.11).

Electricity meters are read approximately monthly according to a schedule established at the beginning of the year, and bills are delivered by special courier around one week after the readings.

⁴The average monthly temperature in Quito in 2011 ranged from 57 °F and 60 °F (Inamhi, 2013).

3.2.1 Electricity Tariff in Quito

The electricity tariff in Quito follows an increasing-block tariff (IBT) pricing structure which is nowadays the standard price policy of local utilities both in developed and developing countries. An IBT pricing schedule exhibits an increasing marginal cost per kWh that increases with the monthly electricity consumption level; it is a step function where height is defined by the marginal price and length by the range of consumption that the marginal price applies to. Absent subsidies and other charges, this price schedule would resemble a typical schedule faced by households in the United States.

However, the residential electricity tariff in Quito includes additional charges and subsidies that produce considerable changes in the cost of electricity at certain levels of consumption.⁵ For instance, households consuming at or below 110 kWh per month pay approximately \$8.5 per month, and those consuming 111 kWh pay around \$12 per month. In other words, increasing consumption by less than 1% implies a change in their monthly electricity bill of approximately 40%. The potential savings are approximately equivalent to 0.5% of the median monthly household income in Ecuador. This notch is due to reduced marginal price, fixed fee and waste disposal fee that are part of the subsidy called “Tarifa de la Dignidad” (Dignity Tariff).⁶ Figure A.19 graphs the total bill as a function of monthly consumption level. As can be seen from the graph, the largest discontinuity in total expenditures is precisely at 111 kWh. Notice also from Figure A.19 that there are two additional discontinuities

⁵The described values correspond to December 2013. The value of the subsidies change each month due to the varying amount of the cross subsidy, which is collected from household with consumption above 160 kWh in a given month (10% surcharge) and distributed to households with consumption below 130 kWh in the following month.

⁶The tariff also includes mandatory contributions for public lighting and Fire Department equal to 9.5% of total electricity expenditure before subsidies, and to a fixed fee of \$1.59, respectively. Finally, there is a waste disposal fee composed of a flat fee and a charge proportional to the electricity consumption level.

in the total bill: at 131 and 161 kWh, although these are less prominent than the one at 111 kWh.

3.2.2 Pre-treatment Distribution of Consumption

The large changes in the cost of electricity do not appear to induce a consumption reduction because we find no evidence of discontinuity of the distribution before the notches, or bunching around them, in historical consumption data. Figure A.20 plots the distribution of consumption corresponding to December 2013. Notice that the plotted density appears to be smooth across the 111th notch. Also, following the procedure proposed by McCrary (2008), we estimate the discontinuity in the density at the 111th threshold, and find that it is small and statistically indistinguishable from zero. Similar results were obtained for the other months and the other two notches.

The lack of evidence of discontinuities in the density before the notches might be explained by the imperfect control that households have on the consumption of electricity (frictions), which makes it difficult to adjust consumption just below a given level. For example, households usually do not have real time information on their consumption. It is also very likely that they do not have complete knowledge on the energy use of different appliances. It could also be explained by an inelastic response to the price schedule. However, if consumers were totally aware of how the tariff works, and willing to reduce consumption in an effort to take advantages of the subsidies, it would be reasonable to expect bunching in the distribution around the notches. We explore this option by using method proposed in Chetty, Friedman, Olsen and Pistaferri (2011) to estimate if there is bunching around the most important notch. Figure A.21 plots frequencies along with the estimated density corresponding to December 2013. There is no graphical evidence of bunching around

the notch.

The fact that the modal point of the distribution is close the 111th notch might suggest that the non-linear price scheme moved the distribution from a higher level of consumption, implying that individuals are aware of the subsidy. This does not seem to be the case since the distribution of consumption corresponding to December 2006 (six months before the creation of the subsidy) is very similar to the one in 2013 (Figure A.22).⁷

3.3 Conceptual Framework

Standard economic theory predicts that consumption is a function of prices. In particular, the central assumption is that individuals optimize with marginal price, and hence changes on it should bring about changes in the quantity demanded. In this sense policymakers often use non-linear financial incentives in order to induce individuals to change behavior as part of energy policy. The policy motivation behind such non-linear schemes is that the discrete change in marginal incentives will induce individuals to change behavior.

However, there is mixed evidence that these non-linear incentives alter behavior in the energy sector. (e.g. see Reiss and White (2005), Borenstein (2009, 2012), and Wolak (2011), Ito (2014)). Several explanations exist for the limited behavioral response. Individuals may be unaware of the non-linear incentives or face choice frictions that prevent them from changing behavior. Alternatively, even after accounting for optimization frictions, individuals may be inelastic with respect to the incentive. In this context, the use of information interventions, becomes an alternative to induce conservation of electricity.⁸

⁷The distribution of consumption corresponding to other months before the creation of the subsidy is very similar to the one in Figure A.22.

⁸This is considered in Jessoe and Rapson (2014). Using a randomized controlled trial they find that high-frequency information about residential electricity usage significantly increases the price

We want to emphasize that our paper does not speak to the size of optimization frictions and the structural price elasticity of electricity, but only to the relative magnitude of the information interventions included in our design.

3.3.1 Information on Prices

In a system of non-linear pricing, consumers might not be aware of how the tariff works, or might have incomplete knowledge on how much can be saved if they reduce consumption up to a certain level. Hence, information that explains how the tariff works can be used as an incentive to induce conservation. It has been shown that salience is an important determinant of economic decisions (Finkelstein, 2009; DellaVigna and Pollet, 2009; Chetty, Looney and Kroft, 2009). If prices are not salient consumers may not be able to fully optimize. Moreover, there may be a cognitive cost of understanding complex pricing schedules. Therefore, information about how the electricity tariff works, given in a simple way, is likely to be welfare improving.⁹

In the context of consumers in Quito, we have good reasons to think that the electricity tariff is not salient. First, the electricity bill includes the monthly consumption, subsidies (if any), other charges, and the total due. However, there is no specific information regarding how the non-linear tariff works. In particular, there is no information on how the price changes at the certain levels, due to subsidies (see Figure A.23).¹⁰ Moreover, as discussed in the previous section, we find no evidence of discontinuity in the distribution of consumers before the notches or bunching elasticity of demand.

⁹The impact of the provision of simplified information has been explored in a variety of domains such as retirement plan decisions (Duflo and Saez, 2003); labor supply response to the income tax rates Chetty and Saez (2013); government transfer programs take-up rates (Bhargava and Manoli, 2013).

¹⁰Detailed information about the electricity tariff relevant for each month is available in the website of EEQ (www.eeq.gov).

around them that may suggest that the notches induce conservation of electricity. Then, the use of price salience is an ideal (indirect) way to test for the effects of non-linear price schemes on conservation of electricity.

3.3.2 Social Comparisons

Recent literature has found evidence on the important impact that non-price incentives can have on conservation. Social comparisons have been particularly effective to encourage environmental conservation (Schultz et al., 2007; Nolan et al., 2008; Allcott, 2011a; Costa and Kahn, 2013; Ferraro and Price, 2013).

Two mechanisms could drive the behavioral response to social comparisons. Households with incomplete information about their house’s production of energy services may gain private information on the optimal level of consumption (Becker, 1965). Alternatively, households may be responding to social norms that affect their moral payoffs (Levitt and List, 2007). If consumers bear a moral cost from energy consumption (derived from environmental concerns, for instance), and if we assume that this utility depends on the beliefs about the “social norm”, then social comparisons could change the moral cost of consumption.

It is important to mention that our experiment does not try to disentangle the effects of these two mechanisms, but, as stated before, aims to measure the impact of the social comparisons relative information about the tariff schedule in a common setting.

3.4 Research Design

Our field experiment focuses on households with historical consumption around the 111th notch in the metropolitan area of Quito. In particular, it targets the approximately 64,000 residential customers with monthly average consumption between 100 and 125 kWh in 2013. We have selected this group because they are more

likely to respond to information regarding the 111 kWh notch. The experiment was designed within the conceptual framework introduced above, and consists of an “informational nudge” where randomly selected households receive, attached to their monthly electricity bill in March 2014, additional information regarding three different treatments: (1) price salience, (2) social comparison, and (3) both together. A fourth group receives no informational flyer and serves as the control group. The households in our sample were equally split among these groups as can be seen in Table B.12. We randomize treatment within the 74 sectors and 24 urban parishes in which EEQ divides the metropolitan area. We do this to to guarantee that there are no systematic differences across geographic areas.

The first treatment group receives a flyer that informs the customer of their average consumption in 2013, the size of the price notch and the effect of the notch on their monthly bill. Specifically, the customers receive information on how much they would save (pay additionally) if they reduce (increase) their monthly consumption just below (above) the 111th notch. For example, a household with average consumption of 115 kWh/month is told how much approximately the monthly bill falls if consumption is reduced to 110 kWh. In addition, the flyer suggests several energy saving tips to reduce consumption.

A second treatment group receives an information intervention that makes a social comparison between the household and other households around 110 kWh. For example, a household with average consumption of 115 kWh is informed that “an average household like you” consumes approximately 110 kWh and that the household averages 115 kWh. Since our data show that the historical distribution of consumptions has its modal value close to the most important notch, we exploit this fact to use the 110 kWh level as a reference point in this treatment. We use this feature to safely compare the effect of the two information interventions as they target

the same population and use the same reference point. As with the first treatment, the flyer in the second treatment offers several energy saving tips. Importantly, the flyer for the second treatment group *makes no mention of the price notch*.

To test for interactive effects between the two treatments, a third treatment combines each of the two information treatments by informing customers of the price notch and making a social comparison. Figures A.24 through A.29 show sample letters of the three treatments in Spanish with the corresponding translations to English.

In the results section presented below, we separately analyze the effect on consumption for households who historically consumed above and below 110 kWh. For households who historically averaged between 111-125 kWh, each treatment is predicted to reduce consumption. In the case of households receiving the price salience treatment, the information intervention informs households of notable dollar savings if consumption is reduced. This information could be useful to consumers unaware of the price notch, or to reduce the cognitive burden to calculate the potential savings of conservation to those who had imperfect knowledge about it. In the case of the social comparison treatment, and as explained in the previous section, the information could be used by households to learn about the optimal level of consumption, and also to increase the moral cost of consumption. Therefore, we expect that higher-than-average consumers respond to such information by reducing consumption.

For households who historically averaged between 100-109 kWh, the treatments could increase or decrease consumption. Households receiving the price salience treatment could react by reducing consumption to reduce the possibility of crossing the 111th threshold, which would increase their payments by several dollars. However, their consumption could increase if households believe they could increase consumption without crossing the threshold. In the case of the social comparison

treatment, households consuming below the reference, could adjust its beliefs about the optimal level of consumption and also increase consumption, or they could try to reduce consumption to avoid the likelihood of becoming a higher-than-average consumer. Naturally, for all treatments it is likely that some proportion of consumers do not read the information or just ignore it.

3.5 Results

As a first step we show that the four groups we have considered are balanced with respect to average consumption corresponding to 2013. This holds for the complete sample of households and for the samples split above and below the 111 kWh notch. The three panels of Table B.12 show that the average, median and standard deviation of consumption is very similar across treatments. Additionally, Table B.13 shows estimates of the differences in average consumption across groups. For all the pairwise comparisons the differences are less than 0.01% and none of them are significant at the 10% level.

In our estimations the dependent variable is a “month” of consumption, which is the average daily consumption during the meter-read window multiplied by 365/12. We first consider the effects for the three months after the intervention (i.e. April-June, 2014). Following the conceptual framework described above, we separately analyze households with historical consumption above and below the notch.

3.5.1 Treatment Effects for Households Historically above the Notch

Table B.14 shows the estimated average treatment effects corresponding to households who historically consumed above the notch. In the first specification we consider a cross sectional estimation that includes only post-intervention observations with year-by-month fixed effects. We find that the social comparison treatment reduces consumption by approximately 1.36 kWh/month (around 1%), and it is signif-

icant at the 5% level. On the other hand, the price notch salience treatment estimate is approximately one third of that of the social comparison, and it is not statistically significant at standard levels. The coefficient for the treatment that combines the two pieces of information falls between the other two estimates and is not significant.

To increase the precision of our estimations we add pre-intervention consumption to the basic specification. Specifically, we include monthly consumption for the same quarter in the year previous to the experiment (i.e. April-June, 2013), and consumption corresponding to the two months before the intervention (i.e. January-February, 2014). These latter variables are used to capture any changes in household consumption patterns after 2013 but before the intervention. The coefficients for this specification are very similar to the basic one. The only noticeable difference is in the coefficient for the treatment that combines the two information interventions, which is slightly bigger and significant at the 10%.

We take advantage of the panel nature of our data and use information on consumption of electricity ranging from February, 2012 to June, 2014 in our last two specifications. The specification in column three is a standard difference-in-difference estimation, whereas the next specification uses both year-by-month and household fixed effects. The estimates for these specifications are very similar to the previous ones.

The effect we find for the social comparison treatment is around half the size of OPOWER Home Energy Reports utilized in the U.S., but it still suggests that low-cost information interventions can induce energy conservation. The relatively small effect of the third treatment suggests that the price salience information does not strengthen the impact of the social comparison, and might even weaken it. This could be explained if the extra information reduces the probability that the customer reads and understands the message.

The non-significant effect we find for the price notch salience treatment is somewhat surprising. This could be due to the frictions mentioned before, which prevent households from optimizing consumption, or because customers did not actually read the messages. Even though we believe these two explanations are reasonable, the fact that we find significant effects for the first treatment implies that at least a small proportion of households read the flyers, and that some were able to successfully reduce consumption, despite the frictions. Another possibility for the lack of response to the price notch treatment is the relatively small impact that non-linear incentives have on behavior in the energy sector (Reiss and White (2005), Borenstein (2009, 2012), and Wolak (2011), Ito (2014)). Alternatively, the small size of the subsidy relative household income (around 0.5% of the median monthly household income) could explain the absence of a significant effect.

Our finding could also be explained by heterogeneous effects. In particular, those households whose consumption falls relatively far from the notch might be less likely to alter behavior than those with consumption close to it. This is because it is more difficult for the former group to reduce consumption to take advantage of the subsidy. To explore this option, we divide the sample into households who were just above the notch (averaging 111-115 kWh/month), and households that were well above the notch (averaging 116-125 kWh/month). Results are presented in Table B.15, which has the same specifications considered in Table B.14. For the latter group, the price notch treatment had no statistically significant effect on consumption. On the other hand, we find suggestive evidence that this treatment induced conservation for households who were just above the notch. Unfortunately, our estimates are imprecise and only the one in the second specification is significant at the 10% level and has a size similar to the corresponding to the social comparison treatment. These findings imply that a considerable effect of the price salience treatment for households

who historically consumed just above the notch cannot be ruled out.

In contrast, Table B.15 shows that the effect of the social comparison treatment is similar for households who historically were several kilowatt hours above the notch and for those who were just above it. This suggests that the effect of the social comparison is significant across the consumption distribution, whereas the effect of the price notch information exists only for those who were just above the notch.

3.5.2 Treatment Effects for Households Historically below the Notch

Importantly, we find that none of the treatments have a significant effect for households historically consuming below the notch. Table B.16, which considers the same specifications in the two previous tables, shows that all the estimates across treatments and specifications are not significant. Hence, we find no evidence of a non-desirable “boomerang” effect created by the information intervention.

3.5.3 Impermanence of Effects

Recent empirical evidence suggests that non-pecuniary incentives only hold in the short term (See for instance, Gneezy and List (2006), Landry et al. (2010), Ferraro and Price (2013)). As pointed out by Gneezy and List (2006), this could be explained if these incentives have the greatest impact shortly after the intervention, when they activate moral sentiments. However, they disappear over time as the decision maker forgets about the intervention.

We test for this in our experiment by estimating separate treatment effects for the three quarters after the intervention for the sample of households with historical consumption above the notch. We do this by interacting indicators for each quarter with indicators for our treatments in the panel framework used in the fourth specification of the results tables. The results are shown in Table B.17. Our findings are consistent with previous literature in that the treatment effects are diminishing

over time. The coefficients corresponding to the second and third quarters after the interventions are smaller than those corresponding to the first. In the case of the social comparison intervention, the estimates corresponding to the second and third quarters are no longer significant at standard levels.

3.6 Conclusions

The second essay uses a large-scale field experiment to analyze the role of information interventions as policy instruments to induce conservation of electricity. A unique feature of the electricity tariff in Quito, Ecuador is used to make salient a sizable price notch that apparently had not historically induced consumption reduction. The experiment also explores the effect of social norms to promote conservation of electricity.

This paper contributes to the literature by estimating the relative magnitudes of these two information interventions in a common setting. Specifically, our design uses the mentioned notch as a reference point for both interventions and targets the same population.

Results are consistent with recent literature finding that social comparisons are effective to encourage environmental conservation. They show that consumers historically consuming above the notch respond to the social comparison by reducing consumption by approximately 1%. The size of this effect is around half the size of OPOWER Home Energy Reports utilized in the U.S., but the size we find still suggests that low-cost information interventions can induce energy conservation.

On the other hand, the estimate for the price notch salience is approximately one third of that of the social comparison, and it is not statistically significant at standard levels. The lack of response to this treatment could be explained by frictions that do not allow households to adjust consumption, or because most customers did not read

the messages. Given the sizable effect of the social comparison treatment, it must be the case that at least a small proportion of targeted customers read the messages and were able to successfully reduce consumption. Hence, the findings imply that the social comparison treatment was indeed more effective in promoting conservation. However, there is also find suggestive evidence that the effect of the price salience treatment exists only for households who were just above the notch, whereas the effect of the social comparison is significant for both households who were just above and well above the notch. These heterogeneous effects for the price notch salience treatment might explain its relatively small overall treatment effect.

The experiments also explores the effect of a third treatment that combines the price notch and social comparison information. Results show that the effect of this treatment falls between the other two. This suggests that the price salience information does not strengthen the impact of the social comparison, and might even weaken it. This could be explained if the extra information reduces the probability that the targeted customer reads and understands the message.

Results also show that none of the treatments have a significant effect for households historically consuming below the notch. Hence, we find no evidence of a non-desirable “boomerang” effect that increases consumption. Also, and consistent with previous literature, we find that the effect of the social comparison intervention diminishes over time.

We believe that these results contribute to the understanding of the capacity of information interventions to encourage conservation of energy in the context of a developing country. The larger effect of the social comparison treatment suggests that similar interventions could be used in longer term projects to promote conservation and reduce the fiscal burden of electricity subsidies. Our findings for the price notch salience treatment support previous literature finding that non-linear price incentives

are not effective in reducing consumption of electricity. However, our results also suggest that information that makes salient non-linear price schemes might be effective if targeted to the population with consumption levels just above price notches.

4. THE IMPACT OF THE ARIZONA IMMIGRATION LAW (SB 1070) ON THE PROPORTION OF THE NONCITIZEN HISPANIC STATE POPULATION

4.1 Introduction

Immigration legislation and its consequences are among the most important topics in the public policy debates in the U.S. In recent years a series of measures at the federal and state level have modified the immigration system. These actions have attracted the attention of researchers trying to assess their effects.¹

In that context, the main objective of this paper is to estimate the effect of state immigration legislation on the stock of undocumented population. To reach this goal, I study the Arizona Senate Bill 1070 enacted in 2010 (SB 1070), which is arguably one of the most strict state immigration bills passed in recent years. In particular, Arizona was the first state to enact a law that mandates law enforcement personnel to inquire about the immigration status of individuals suspected to be illegal aliens, and detain those who fail to show proper documentation. It also makes it a crime for an unauthorized individual to apply for or hold a job in Arizona. In other words, this law increases the expected costs and risk of being an illegal immigrant in Arizona.

Anecdotal evidence suggests that the passage of this law caused a reduction in the Hispanic population in Arizona, with churches, schools, businesses, and health care facilities reporting drops in the number of Hispanic users.² Clearly, these reports do not imply a causal relation and could be misleading. Instead of relying on information not statistically representative, I estimate the effect of this law on the composition of the population Arizona, specifically, in the proportion of noncitizen Hispanics

¹See for instance, Hanson and Spilimbergo (1999); Dávila, Pagán and Soydemir (2002); Hanson, Robertson and Spilimbergo (2002); Gathmann (2008); Amuedo-Dorantes and Bansak (2012); Good (2013).

²Goldberg (2010), Gomez (2010)

living in that state. In order to get a consistent estimate of this effect, I use state-level aggregate data from the Current Population Survey (CPS) and the synthetic control method proposed in Abadie, Diamond and Hainmueller (2010) to calculate a counterfactual for Arizona.

In that sense this paper follows closely Bohn, Lofstrom and Raphael (2014). The authors estimate the impact of the Arizona’s 2007 Legal Arizona Workers Act (LAWA) using synthetic control methods, and find that this law caused a notable decline in the share of the Hispanic noncitizen population in Arizona.

The use of SB 1070 has one important advantage over the use of LAWA. The latter aimed to reduce the “demand” of undocumented workers by requiring employers to verify the identity of new employees using the federal E-verify system. On the other hand, SB 1070 targeted undocumented individuals directly, therefore not only making it more difficult to find a job, but also more risky to reside in Arizona. This feature offers a wider perspective to study state-level legislation, by allowing the estimation of its impact on the labor supply not included in the formal sector, and also on the population not looking for a job.

This paper is also related to Hoekstra and Orozco-Aleman (2014), which studies to effect of SB 1070 on the flow of immigrants from Mexico. The paper finds that passage of the Arizona Immigration Law reduced the flow of undocumented immigrants from Mexico to Arizona by 30 to 70 percent. Another closely related paper is Amuedo-Dorantes and Lozano (2015). The authors use synthetic control methods to estimate the effect of SB 1070 on the proportion of noncitizen Hispanics in Arizona and find that the effect of the law since it was passed until 2013 was minimal; however, the paper does not consider short term effects.

One contribution of this paper is that it assess the flexibility of the illegal population to move as a response to changing conditions. Shortly after the law was

passed, on April 23, 2010, lawsuits were filed against it, and before it went into effect, on July 29, the most controversial parts of the law were blocked by a U.S. District Court. Moreover, in April 2011, the United States Court of Appeals upheld the district court's decision. However, the U.S. Supreme Court upheld in 2012 parts of the original law, including the one mandating, if there is reasonable suspicion, police officers to inquire the immigration status of individuals suspected to be in the country illegally. These events allow the estimation of the short-term effects of this state legislation given its changing implications. In that sense, this paper joins the small, but growing literature, studying the response of undocumented individuals to unfavorable conditions (See for instance, Cadena and Kovak (2013), Watson (2013)).

Results indicate that the Arizona Immigration Law produced a statistically significant decline in the share of noncitizen Hispanics in Arizona between 10% and 16%. However, this effect lasted less than one year, as the evidence suggests that it vanishes after a few months.

4.2 Institutional Background

Arizona has historically passed legislation aimed at restricting illegal immigration. For instance, under a 1996 Arizona law, driver's licenses are available only to citizens or those who prove legal presence in the country. In 2004 the Arizona Proposition 200 was passed requiring proof of citizenship to register to vote, voter identification at the polling place, and verification of immigration status of applicants of non-federally mandated public benefits. In 2007 the Legal Arizona Workers Act (LAWA) was enacted, which mandated all Arizona employers to use the E-Verify system during the employment process to assess the legal eligibility of new employees. In fact, laws similar to SB1070 were approved by the Arizona state legislature in 2006 and 2008, but were vetoed by the Governor.

The passage of SB 1070 on April 23, 2010 received significant attention by the media nationwide, which included live broadcasting of the Governor signing the bill. This law was qualified as the nation's toughest bill, and caused immediate reaction for and against it.³

In general terms SB 1070 implies that immigration offenses in Arizona are not only violations of federal laws, but are also state crimes. Specifically, it makes it illegal for an unauthorized individual to apply for or hold a job in Arizona. It also imposes penalties on those transporting, sheltering or hiring undocumented individuals. But the most controversial part of the bill is the so called "show me your papers" provision. This clause requires law enforcement personnel to inquire about the immigration status of those they reasonably suspect are in the country illegally, and to detain those who fail to show proper documentation.

This law was scheduled to go into effect on July 29, 2010. However, just one week after it was signed, it was modified by the Arizona House Bill 2162 (HB 2162). The changes were a response to critics stating that the law encouraged "racial profiling". The main modification indicates that prosecutors would not investigate immigration status based on race, color or national origin. Another adjustment says that law enforcement personnel may only inquire immigration status of those they stop, detain or arrest.

Despite of those modifications, this immigration bill faced several legal challenges, including one filed by the United States Department of Justice. In response to this lawsuit, a U.S. District Judge issued a preliminary injunction on July 28, 2010 that blocked temporally the "show me your papers" provision . Therefore, the enforcement of SB 1070, did not initially include this clause. The decision of the U.S. District was upheld on April 11, 2011 by the Ninth Circuit panel, thus ruling in

³Harris, Rau and Creno (2010), Archibold (2010)

favor of the Department of Justice. However, in July, 2012, the U.S. Supreme Court upheld the “show me your papers” clause, which consequently ended up being part of the Arizona legislation.

4.3 Data and Research Design

To evaluate the impact of the Arizona Immigration Law on the stock of undocumented population living in Arizona, it would be ideal to know the legal status of individuals. Unfortunately, this information is not part of any state-level official survey in the U.S.⁴ Due to this limitation, I use the proportion of Hispanic noncitizens as a proxy for legal status (Passel and Cohn, 2009a), (Passel and Cohn, 2009b).

I utilize the monthly Current Population Survey (CPS) data sets corresponding to the period 2009-2012, and combine them within 5-month periods to estimate the proportion of residents in each state who are reported to be Hispanic noncitizens.⁵ The analysis of this paper focuses on the demographic group more likely to be impacted by the law: Hispanic noncitizens with a high school diploma or less, who are between 15 and 45 of age.⁶ In the following analysis the first “post-treatment” period is the 5-month term after the Arizona Immigration Law took effect, or the interval between August and November 2010. Columns 2 and 3 of Table B.18 show the proportion of the population who are Hispanic noncitizens for both Arizona and the other states. The estimation corresponding to states other than Arizona shows a steady path. On the other hand, the estimation corresponding to Arizona shows a modest upward trend before the law was implemented, and an important reduction

⁴The National Agricultural Workers Survey (NAWS), is the only survey that records legal status of individuals; however, except for California, the data are not available at the state level.

⁵This combination allows the estimation of short-term effects while adding enough observations to increase precision. Estimations with data combined within quarters produce very similar results.

⁶Using the 2008 American Community Survey, Bohn, Lofstrom and Raphael (2014) estimate that in Arizona 90% of the population defined as noncitizen of working age with no more than high school diploma were unauthorized.

from that point and until the period between April and July 2011. Specifically, between the periods April-July 2010 and April-July 2011, the proportion of Hispanics noncitizens went down from around 7.3% to around 5.7%. This period is similar to the one between the partial implementation of the law and the decision of the United States Court of Appeals to uphold the district court’s decision to block the most controversial part of the law (see previous section). From this point, there is an upward trend that lasted until the beginning of 2012, which is when the U.S. Supreme Court decided to uphold parts of the original law, including the “show me your papers” mandate. Table B.18 also shows in columns 4 and 5 the proportion of Hispanics noncitizens with a high school diploma or less, between the ages of 15 and 45. This variable shows a path similar to the one corresponding to Hispanics noncitizens.

The previous estimations offer a general idea of the trends of the variables that motivate this study. However, they do not offer a causal interpretation. In order to get a consistent estimate of the effect of Arizona Immigration Law, it is necessary to obtain a suitable counterfactual. To obtain it, I use the synthetic control method proposed in Abadie, Diamond and Hainmueller (2010). This method produces a synthetic control by using convex combinations of potential control units (donors) and selecting the one that better replicates the trajectory of the treated unit before treatment. Formally, consider $J + 1$ units and T periods. Let unit 1 be the treated one during the periods $T_0 + 1, \dots, T$. The J other units belong to the “donor” group. Let Y_A denote the outcome of interest for the treated unit, and Y_D denote a matrix containing the corresponding outcome for the donor units. The synthetic control method solves the following minimization problem:

$$\hat{W} = \arg \min_W (A_b - D_b W)' V (A_b - D_b W) \quad (4.1)$$

Where A_b is a $(k \times 1)$ vector of pre-intervention variables that predict the outcome of interest for the treated unit, $D_b W$ is a weighted average of the pre-treatment vectors for the donor units, W is a $(J \times 1)$ vector of positive weights that add up to one, and V is a positive $(k \times k)$ semidefinite matrix used to allow different weights to the predictor variables.⁷ Once \hat{W} is estimated, the synthetic control for period t is calculated as $\hat{Y}_t = \hat{W}' Y_{Dt}$. The estimations presented in the following section use the pre-treatment values of the outcome variable as only predictors. The use of other controls did not change the results of the synthetic control method.

This paper follows closely the application of synthetic control methods used in Bohn, Lofstrom and Raphael (2014). Specifically, once the synthetic Arizona is obtained, the treatment effect is calculated as a difference-in-difference estimate. Therefore, the identifying assumption is that in the absence of the Law, Arizona would have followed the same trajectory as the synthetic Arizona. When using the synthetic control method, I excluded from the sample the states that implemented laws similar to SB 1070 between 2010 and 2012: Alabama, Georgia, Indiana, South Carolina, and Utah. The results do not change if these states are not removed.

Table B.19 shows the weights used to construct the synthetic Arizona for the two outcome variables. Four states received positive weights: Texas, California, Washington, and Kentucky. As the table shows the weights are different for the two

⁷The estimations were performed using the Stata Code developed by Abadie, Diamond and Hainmueller (2010), and its default option for selecting the V matrix that uses a regression-based method to minimize the mean squared error for the pre-intervention period. Since the use of the advanced fully nested option produced very similar results, I decided to report only the default estimations.

outcome variables. The large contribution of Texas and California to the synthetic Arizona is not surprising since the two states have a relatively large Hispanic noncitizen population. On the other hand, Washington, and Kentucky have relatively small Hispanic noncitizen population, but they experienced trends similar to Arizona before the implementation of the Arizona Immigration Law. This explains why these states were selected to build the synthetic Arizona.

To perform inference analysis, I use the permutation method proposed in Abadie, Diamond and Hainmueller (2010). In particular, I calculate the same difference-in-difference estimate outlined previously for every state in the sample to obtain a distribution of “placebo” estimates. This distribution allows the comparison of the estimated effect for Arizona with an effect estimated for a state chosen at random. Under the hypothesis non-positive treatment effect, the estimate for Arizona is not expected to be abnormally large within the distribution of placebo effects. To estimate the p-value for this null hypothesis, I follow Bohn, Lofstrom and Raphael (2014).⁸ First, I rank the estimated treatment effects from the smallest to the largest, then calculate the one-sided p-value as the position of the estimate corresponding to Arizona over the total number of estimates.

4.4 Results

I start presenting graphic evidence that compares Arizona and the synthetic Arizona before and after the implementation of the Immigration Law. Figure A.30 shows this comparison for the proportion of Hispanic noncitizens. The synthetic Arizona tracks very closely Arizona before the implementation of the law, but they begin to diverge in the next period. This difference persists for around one year and then vanishes, due to an increase in the proportion of Hispanic noncitizens in Arizona.

⁸Following Bohn, Lofstrom and Raphael (2014), I exclude Arizona from the donor pool for each placebo estimate.

Interestingly, the increase coincides with the decision of the United States Court of Appeals to uphold the district court’s decision to block the most controversial part of the law (“show me your papers” provision). In other words, this change happened around the time when likely the perception of severity of the bill was diminishing. As explained before, the Supreme Court ended up upholding this provision in 2012. This last decision may explain the downward trend of the proportion of Hispanic noncitizens in Arizona since then. Because this change happened several periods after the bill was implemented, I do not estimate effects specific for this period.

Figure A.31 shows the proportion of Hispanic noncitizens between 15 and 45 years of age with high school diploma or less for both Arizona and its synthetic control. The trends that the figure shows are similar to the previous one, but the differences after the implementation of the bill are larger.

Table B.20 presents the corresponding estimates. Panel A shows the differences between Arizona and the synthetic control for three periods. There are no significant differences in the pre-treatment period. On the other hand, the differences in the post-treatment period between August 2010 and July 2011 (first post-treatment period) are -0.7 percentage points for Hispanic noncitizens, and -1.4 percentage points for Hispanic noncitizens with high school diploma or less and of age between 15 and 45. Finally, the estimations in the post-period between August 2010 and December 2012 are approximately half of the ones estimated for the first post-treatment period.

Panel B of Table B.20 shows the difference-in-difference estimates for the first post-treatment period. Since the differences in the pre-treatment period are relatively small, these estimates are very similar to the simple difference estimates. For the proportion of Hispanic noncitizens I find a reduction of around -0.7 percentage points, which implies a reduction of approximately 10%. The application of the permutation test outlined before (see Figures A.32 and A.33) brings about a one-tailed p-value of

0.087. For the Hispanic noncitizens with High School Diploma or less between the ages of 15 and 45 the estimated reduction is around 1.5%, which implies a reduction of around 16%, with a one tailed p-value of 0.043.

Finally, Panel C of Table B.20 shows the estimates for the post-period between August 2010 and December 2012. They are smaller than the ones estimated for the first post-treatment period and no significant at the 10% significance level. The smaller magnitude of these estimates is due mainly to the increase in the proportion of Hispanic noncitizens around the second semester of 2011.

As a robustness check, I use the same methodology to estimate the effect for groups less likely to be affected by SB 1070. Table B.21 shows the estimates corresponding to Hispanics naturalized citizens, non-Hispanics non-citizens, and Hispanic born in the U.S. For all of these groups, I find little evidence that they responded to the law. The estimated effects are close to zero and not significant at standard levels.

4.5 Conclusions

This paper estimates the impact of the Arizona Immigration Law of 2010. The results indicate that this bill produced a significant reduction in the proportion of Hispanic noncitizens living in Arizona estimated to be between 10% and 16%. However, this impact disappeared one year after the implementation of the Law. The short length of the effect could be partially explained by the decision of the United States Court of Appeals to uphold the district court's decision to block the most controversial part of the law ("show me your papers" provision). That event likely reduced the perceived severity of the Law.

These results support previous evidence of the high mobility of the undocumented population in the United States, and contribute to the understanding of the effects

of immigration legislation. In particular, the evidence suggests that the response of the undocumented population facing higher risk of deportation is to quickly move out. The findings also suggest that when that risk diminishes, the undocumented population increases.

It is important to point out the limitations of these results. The most important one is the short length of the events that makes it difficult to estimate effects with precision. Also, the unavailability of legal status in the official surveys, together with the possibility of untruthful answers might bias the estimations. More research is needed to shed light on the effects of the dynamic immigration policies at the federal and state level, not only for the undocumented population, but also for citizens that might be affected by those policies.

5. CONCLUSIONS

The main objective of this dissertation is to estimate the effects of relevant public policies using state-of-the-art methods, emphasizing the importance of the correct identification of treatment effects. To reach this objective, the document studies three public policies and uses different empirical methods to determine their impacts.

The first essay estimates the impact of tax notifications on compliance and reported taxes in Ecuador 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. I also find suggestive evidence that the effect of the intervention persists in the year following the treatment, and 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 here.

The second essay uses a large-scale field experiment to analyze the role of information interventions as policy instruments to induce conservation of electricity. A unique feature of the electricity tariff in Quito, Ecuador is used to make salient a sizable price notch that apparently had not historically induced consumption reduction. The experiment also explores the effect of social norms to promote conservation of electricity.

Results are consistent with recent literature finding that social comparisons are effective to encourage environmental conservation. They show that consumers historically consuming above the notch respond to the social comparison by reducing consumption by approximately 1%. The size of this effect is around half the size of OPOWER Home Energy Reports utilized in the U.S., but the size we find still suggests that low-cost information interventions can induce energy conservation.

On the other hand, the estimate for the price notch salience is approximately one third of that of the social comparison, and it is not statistically significant at standard levels. The lack of response to this treatment could be explained by frictions that do not allow households to adjust consumption, or because most customers did not read the messages. Given the sizable effect of the social comparison treatment, it must be the case that at least a small proportion of targeted customers read the messages and were able to successfully reduce consumption. Hence, the findings imply that the social comparison treatment was indeed more effective in promoting conservation. However, there is suggestive evidence that the effect of the price salience treatment exists only for households who were just above the notch, whereas the effect of the social comparison is significant for both households who were just above and well above the notch. These heterogeneous effects for the price notch salience treatment might explain its relatively small overall treatment effect.

The experiments also explore the effect of a third treatment that combines the

price notch and social comparison information. Results show that the effect of this treatment falls between the other two. This suggests that the price salience information does not strengthen the impact of the social comparison, and might even weaken it. This could be explained if the extra information reduces the probability that the targeted customer reads and understands the message.

Results also show that none of the treatments have a significant effect for households historically consuming below the notch. Hence, we find no evidence of a non-desirable “boomerang” effect that increases consumption. Also, and consistent with previous literature, we find that the effect of the social comparison intervention diminishes over time.

These results contribute to the understanding of the capacity of information interventions to encourage conservation of energy in the context of a developing country. The larger effect of the social comparison treatment suggests that similar interventions could be used in longer term projects to promote conservation and reduce the fiscal burden of electricity subsidies. The findings for the price notch salience treatment support previous literature finding that non-linear price incentives are not effective in reducing consumption of electricity. However, the results also suggest that information that makes salient non-linear price schemes might be effective if targeted to the population with consumption levels just above price notches.

The last essay estimates the impact of the Arizona Immigration Law of 2010. Results indicate that this bill produced a significant reduction in the proportion of Hispanic noncitizens living in Arizona estimated to be between 10% and 16%. However, this impact disappeared one year after the implementation of the Law. The short length of the effect could be partially explained by the decision of the United States Court of Appeals to uphold the district court’s decision to block the most controversial part of the law (“show me your papers” provision). That event

likely reduced the perceived severity of the Law.

These results support previous evidence of the high mobility of the undocumented population in the United States, and contribute to the understanding of the effects of immigration legislation. In particular, the evidence suggests that the response of the undocumented population facing higher risk of deportation is to quickly move out. The findings also suggest that when that risk diminishes, the undocumented population tends to increase.

It is important to point out the limitations of these results. The most important one is the short length of the events that makes it difficult to estimate effects with precision. Also, the unavailability of legal status in the official surveys, together with the possibility of untruthful answers might bias the estimations. More research is needed to shed light on the effects of the dynamic immigration policies at the federal and state level, not only for the undocumented population, but also for citizens that might be affected by those policies.

The results of the three essays presented in this document contribute to the understanding of the impact of the public policies that motivated this dissertation. The conclusions delineated here could be valuable for policymakers interested in strengthen the impact of these policies. The findings of this document also contribute to the understanding of the behavior of individuals in the context of economic and social incentives.

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APPENDIX A

FIGURES

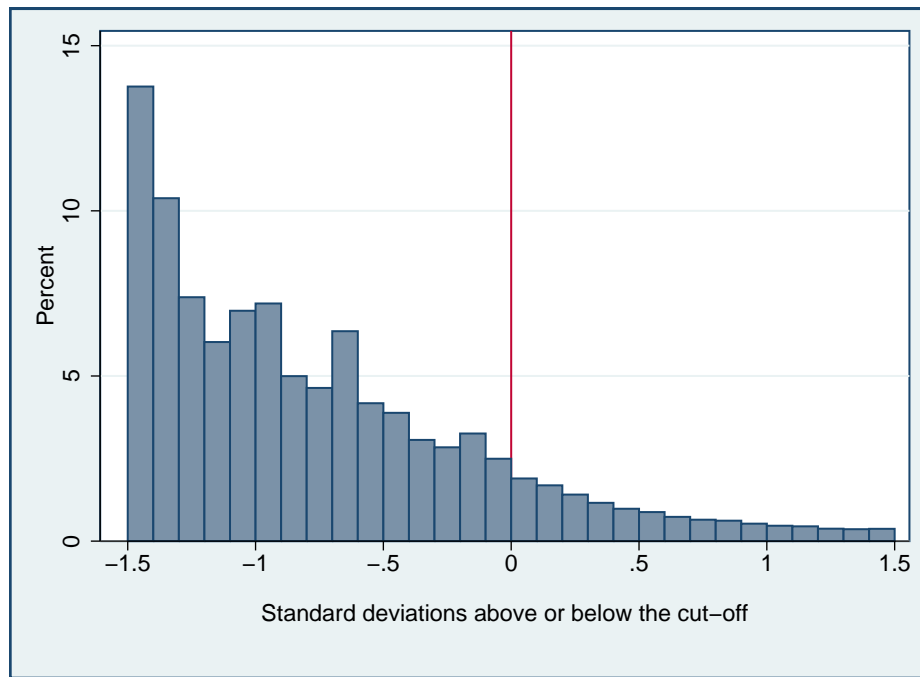


Figure A.1: Histogram of the running variable

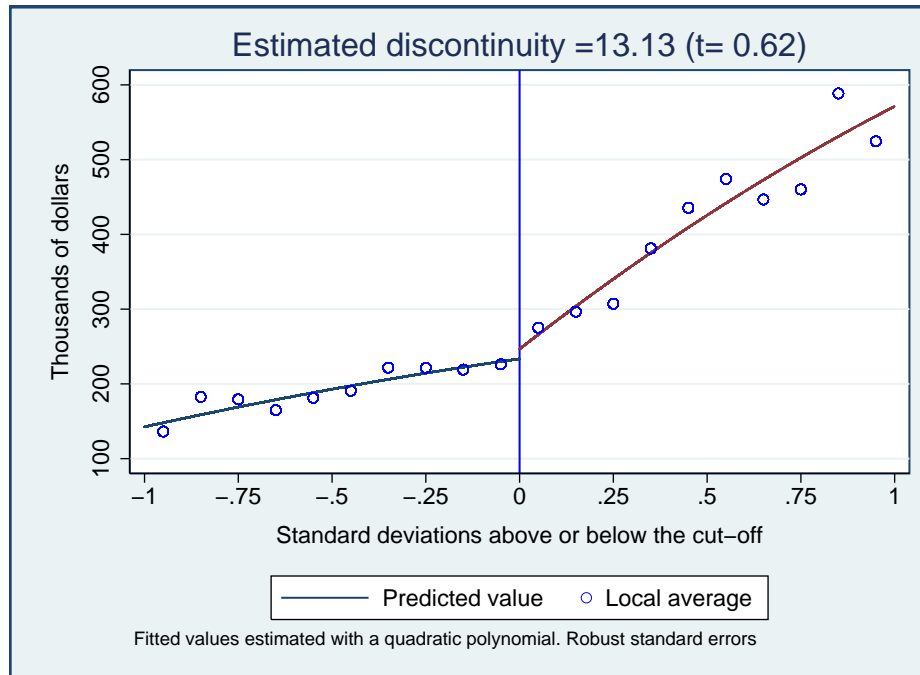


Figure A.2: Discontinuities of covariates at the selection threshold (total assets)

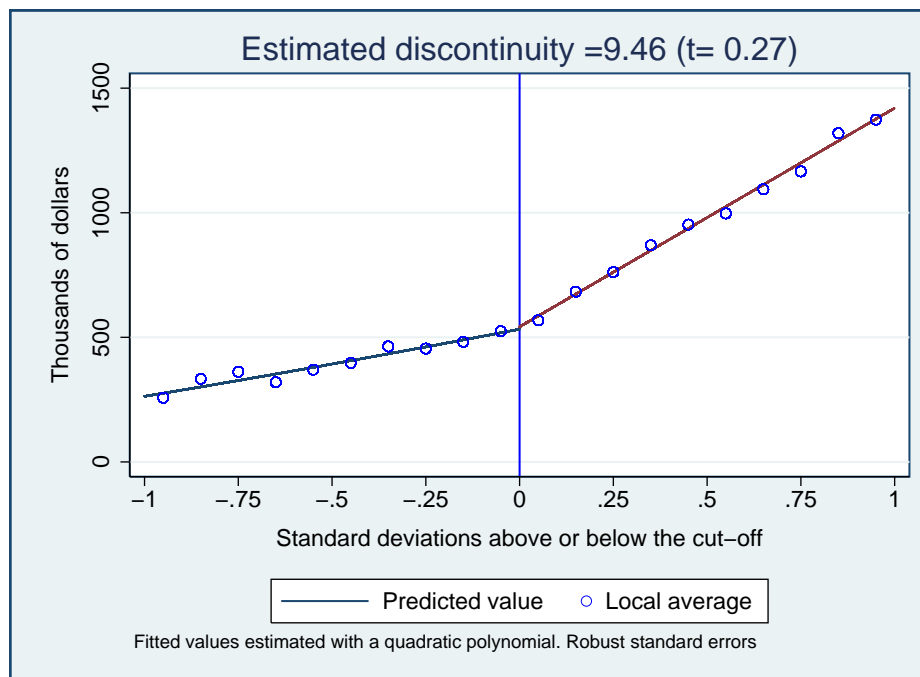


Figure A.3: Discontinuities of covariates at the selection threshold (taxable income)

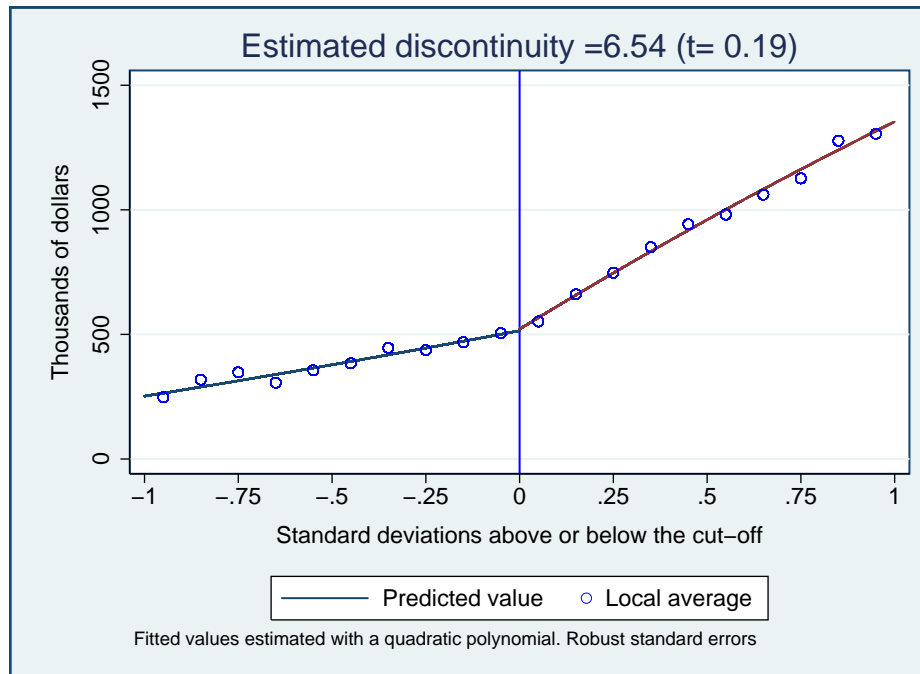


Figure A.4: Discontinuities of covariates at the selection threshold (deductable costs and expenses)

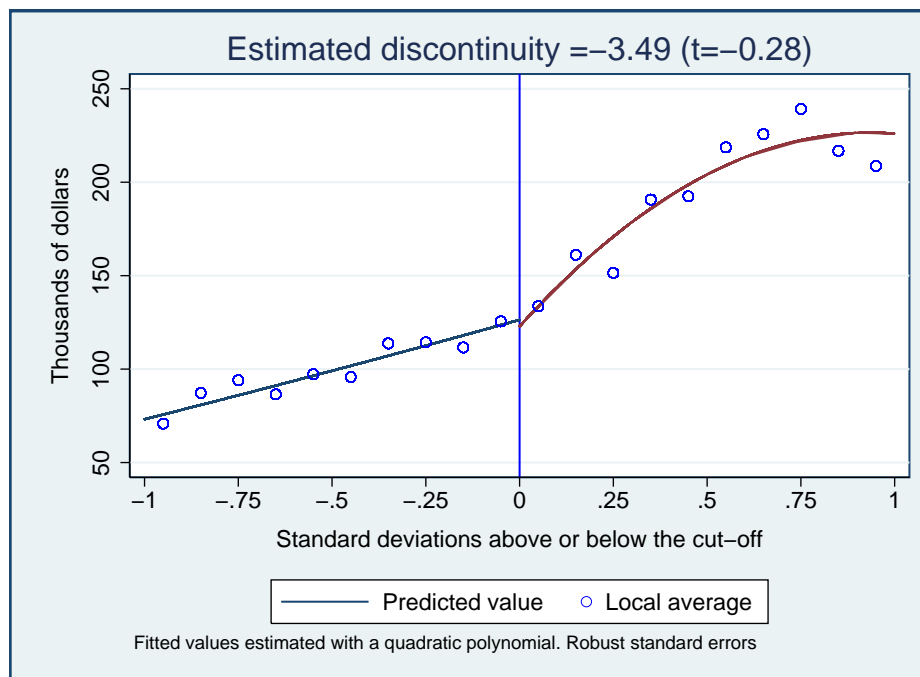


Figure A.5: Discontinuities of covariates at the selection threshold (net worth)

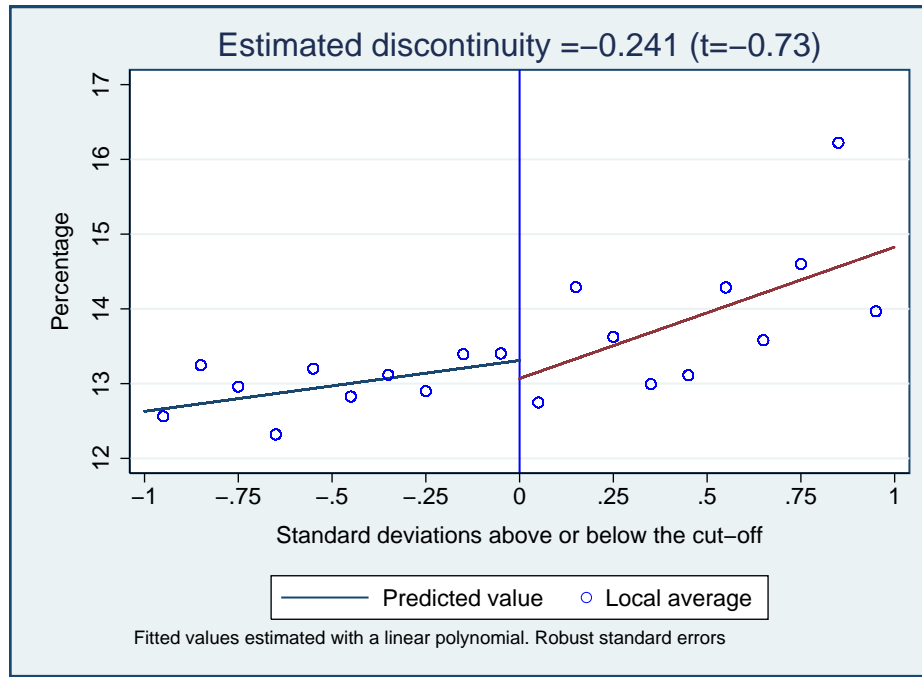


Figure A.6: Discontinuities of covariates at the selection threshold (years of operation)

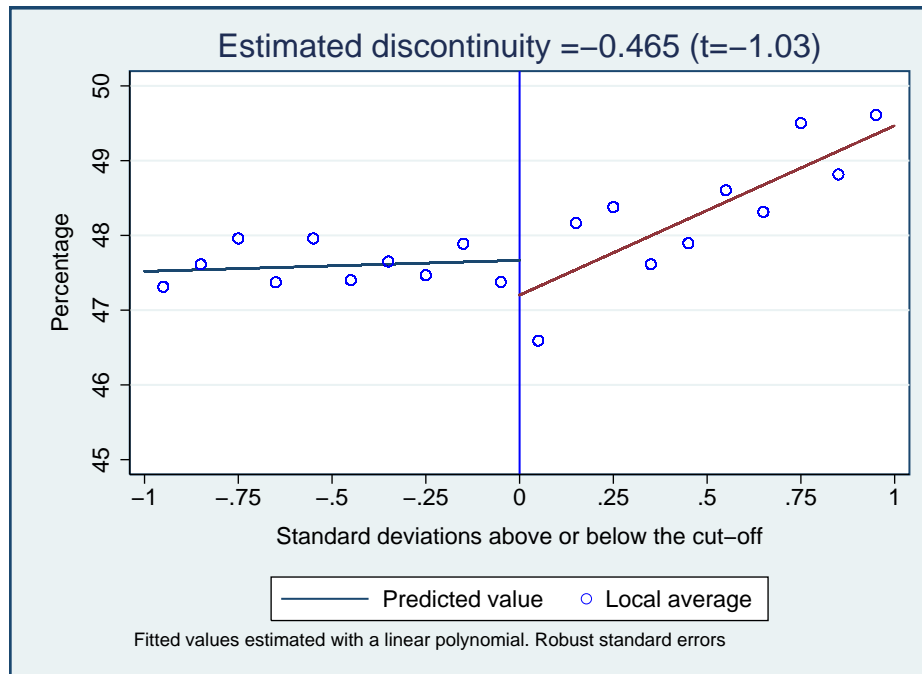


Figure A.7: Discontinuities of covariates at the selection threshold (age of legal representative)

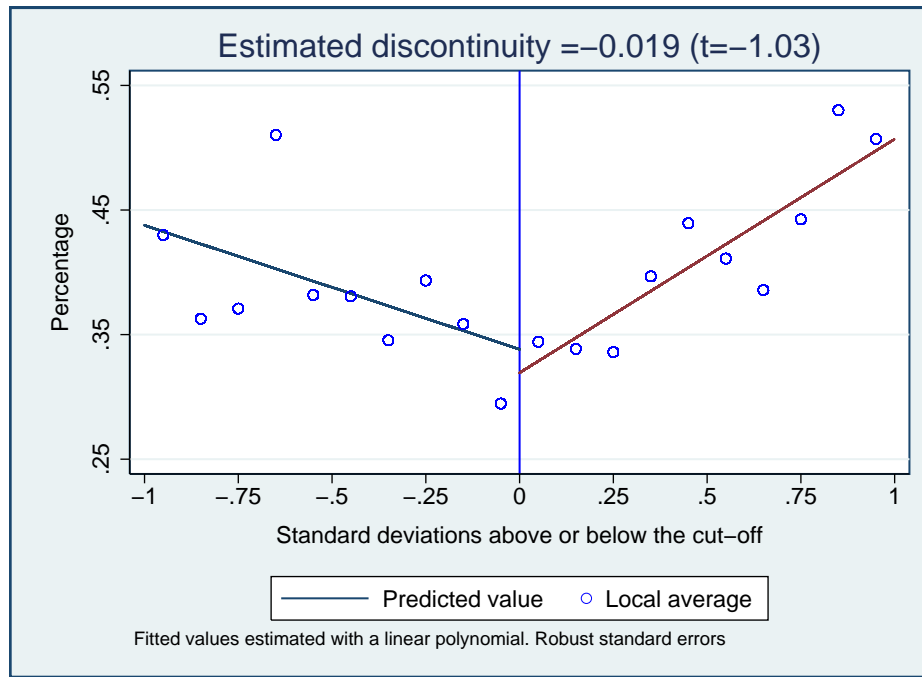


Figure A.8: Discontinuities of covariates at the selection threshold (corporation (binary))

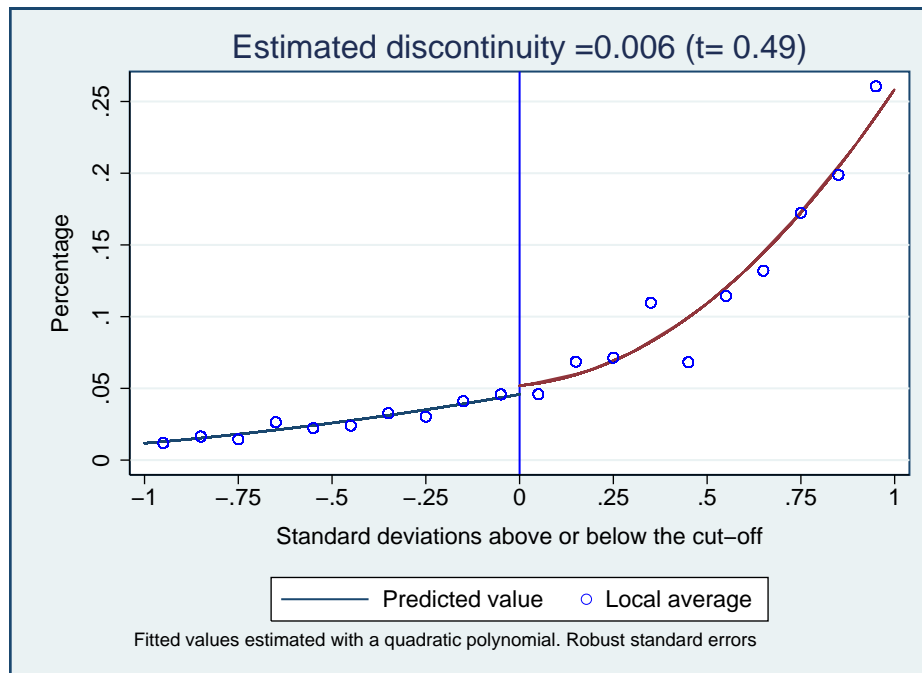


Figure A.9: Discontinuities of covariates at the selection threshold (special taxpayer (binary))

Note: Special taxpayers are those required to withhold taxes from other taxpayers

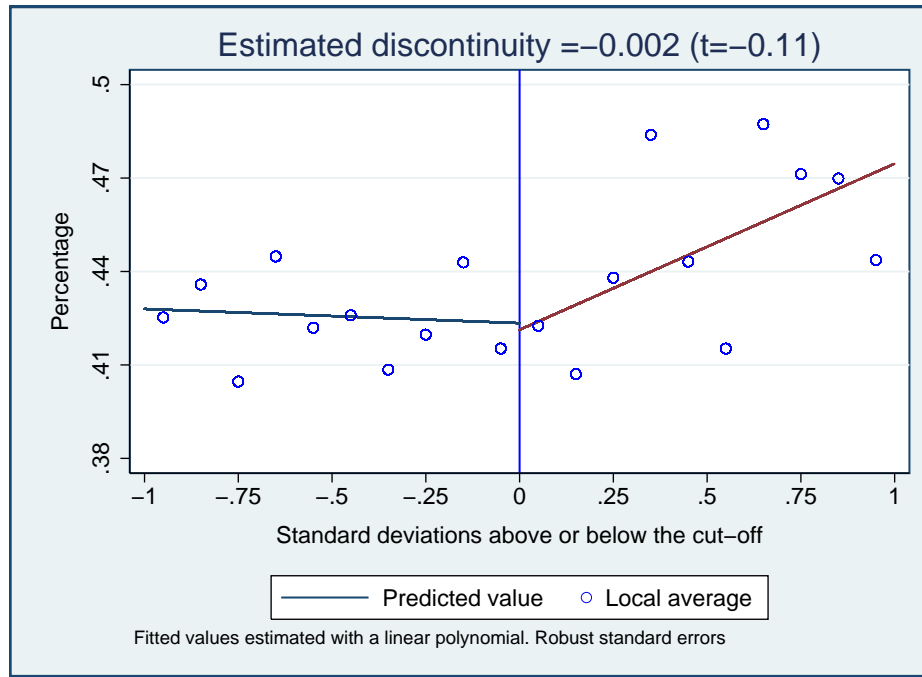


Figure A.10: Discontinuities of covariates at the selection threshold (legal representative college education (binary))

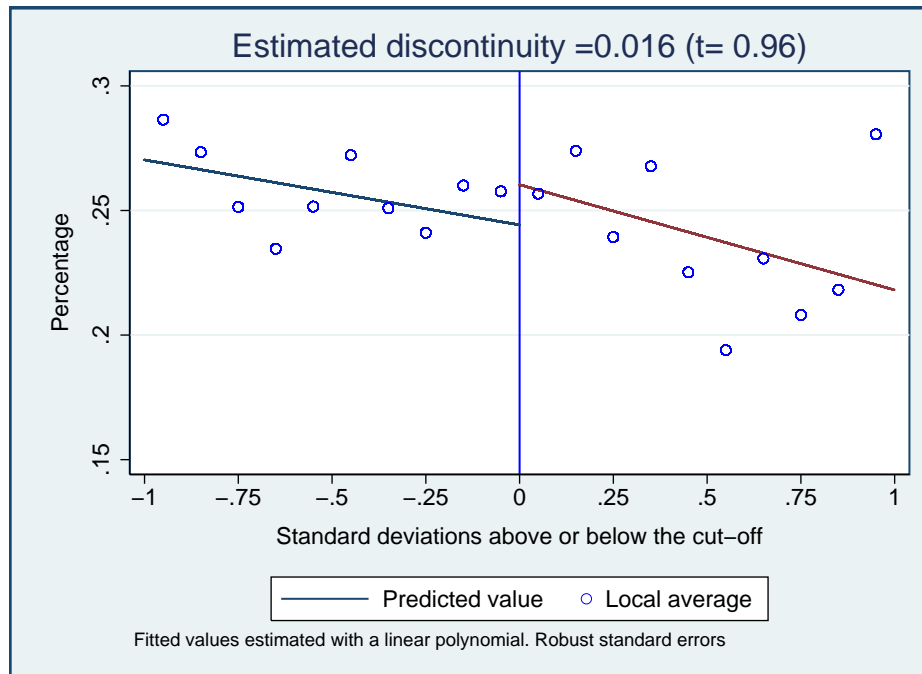


Figure A.11: Discontinuities of covariates at the selection threshold (female legal representative (binary))

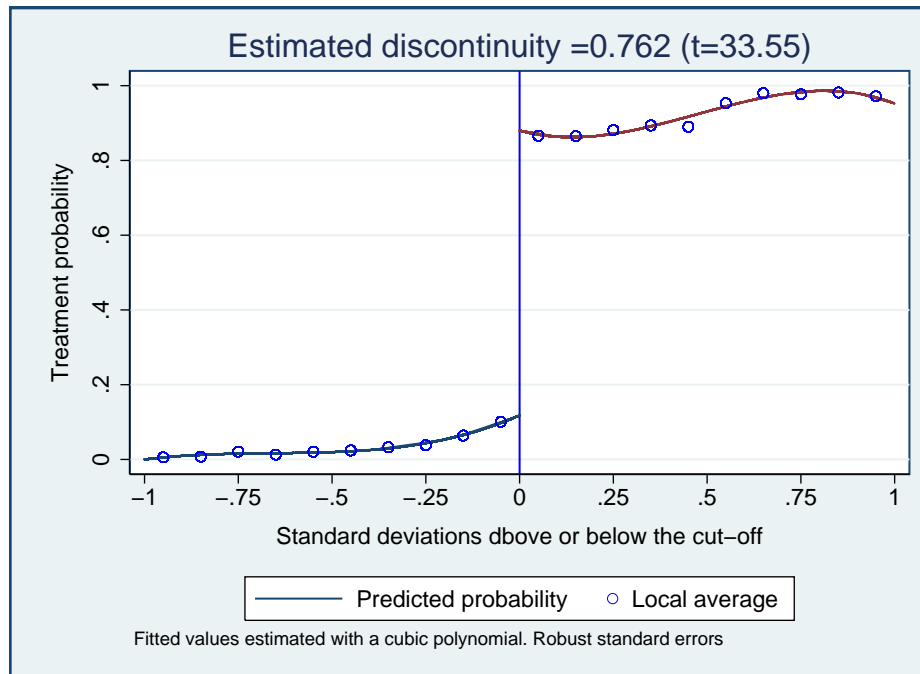


Figure A.12: Discontinuity in the probability of receiving the tax notification (First Stage)

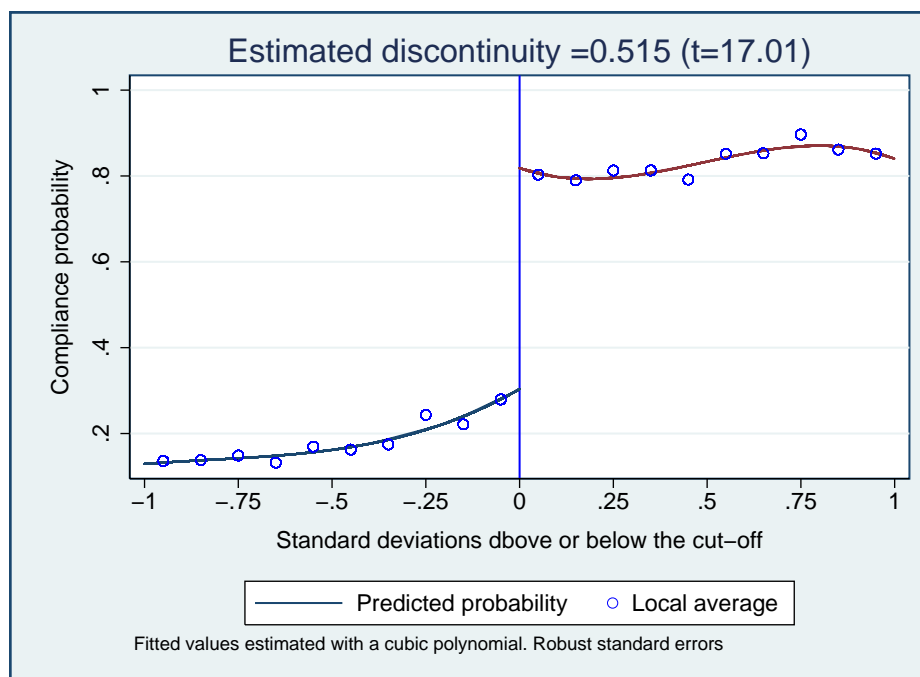


Figure A.13: Discontinuity in the probability of compliance

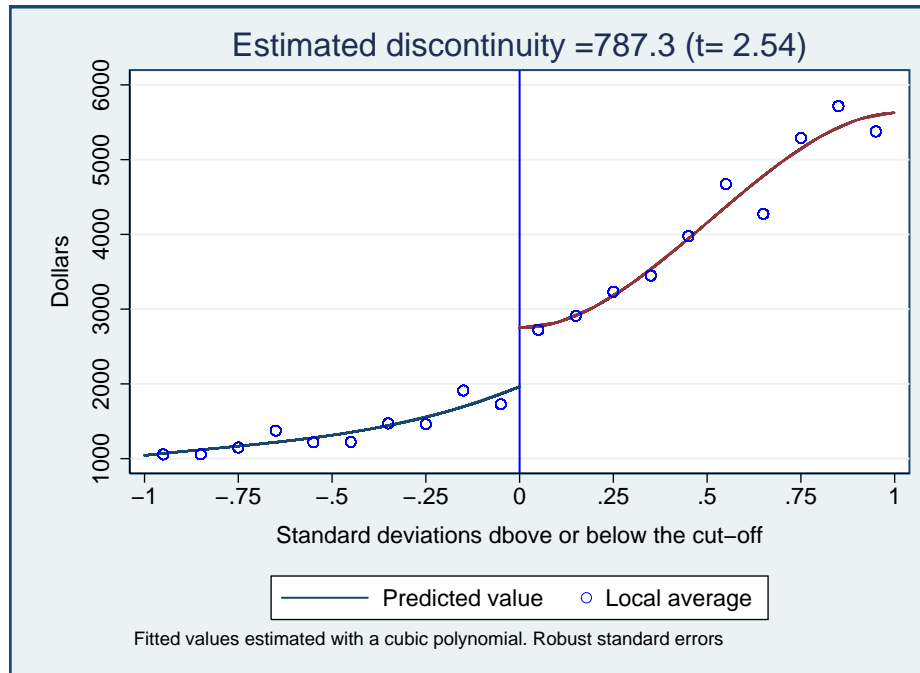


Figure A.14: Discontinuity in reported taxes (post-treatment ITA 2010)

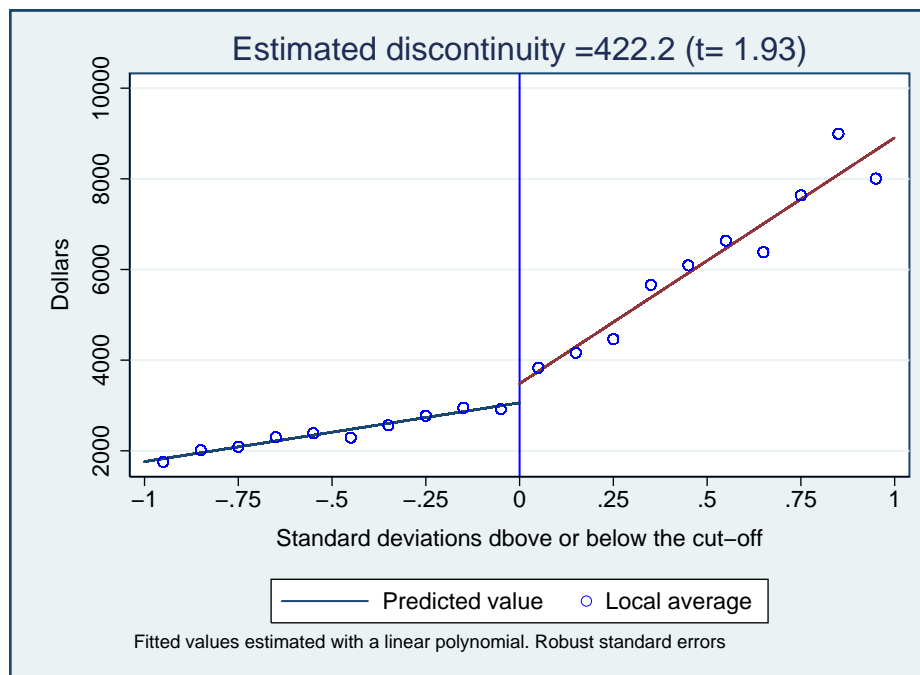


Figure A.15: Persistence effect (Discontinuity in reported taxes - ITA 2011)

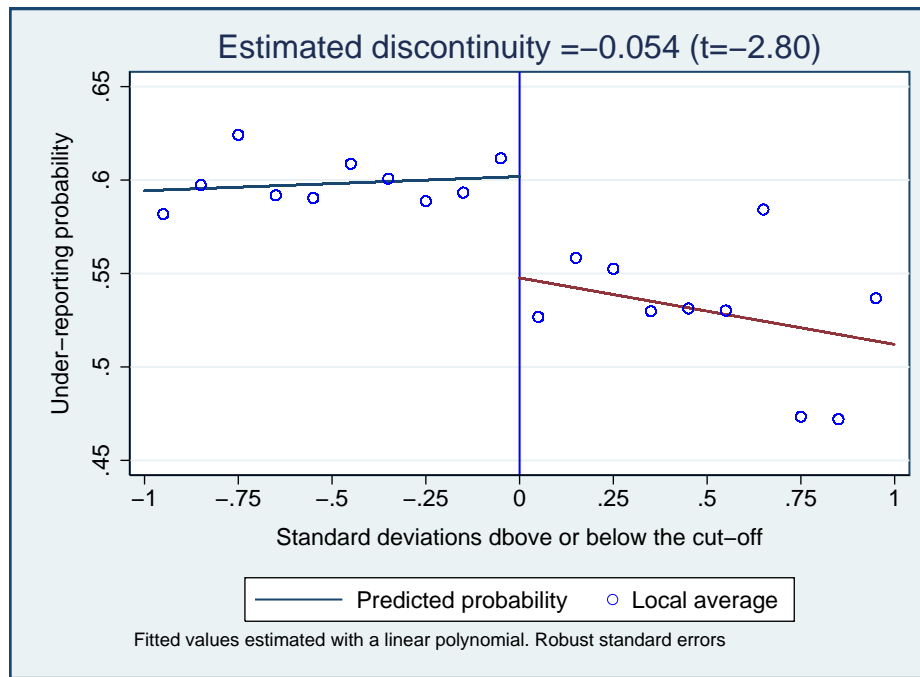


Figure A.16: Persistence effect (Discontinuity in the probability of under-reporting taxes - ITA 2011)

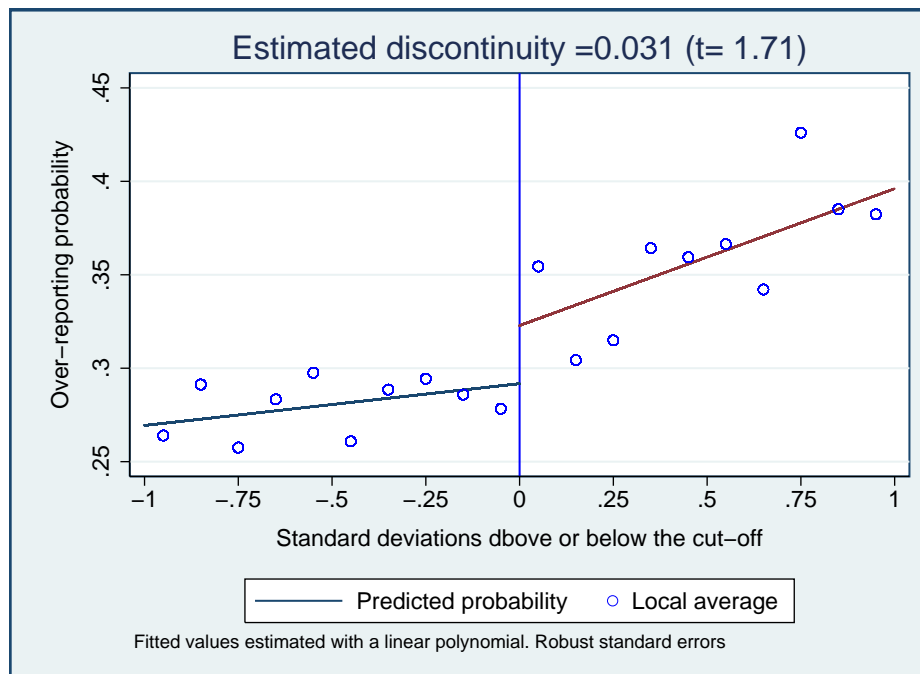


Figure A.17: Persistence effect (Discontinuity in the probability of over-reporting taxes - ITA 2011)

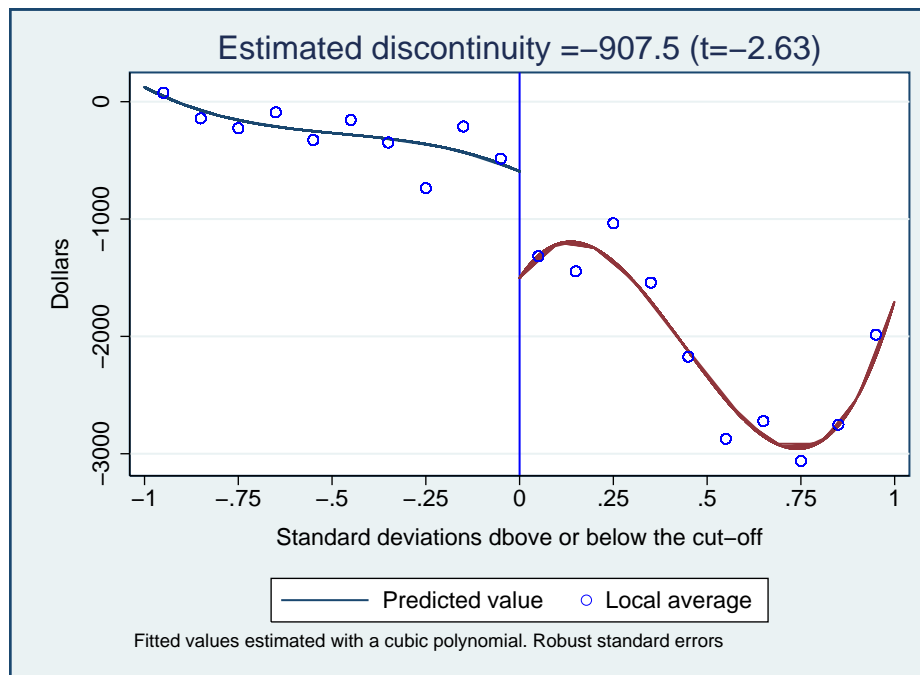


Figure A.18: Persistence effect (Discontinuity in the difference between under-reported ITA 2011 and 2010)

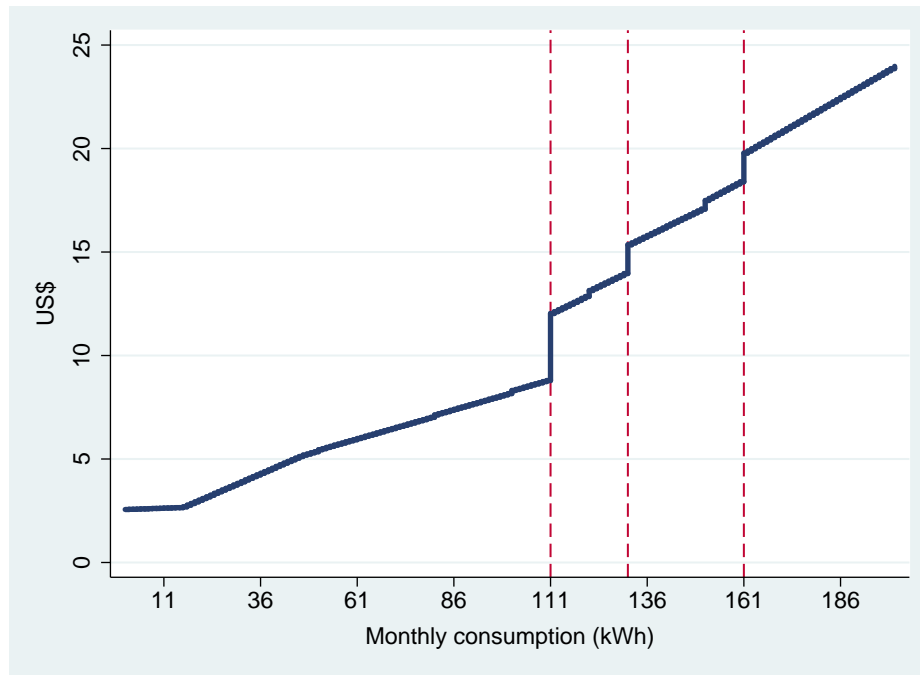


Figure A.19: Sample tariff function for EEQ residential customers. December 2013

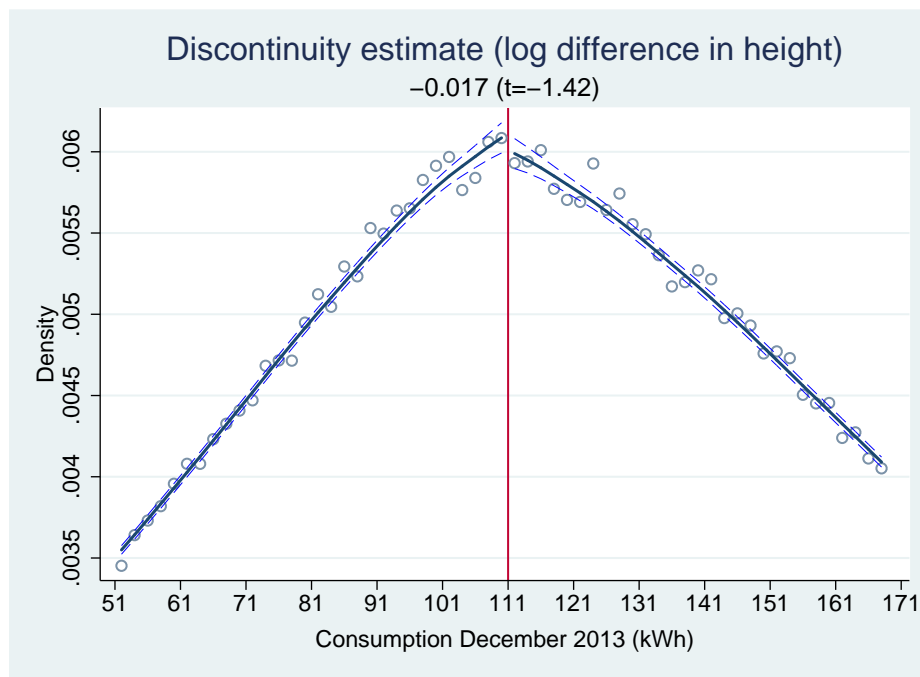


Figure A.20: Discontinuity in the density at the notch

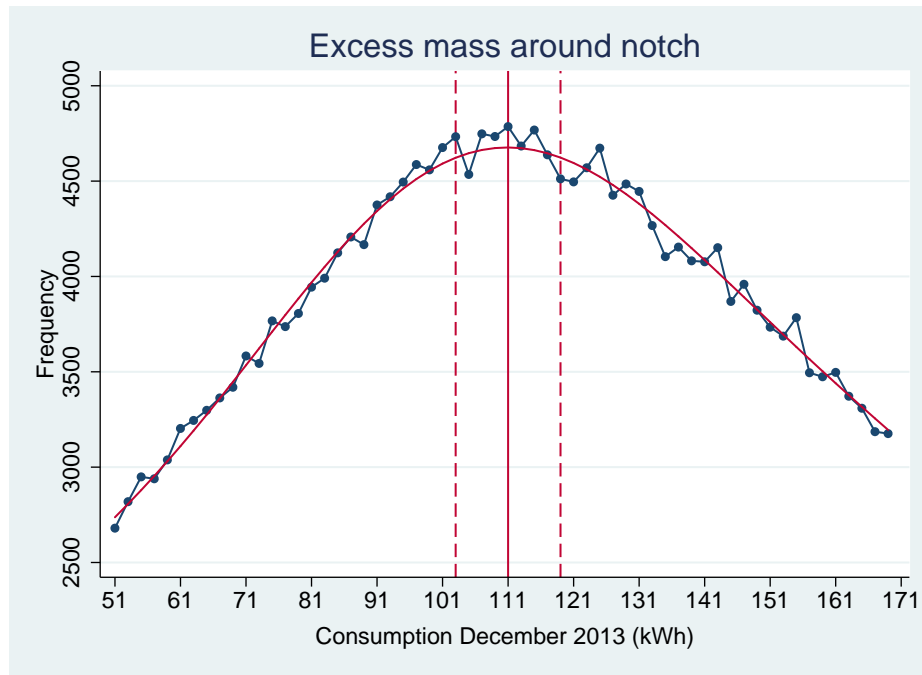


Figure A.21: Bunching around the notch

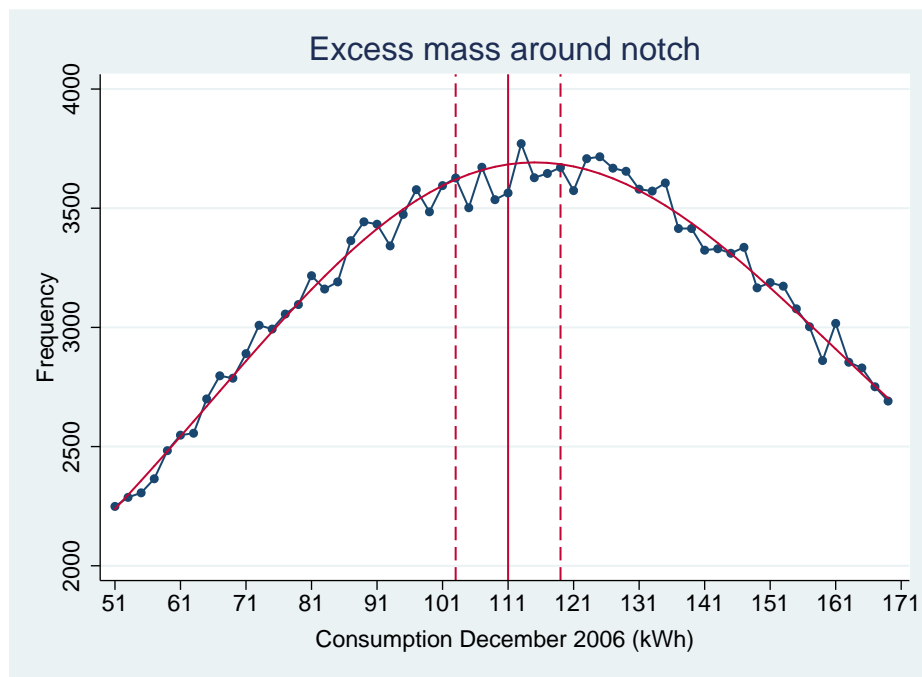


Figure A.22: Consumption distribution prior to notch's creation

SUMINISTRO: XXXXXX-X

Plan/Geocódigo: XX XX-XX-X-XX



INFORMACIÓN IMPORTANTE

Ahorre Electricidad y Ahorre Dinero

Estimado Cliente:

La siguiente información de su consumo mensual de electricidad durante el año pasado puede ser de su interés.

Su consumo promedio mensual fue aproximadamente: XXX kWh

Un hogar similar al suyo consume en promedio: 110 kWh

Esto significa que, durante el año pasado usted consumió aproximadamente **X,XX % más** que otros hogares similares. Le exhortamos a que haga un uso eficiente de la energía para ahorrar dinero.

Por favor lea con atención los siguientes consejos sobre ahorro de energía eléctrica para que empiece a ahorrar dinero ya! Comparta esta información con los demás miembros del hogar.

- No deje la puerta del refrigerador abierta por mucho tiempo y asegúrese que la puerta cierre herméticamente.
- No deje el televisor encendido si nadie lo mira.
- No olvide apagar las luces al salir de una habitación.

¡AHORRE ELECTRICIDAD, AHORRE DINERO!

Figure A.24: Sample letter for social comparison (Spanish)

Meter ID:

XXXXXX-X

Geocode: XX XX-XX-X-XX



IMPORTANT INFORMATION

Save Electricity and Save Money

Dear Customer:

We thought that you might be interested in the following information regarding your monthly electricity use over the past year.

Your average monthly consumption was: **XXX kWh**

The average household like you consumes: **110 kWh**

Over the past year, this means that you have consumed approximately **X,XX % more** electricity per month than others like you. We encourage you to use energy wisely to save money.

Please read carefully the following savings tips so you can learn how to save right away. Share this information with all the other members of the household.

- Don't leave the refrigerator door open for too long and make sure it closes tightly.
- Turn off the television if nobody is watching it.
- Don't forget to turn off the lights when leaving a room.

¡SAVE ELECTRICITY, SAVE MONEY!

Figure A.25: Sample letter for social comparison (translation)

SUMINISTRO:

XXXXXXX

Plan/Geocódigo: XX XX-XX-X-XXXX



INFORMACIÓN IMPORTANTE

Ahorre Electricidad y Ahorre Dinero

Estimado Cliente:

La tarifa eléctrica establecida por el CONELEC funciona de manera progresiva. Esto implica que si usted consume más de 110 kWh al mes, hay un incremento importante de costo en su factura.

La siguiente información de su consumo mensual de electricidad durante el año pasado puede ser de su interés.

Su consumo promedio mensual fue aproximadamente: XXX kWh

Esto significa que, durante el año pasado usted pagó en promedio alrededor de **US\$ XX** por consumo de electricidad cada mes (**US\$ XXX** al año). Si usted reduce su consumo mensual de electricidad en **X kWh** (alrededor de **X,XX %** de su consumo promedio), su pago mensual se reduciría cerca de **XX,XX %**, por lo cual **usted ahorraría US\$ XX al año**. Le exhortamos a que haga un uso eficiente de energía para ahorrar dinero.

Por favor lea con atención los siguientes consejos sobre ahorro de energía eléctrica para que empiece a ahorrar dinero ya! Comparta esta información con los demás miembros del hogar.

- No deje la puerta del refrigerador abierta por mucho tiempo y asegúrese que la puerta cierre herméticamente.
- No deje el televisor encendido si nadie lo mira.
- No olvide apagar las luces al salir de una habitación.

¡AHORRE ELECTRICIDAD, AHORRE DINERO!

Figure A.26: Sample letter for price notch salience (Spanish)

Meter ID:

XXXXXXX

Geocode: XX XX-XX-X-XXXX



IMPORTANT INFORMATION

Save Electricity and Save Money

Dear Customer:

The electric tariff established by CONELEC is progressive. What this means for you is that there is a large increase in your monthly bill should you consume more than 110 kWh.

We thought that you might be interested in the following information regarding your monthly electricity use over the past year.

Your average monthly consumption was:

XXX kWh

Over the past year, this means that you have paid around **US\$ XX** a month for the electricity you use (**US\$ XXX** per year). If you were to reduce your electricity use by **X kWh** per month (around **X,XX %** of your current consumption), you would reduce your monthly energy bill by nearly **XX,XX %** and would **save approximately US\$ XX per year**. We encourage you to use energy wisely to save money.

Please read carefully the following savings tips so you can learn how to save right away. Share this information with all the other members of the household.

- Don't leave the refrigerator door open for too long and make sure it closes tightly.
- Turn off the television if nobody is watching it.
- Don't forget to turn off the lights when leaving a room.

¡SAVE ELECTRICITY, SAVE MONEY!

Figure A.27: Sample letter for price notch salience (translation)

SUMINISTRO: XXXXX-X

Plan/Geocódigo: XX XX-XX-X-XXX



INFORMACIÓN IMPORTANTE

Ahorre Electricidad y Ahorre Dinero

Estimado Cliente:

La tarifa eléctrica establecida por el CONELEC funciona de manera progresiva. Esto implica que si usted consume más de 110 kWh al mes, hay un incremento importante de costo en su factura.

La siguiente información de su consumo mensual de electricidad durante el año pasado puede ser de su interés.

Su consumo promedio mensual fue aproximadamente: XXX kWh

Un hogar similar al suyo consume en promedio: 110 kWh

Esto significa que, durante el año pasado usted pagó en promedio alrededor de **US\$ XX** por consumo de electricidad cada mes (**US\$ XXX** al año). Si usted reduce su consumo mensual de electricidad en **X kWh** (alrededor de **X,XX %** de su consumo promedio), su pago mensual se reduciría cerca de **XX,XX %**, con lo cual **ahorraría US\$ XX al año**. Le exhortamos a que haga un uso eficiente de electricidad para ahorrar dinero.

Por favor lea con atención los siguientes consejos sobre ahorro de energía eléctrica para que empiece a ahorrar dinero ya! Comparta esta información con los demás miembros del hogar.

- No deje la puerta del refrigerador abierta por mucho tiempo y asegúrese que la puerta cierre herméticamente.
- No deje el televisor encendido si nadie lo mira.
- No olvide apagar las luces al salir de una habitación.

¡AHORRE ELECTRICIDAD. AHORRE DINERO!

Figure A.28: Sample letter for social comparison and price salience combined (Spanish)

Meter ID:

XXXXXXX

Geocode: XX XX-XX-X-XXXX



IMPORTANT INFORMATION

Save Electricity and Save Money

Dear Customer:

The electric tariff established by CONELEC is progressive. What this means for you is that there is a large increase in your monthly bill should you consume more than 110 kWh.

We thought that you might be interested in the following information regarding your monthly electricity use over the past year.

Your average monthly consumption was: XXX kWh

The average household like you consumes: 110 kWh

Over the past year, this means that you have paid around **US\$ XX** a month for the electricity you use (**US\$ XXX** per year). If you were to reduce your electricity use by **X kWh** per month (around **X,XX %** of your current consumption), you would reduce your monthly energy bill by nearly **XX,XX %** and would **save approximately US\$ XX per year**. We encourage you to use energy wisely to save money.

Please read carefully the following savings tips so you can learn how to save right away. Share this information with all the other members of the household.

- Don't leave the refrigerator door open for too long and make sure it closes tightly.
- Turn off the television if nobody is watching it.
- Don't forget to turn off the lights when leaving a room.

¡SAVE ELECTRICITY, SAVE MONEY!

Figure A.29: Sample letter for social comparison and price salience combined (Translation)

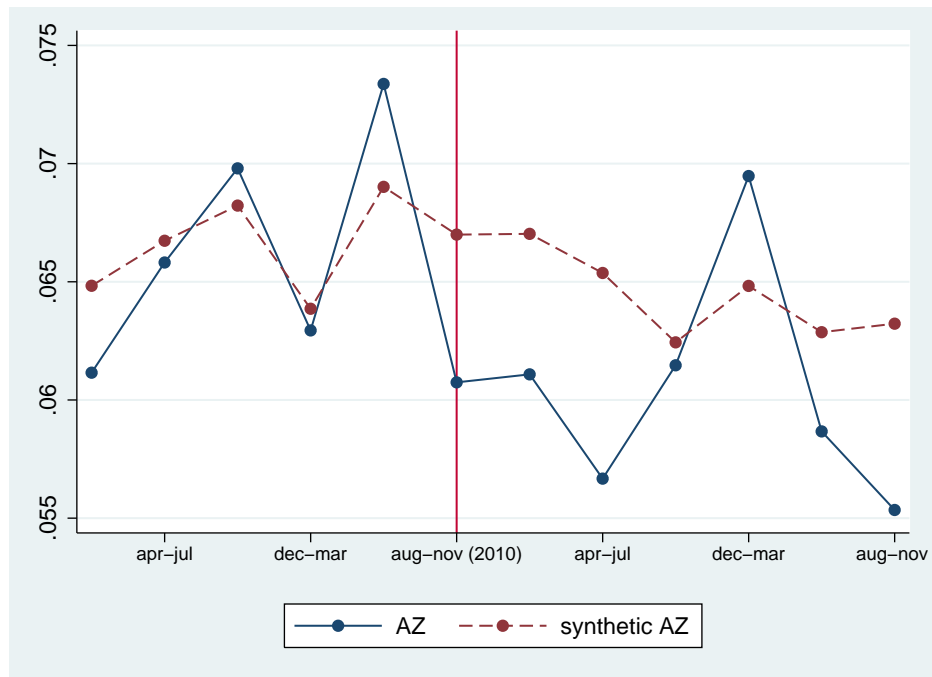


Figure A.30: Proportion of noncitizen Hispanic in Arizona and synthetic control

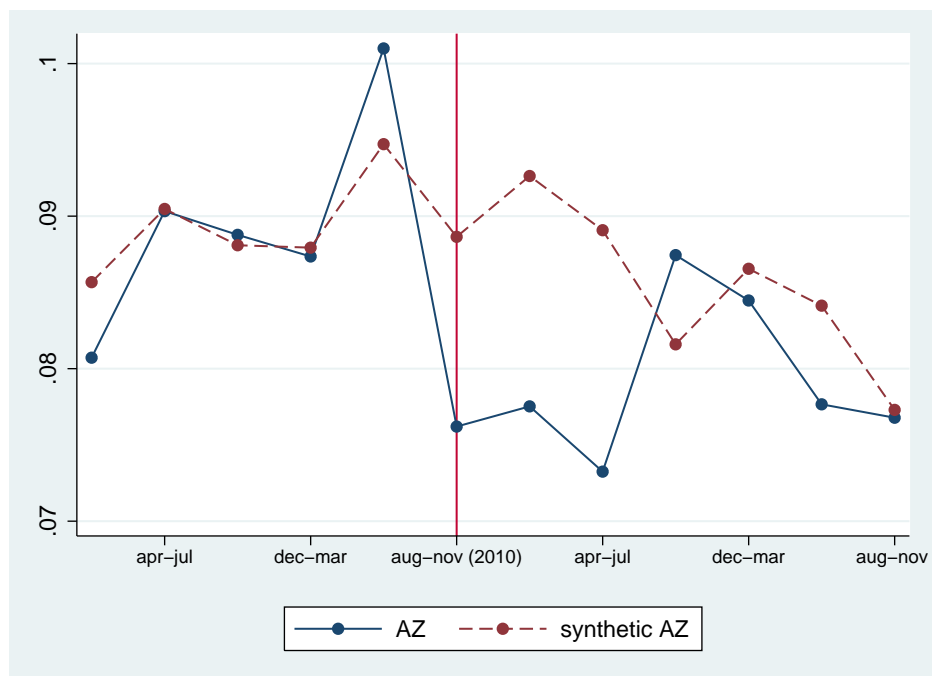


Figure A.31: Proportion of noncitizen Hispanic between 15 and 45 years of age with high school diploma or Less in Arizona and synthetic control

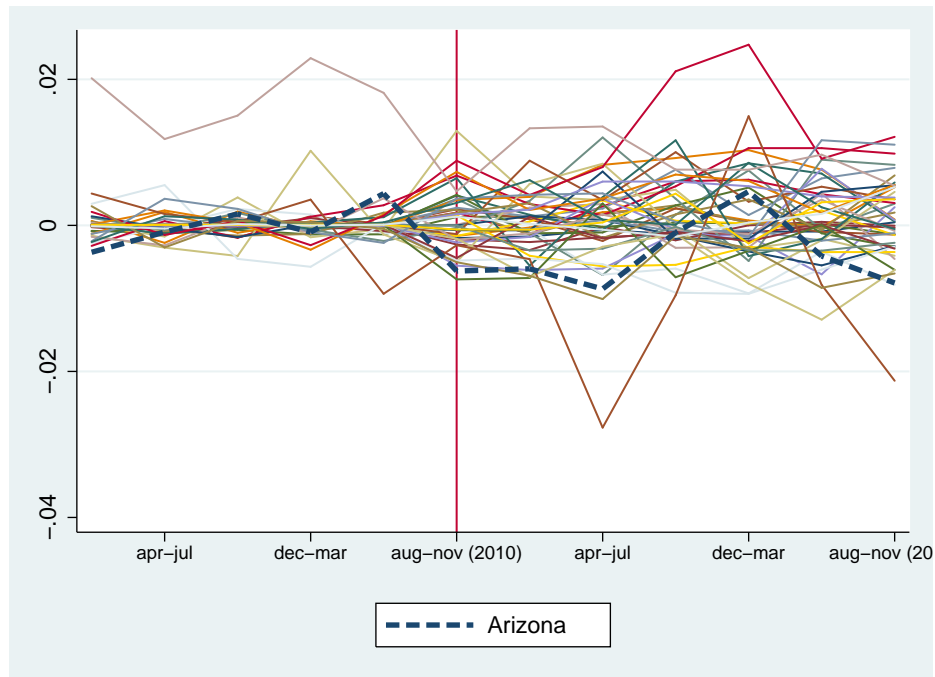


Figure A.32: Difference in the proportion of noncitizen Hispanic relative to the synthetic Control - 46 states

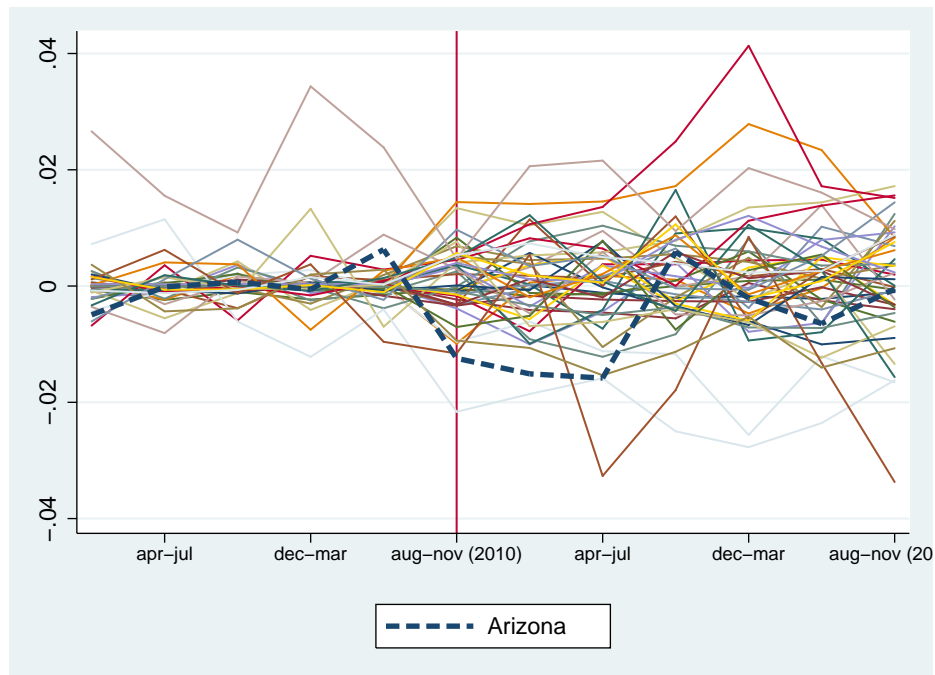


Figure A.33: Difference in the proportion of noncitizen Hispanic between 15 and 46 years of age with high school diploma or less relative to the synthetic control - 46 states

APPENDIX B

TABLES

Table B.1: Examples of application of the income tax advance (ITA)

Example	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
<i>ITA filed in 2010</i>			
(F) Greater btw A and D	120	100	100
(G) Taxes Due (F-C-E)	40	20	-15

Source: Author calculations and SRI.

Note: This table 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 B.2: Frequencies by province

Province	Under-reported ITA 2010		Selected to receive the tax notification	Received the tax notification	Corrected or justified differences
	(Count)	(Percent)			
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 B.3: Summary statistics

	Mean	Standard deviation
<i>Panel A. Outcome variables</i>		
Corrected or justified 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
<i>Panel B. Covariates</i>		
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 B.4: Regression discontinuity estimates of the discontinuity in the probability of receiving the tax notification (first stage)

		Discontinuity	
		(1)	(2)
<i>Panel A. Bandwidth: 1 std. dev.</i>			
	Cubic ^a	0.762*** (0.023)	0.750*** (0.019)
	Quadratic	0.755*** (0.017)	0.762*** (0.014)
	Linear	0.779*** (0.011)	0.804*** (0.009)
Controls		No	Yes
Observations		14,340	14,260
<i>Panel B. Bandwidth: 0.5 std. dev.</i>			
	Cubic	0.752*** (0.032)	0.761*** (0.026)
	Quadratic ^a	0.746*** (0.025)	0.732*** (0.020)
	Linear	0.766*** (0.016)	0.773*** (0.013)
Controls		No	Yes
Observations		6,084	6,052
<i>Panel C. Bandwidth: 0.25 std. dev.</i>			
	Quadratic ^a	0.771*** (0.034)	0.786*** (0.028)
	Linear	0.738*** (0.023)	0.745*** (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 bandwidth selected using the Akaike Information Criterion (AIC) calculated for regressions that include additional covariates.

Source: Author calculations and SRI.

Table B.5: 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)
<i>Panel A. Bandwidth: 1 std. dev.</i>				
Cubic polynomial ^a	0.515*** (0.030)	0.505*** (0.028)	0.675*** (0.032)	0.673*** (0.032)
Quadratic polynomial	0.505*** (0.023)	0.520*** (0.021)	0.669*** (0.024)	0.683*** (0.024)
Linear polynomial	0.541*** (0.015)	0.563*** (0.014)	0.694*** (0.016)	0.700*** (0.015)
Controls	No	Yes	No	Yes
Observations	14,340	14,260	14,340	14,260
<i>Panel B. Bandwidth: 0.5 std. dev.</i>				
Cubic polynomial	0.509*** (0.043)	0.521*** (0.039)	0.676*** (0.046)	0.685*** (0.046)
Quadratic polynomial ^a	0.494*** (0.033)	0.483*** (0.030)	0.663*** (0.035)	0.660*** (0.036)
Linear polynomial	0.520*** (0.022)	0.533*** (0.019)	0.678*** (0.023)	0.689*** (0.022)
Controls	No	Yes	No	Yes
Observations	6,084	6,052	6,084	6,052
<i>Panel C. Bandwidth: 0.25 std. dev.</i>				
Quadratic polynomial	0.485*** (0.046)	0.511*** (0.042)	0.629*** (0.049)	0.650*** (0.047)
Linear polynomial ^a	0.496*** (0.031)	0.510*** (0.028)	0.672*** (0.034)	0.685*** (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 bandwidth selected using the Akaike Information Criterion (AIC) calculated for the reduced form regressions that include additional covariates.

Source: Author calculations and SRI.

Table B.6: 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)
<i>Panel A. Bandwidth: 1 std. dev.</i>				
Cubic polynomial ^a	787.28** (309.88)	1,013.75*** (200.05)	1,036.75** (405.46)	1,356.11*** (269.28)
Quadratic polynomial	637.57*** (239.10)	696.00*** (158.46)	845.51*** (315.19)	913.85*** (208.34)
Linear polynomial	698.38*** (169.47)	835.01*** (119.48)	896.28*** (216.05)	1,038.20*** (148.69)
Controls	No	Yes	No	Yes
Observations	14,061	13,982	14,061	13,982
<i>Panel B. Bandwidth: 0.5 std. dev.</i>				
Cubic polynomial ^a	1,173.22*** (425.81)	1,406.41*** (260.25)	1,563.56*** (564.00)	1,859.55*** (350.20)
Quadratic polynomial	820.33** (321.51)	923.60*** (201.31)	1,105.33** (430.04)	1,267.52*** (277.75)
Linear polynomial	623.39*** (220.08)	741.50*** (142.21)	814.14*** (285.39)	959.79*** (184.16)
Controls	No	Yes	No	Yes
Observations	5,995	5,963	5,995	5,963
<i>Panel C. Bandwidth: 0.25 std. dev.</i>				
Quadratic polynomial	1,343.79*** (450.28)	1,367.99*** (278.06)	1,749.16*** (582.91)	1,753.07*** (361.80)
Linear polynomial ^a	837.21*** (292.71)	1,072.55*** (189.44)	1,138.16*** (394.89)	1,443.01*** (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 bandwidth selected using the Akaike Information Criterion (AIC) calculated for the reduced form regressions that include additional covariates.

Source: Author calculations and SRI.

Table B.7: 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)
<i>Panel A. Bandwidth: 1 std. dev.</i>				
Cubic polynomial	477.61 (392.55)	653.87*** (251.29)	626.47 (512.98)	866.29*** (333.76)
Quadratic polynomial	439.10 (307.50)	399.02** (197.11)	580.91 (405.34)	520.77** (257.34)
Linear polynomial ^a	422.17* (218.39)	539.15*** (143.85)	541.83* (279.28)	668.39*** (178.43)
Controls	No	Yes	No	Yes
Observations	13,697	13,622	13,697	13,622
<i>Panel B. Bandwidth: 0.5 std. dev.</i>				
Cubic polynomial	638.25 (535.26)	665.25** (320.62)	847.87 (708.15)	870.84** (421.44)
Quadratic polynomial	679.62* (406.26)	607.99** (252.19)	913.60* (543.26)	825.86** (343.40)
Linear polynomial ^a	310.51 (283.07)	394.30** (175.58)	404.60 (367.76)	506.56** (225.66)
Controls	No	Yes	No	Yes
Observations	5,854	5,824	5,854	5,824
<i>Panel C. Bandwidth: 0.25 std. dev.</i>				
Quadratic polynomial	771.02 (569.90)	464.28 (333.88)	995.74 (732.90)	587.39 (423.41)
Linear polynomial ^a	456.60 (375.46)	575.37** (235.39)	619.88 (507.75)	767.99** (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 bandwidth selected using the Akaike Information Criterion (AIC) calculated for the reduced form regressions that include additional covariates. Source: Author calculations and SRI.

Table B.8: 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
<i>Panel A. Dependent variable:</i> <i>Reported post-treatment ITA 2010 - US\$</i>			
Probability of attrition	0.012 (0.013)	0.012 (0.018)	0.003 (0.013)
RDD estimate	1,356.11*** (269.28)	1,859.55*** (350.20)	1,443.01*** (256.12)
Lower bound	1,339.78 (273.90)	1,822.29 (344.22)	1,428.21 (264.72)
Upper bound	1,493.59 (326.31)	2,022.95 (374.14)	1,511.14 (272.11)
Order of polynomial	Cubic	Cubic	Linear
Observations	14,061	5,963	3,059
<i>Panel B. Dependent Variable: Reported ITA 2011 - US\$</i>			
Probability of attrition	0.001 (0.009)	0.002 (0.013)	-0.007 (0.019)
RDD estimate	668.39*** (178.43)	506.56** (225.66)	767.99** (314.69)
Lower bound	660.00 (181.25)	492.54 (229.58)	769.10 (323.73)
Upper bound	709.77 (204.32)	604.59 (242.83)	867.80 (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 B.9: Regression discontinuity estimates of the persistence of the effect (LATE) of the tax notification

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

Table B.10: Summary statistics: Monthly electricity consumption 2013 (kWh)

Month	Mean	Standard deviation	Median
January	153.0	129.9	130
February	142.2	119.7	122
March	136.8	116.9	117
April	144.1	122.9	123
May	151.1	127.6	130
June	147.9	125.4	127
July	138.9	189.1	118
August	149.2	125.0	128
September	144.8	123.6	124
October	144.9	122.0	124
November	147.4	124.3	126
December	148.3	127.2	126

Source: Author calculations and EEQ.

Statistics correspond to the Quito Metropolitan District.

Table B.11: Monthly electricity use for EEQ households

End Use	Average usage (kWh per month)	Share of total
Refrigerator	39.8	37.7%
Other appliances	12.8	12.1%
Television	12.7	12.0%
Lighting	9.4	8.9%
Washing machine	8.8	8.3%
Water heater	8.0	7.6%
Ironing	6.6	6.5%
Cooking	4.0	3.8%
Music Eelectronics	2.8	2.7%
Heating	0.7	0.7%

Source: ENERINTER Asesoría Energética Internacional, 2012.

Data for EEQ households with monthly average usage between 99 and 110 kWh

Table B.12: Pre-treatment: Household yearly average in 2013 monthly consumption (kWh)

Group	Count	Average	Median	standard deviation
<i>Panel A. All sample</i>				
Control	15,875	112.35	112.33	7.22
Social comparison	15,854	112.30	112.17	7.21
Price notch salience	15,860	112.35	112.25	7.22
Both	15,853	112.32	112.17	7.17
<i>Panel B. Above the notch</i>				
Control	9,425	117.42	117.42	4.34
Social comparison	9,359	117.37	117.33	4.37
Price notch salience	9,406	117.42	117.42	4.35
Both	9,381	117.35	117.33	4.38
<i>Panel C. Below the notch</i>				
Control	6,450	104.95	104.92	2.92
Social comparison	6,495	104.98	105.00	2.92
Price notch salience	6,454	104.96	105.00	2.92
Both	6,472	105.02	105.08	2.88

Notes: This table reports summary statistics of the pretreatment variable – average 2013 monthly consumption – for each household in the treatment groups. The notch that defines Panels B and C is 110 kWh/month as explained in the text.

Table B.13: Pre-treatment: Test of differences in household average 2013 monthly consumption across groups

	Difference	Difference (%)	Standard error	p-value
<i>Panel A. All sample</i>				
Social comparison vs. Control	-0.057	-0.05%	0.081	0.49
Price notch salience vs. Control	-0.007	-0.01%	0.081	0.94
Both vs. Control	-0.036	-0.03%	0.081	0.65
Price notch salience vs. Social comparison	0.050	0.04%	0.081	0.54
Both vs. Social comparison	0.020	0.02%	0.081	0.80
Both vs. Price notch salience	0.030	0.03%	0.081	0.71
<i>Panel B. Above the notch</i>				
Social comparison vs. Control	-0.048	-0.04%	0.064	0.45
Price notch salience vs. Control	-0.007	-0.01%	0.063	0.91
Both vs. Control	-0.071	-0.06%	0.064	0.26
Price notch salience vs. Social comparison	0.041	0.04%	0.064	0.52
Both vs. Social comparison	-0.023	-0.02%	0.064	0.72
Both vs. Price notch salience	0.064	0.05%	0.064	0.32
<i>Panel C. Below the notch</i>				
Social comparison vs. Control	0.035	0.03%	0.051	0.50
Price notch salience vs. Control	0.013	0.01%	0.051	0.79
Both vs. Control	0.073	0.07%	0.051	0.15
Price notch salience vs. Social comparison	-0.021	-0.02%	0.051	0.68
Both vs. Social comparison	0.039	0.04%	0.051	0.45
Both vs. Price notch salience	-0.060	-0.06%	0.051	0.24

Notes: This table reports tests of differences in means of the pretreatment 2013 average monthly consumption across the households in each of the treatment groups. We report p-values of tests of the null hypothesis that the means are equal for each pairwise comparison. Standard errors are robust. The notch that defines Panels B and C is 110 kWh/month as explained in the text.

Table B.14: Estimates of average treatment effects for households above the notch.
First quarter after the intervention

	(1)	(2)	(3)	(4)
Social comparison	-1.362** (0.599)	-1.247*** (0.461)	-1.226** (0.603)	-1.219** (0.603)
Price notch salience	-0.378 (0.612)	-0.426 (0.469)	-0.213 (0.613)	-0.206 (0.613)
Both	-0.760 (0.607)	-1.064** (0.464)	-0.710 (0.606)	-0.710 (0.606)
Constant	121.986*** (0.438)	- -	- -	- -
Pre-treatment consumption	No	Yes	No	No
Post-treatment indicator	No	No	Yes	No
Year-by-month fixed effects	Yes	Yes	No	Yes
Houshold fixed effects	No	No	No	Yes
N	110,242	110,242	1,018,486	1,018,486

Notes: The dependent variable is a “month” of consumption, which is the average daily consumption during the meter-read window multiplied by 365/12. The sample is all households who had pre-treatment average annual consumption above 110 kWh. Specifications (1) and (2) use post-treatment observations for the period April-June 2014 in a cross sectional setting. Specifications (3) and (4) use a panel setting with data ranging from February 2012 to June 2014. Pre-treatment consumption is the monthly consumption for the periods April-June, 2013 and January-February, 2014, see text for details. Robust standard errors clustered at the household level in parentheses; * p<0.1; ** p<0.05; *** p<0.01.

Table B.15: Estimates of average treatment effects for households above the notch.
First quarter after the intervention

	(1)	(2)	(3)	(4)
<i>Panel A. 111-115 kWh</i>				
Social comparison	-1.555 (0.970)	-1.266* (0.739)	-1.104 (0.985)	-1.121 (0.985)
Price notch salience	-0.935 (0.970)	-1.521* (0.777)	-0.606 (0.988)	-0.596 (0.988)
Both	-0.630 (1.003)	-1.174 (0.756)	-0.342 (1.007)	-0.345 (1.008)
Constant	117.854*** (0.713)	- -	- -	- -
N	41,230	41,230	380,893	380,893
<i>Panel B. 116-125 kWh</i>				
Social comparison	-1.175 (0.759)	-1.236** (0.590)	-1.307* (0.764)	-1.287* (0.763)
Price notch salience	-0.076 (0.783)	0.202 (0.588)	0.014 (0.780)	0.024 (0.780)
Both	-0.723 (0.758)	-0.976* (0.586)	-0.952 (0.758)	-0.950 (0.757)
Constant	124.414*** (0.552)	- -	- -	- -
N	69,012	69,012	637,593	637,593
Pre-treatment consumption	No	Yes	No	No
Post-treatment indicator	No	No	Yes	No
Year-by-month fixed effects	Yes	Yes	No	Yes
Houshold fixed effects	No	No	No	Yes

Notes: The dependent variable is a “month” of consumption, which is the average daily consumption during the meter-read window multiplied by 365/12. The sample is all households who had pre-treatment average annual consumption above 110 kWh. Specifications (1) and (2) use post-treatment observations for the period April-June 2014 in a cross sectional setting. Specifications (3) and (4) use a panel setting with data ranging from February 2012 to June 2014. Pre-treatment consumption is the monthly consumption for the periods April-June, 2013 and January-February, 2014, see text for details. Robust standard errors clustered at the household level in parentheses; * p<0.1; ** p<0.05; *** p<0.01.

Table B.16: Estimates of average treatment effects for households below the notch.
First quarter after the intervention

	(1)	(2)	(3)	(4)
Social comparison	0.371 (0.711)	-0.607 (0.564)	0.160 (0.714)	0.159 (0.713)
Price notch salience	0.372 (0.725)	-0.192 (0.554)	0.026 (0.721)	0.026 (0.721)
Both	-0.102 (0.706)	-0.664 (0.557)	-0.214 (0.714)	-0.190 (0.713)
Constant	110.352*** (0.520)	- -	- -	- -
Pre-treatment consumption	No	Yes	No	No
Post-treatment indicator	No	No	Yes	No
Year-by-month fixed effects	Yes	Yes	No	Yes
Houshold fixed effects	No	No	No	Yes
N	75,680	75,680	699,151	699,151

Notes: The dependent variable is a “month” of consumption, which is the average daily consumption during the meter-read window multiplied by 365/12. The sample is all households who had pre-treatment average annual consumption below 110 kWh. Specifications (1) and (2) use post-treatment observations for the period April-June 2014 in a cross sectional setting. Specifications (3) and (4) use a panel setting with data ranging from February 2012 to June 2014. Pre-treatment consumption is the monthly consumption for the periods April-June, 2013 and January-February, 2014, see text for details. Robust standard errors clustered at the household level in parentheses; * p<0.1; ** p<0.05; *** p<0.01.

Table B.17: Estimates of average treatment effects by quarter for households above the notch

	April-June	July-September	October-December
Social comparison	-1.216** (0.603)	-0.861 (0.657)	-0.428 (0.739)
Price notch salience	-0.200 (0.613)	-0.126 (0.663)	-0.212 (0.736)
Both	-0.707 (0.606)	-0.387 (0.658)	-0.569 (0.743)

Notes: The dependent variable is a “month” of consumption, which is the average daily consumption during the meter-read window multiplied by 365/12. The estimations are from panel data specifications with year-by-month and household fixed effects. Data ranges from February 2012 to December 2014. Robust standard errors clustered at the household level in parentheses; * p<0.1; ** p<0.05; *** p<0.01.

Table B.18: Proportion of Hispanic noncitizen population in Arizona and other states

	Hispanic noncitizens		Hispanic noncitizens between 15 and 45 years of age High school diploma or less	
	Arizona	Other states	Arizona	Other states
Dec-Mar	6.12%	4.07%	8.07%	5.49%
Apr-Jul	6.58%	4.08%	9.03%	5.55%
Aug-Nov (2009)	6.98%	4.17%	8.88%	5.54%
Dec-Mar	6.29%	3.96%	8.74%	5.36%
Apr-Jul	7.34%	4.15%	10.10%	5.67%
<i>Aug-Nov (2010)</i>	6.07%	4.10%	7.62%	5.57%
Dec-Mar	6.11%	3.96%	7.75%	5.36%
Apr-Jul	5.67%	3.92%	7.33%	5.39%
Aug-Nov (2011)	6.15%	3.96%	8.74%	5.35%
Dec-Mar	6.95%	3.92%	8.45%	5.20%
Apr-Jul	5.87%	3.87%	7.77%	5.29%
Aug-Nov (2012)	5.53%	3.96%	7.68%	5.34%

Source: Current Population Survey

Table B.19: States with positive weights in the synthetic control estimations

	Hispanic noncitizens	Hispanic noncitizens between 15 and 45 years of age High school diploma or less
Texas	40.3%	15.1%
Washington	24.4%	40.5%
California	23.3%	41.5%
Kentucky	12.1%	2.9%

Table B.20: Estimated impact of the introduction of SB 1070 on the proportion of noncitizen Hispanics among Arizona residents in the respective group

	All	High school diploma or Less (15-45)
<i>Panel A. Average differences relative to the synthetic control</i>		
1. Pre-treatment (2009 -Jul 2010)	0.000	0.000
2. Post-treatment (Aug 2010 - Jul 2011)	-0.007	-0.014
3. Post-treatment (Aug 2010 - Dec 2012)	-0.004	0.007
<i>Panel B. Diff-in-Diff. (A2-A1)</i>		
Difference post - pre	-0.007	-0.015
Rank lowest to highest effect	4	2
Equivalent p-value (one-tailed test)	0.087	0.043
<i>Panel C. Diff-in-Diff. (A3-A1)</i>		
Difference post - pre	-0.004	-0.007
Rank lowest to highest effect	7	6
Equivalent p-value (one-tailed test)	0.152	0.130

Notes: One-sided p-value calculated as the relative position of the estimate corresponding to Arizona within the placebo effects distribution.

Table B.21: Robustness checks. Estimated impact of the introduction of SB 1070 for groups non affected by the legislation

	Estimate	Rank	Equivalent p-value
<i>Panel A. Post treatment (Aug 2010 - Jul 2011)</i>			
Share of Hispanic naturalized citizens	-0.0010	16	0.35
Share of non-Hispanic non-citizens	-0.0020	15	0.33
Share of Hispanic born in the U.S.	-0.0007	16	0.35
<i>Panel B. Post treatment (Aug 2010 - Dec 2012)</i>			
Share of Hispanic naturalized citizens	0.0007	28	0.61
Share of non-Hispanic non-citizens	-0.0009	25	0.54
Share of Hispanic born in the U.S.	0.0005	23	0.50

Notes: This table shows difference-in-difference estimates calculated from the synthetic control calculation, see text for details. One-sided p-value calculated as the relative position of the estimate corresponding to Arizona within the placebo effects distribution.