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Abstract

This paper investigates if research findings change political leaders' beliefs and cause policy change. Collaborating with the National Confederation of Municipalities in Brazil, we work with 2,150 municipalities and the mayors who control their policies. We use experiments to measure mayors' demand for research information and their response to learning research findings. In one experiment, we find that mayors and other municipal officials are willing to pay to learn the results of impact evaluations, and update their beliefs when informed of the findings. They value larger-sample studies more, while not distinguishing on average between studies conducted in rich and poor countries. In a second experiment, we find that informing mayors about research on a simple and effective policy (reminder letters for taxpayers) increases the probability that their municipality implements the policy by 10 percentage points. In sum, we provide direct evidence that providing research information to political leaders can lead to policy change. Information frictions may thus help explain failures to adopt effective policies.

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How Research Affects Policy: Experimental Evidence from 2,150 Brazilian Municipalities*

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1 Introduction

Recent decades have seen an explosion of program evaluation research in economics.[1] But how interested in and open to academic research *are* political leaders? And, insofar as they "consume" research, can and do they act on new findings? These are questions of fundamental importance for the science ecosystem. Despite the money and effort devoted to evaluating policy impact, we have little understanding of whether the conditions necessary for the public to ultimately benefit hold: whether political leaders *value* such research; whether it *changes their beliefs* about policy effectiveness; and whether leaders ultimately *implement* policies that they otherwise would not have in response to new research findings. In short, is a lack of (access to) research information a binding constraint on policy choice?

In this paper, we provide experimental evidence on these questions. To do so, we leverage an unusual collaboration with the National Confederation of Municipalities (*Confederação Nacional de Municípios*, or CNM) in Brazil. We first report results from a demand/beliefs experiment measuring policy-makers' willingness-to-pay (hereafter WTP) to learn the findings of rigorous impact evaluation research, as well as how such findings affect their beliefs. 900 municipal officials (primarily mayors) from 657 municipalities participated in this first experiment. To estimate the ultimate impact on actual policy adoption, we use a second, larger-scale policy-adoption field experiment with 1,818 Brazilian mayors. A randomly-selected treatment group of mayors was invited to attend a research information session at a large CNM convention. A presenter informed the audience about the findings of a set of RCTs showing positive effects on tax compliance of a taxpayer reminder letter policy. We then measured policy adoption at the municipality level 15 to 24 months later. In combination, the demand/beliefs and policy-adoption experiments allow us to estimate both the extent to which research findings influence policy if directly provided to political leaders, and the interest and belief-updating that mediates such influence.

Brazil's municipalities are an excellent setting to investigate how research affects policy practice for three reasons. First, their political leaders hold a role analogous to that of many countries' head of state: Brazilian mayors are directly elected and individually wield considerable de jure power over policy choices within the areas municipalities control, and over 90 percent also raise tax revenues locally.[2] Second, there are 5,570 municipalities in Brazil, and our collaboration with CNM gives us direct access to their leadership. This enabled us to carry out experiments at the polity level. Finally, local governments and governments in developing countries may lack the resources or technical capacity to acquire relevant research information.

Demand/beliefs experiment. Our first experiment finds that the political leaders of Brazil's municipalities exhibit significant demand for research and change their beliefs in response to research findings. The policy context for the experiment is Early Childhood Development (ECD) programs,

[1] For example, more than 2,500 studies have been registered with the American Economic Association's registry for randomized controlled trials (RCTs) since its launch in May 2013.

[2] In Brazil, municipalities control policy areas such as pre-school and primary education, and preventative health and sanitation. Over 90 percent of Brazilian municipalities raise tax revenues locally—primarily from property and service taxes—in addition to the federal and state transfers they receive.

whose impacts on children's test scores have been estimated in existing, rigorous research. We make use of four otherwise comparable RCTs conducted in different locations and with different sample sizes.[3] Our experiment begins by eliciting beliefs about the likely impact of an ECD policy if implemented in the participant's own municipality. We then present the participant with one randomly-selected study, mentioning two study characteristics (location and sample size). We elicit the participant's WTP to learn the study's results using an incentive-compatible procedure, and then randomize whether the individual actually receives the result (conditional on their WTP).[4] If the results of the study are revealed, we elicit the participant's posterior beliefs about the likely effect of the policy. We also elicit incentivized beliefs about the likely effect in the contexts where the policy was actually implemented and evaluated. Finally, we offer the participant the opportunity to pay for practical advice on how to implement the ECD program.

We find that, while participants hold widely varying beliefs about the impact of the ECD policy to begin with, they are willing to pay an arguably fairly high amount to find out the results of an impact evaluation: about USD 36 on average (under certain assumptions to benchmark the experimental currency). The average WTP is higher for studies with a large sample size, and among officials from municipalities that had already implemented a similar program, but *not* for studies conducted in a location that is closer to Brazil's income level. Learning the results of an RCT causes officials to update their beliefs about impact: their posterior is a weighted average of their prior and the revealed study's findings. Consistent with the demand (WTP) findings, policy-makers update their beliefs more when they receive large sample studies, but not when they receive studies conducted in developing countries rather than the U.S. While we cannot rule out that these differential responses to different studies are in part driven by attributes participants expect to correlate with sample size and the economic development of the study location—the two study characteristics we explicitly state—60 percent of the participants who report preferring the large sample studies in a debriefing survey mention statistical precision as a reason.

While our experiment is not designed to test a complete model of rational learning—we do not know how much participants *should* pay for, or *should* update their beliefs in response to, research—we find little evidence for certain deviations from Bayesian learning. Specifically, participants on average do not display confirmation bias—they do not interpret information in a way that tends to reinforce their prior—nor do they respond asymmetrically to good versus bad news regarding the policy (relative to their prior). We also do not find evidence for politically-motivated updating. Randomly informing participants (truthfully) that recent leftist governments in Brazil strongly supported the use of ECD policies had no statistically significant effect on their priors, WTP or belief-updating, on average or differentially by partisan affiliation.

Altogether, the findings of our first experiment suggest that political leaders are fairly sophis-

[3]The studies we use are Grantham-McGregor *et al.* (1991); Walker *et al.* (2005); Puma *et al.* (2010); Barnett (2011); Attanasio *et al.* (2014). These are all high-quality studies of the impact of ECD in respectively Jamaica (first two studies), the U.S. as a whole, Michigan, and Colombia, with varying sample sizes.

[4]WTP is elicited in terms of an experimental currency. Specifically, each participant is endowed with lottery tickets with a chance to win an expenses-paid trip to the United States. They may instead use some of these lottery tickets to purchase access to the findings of the research.

ticated consumers of research information, at least once such information is made (easily) accessible. In line with this interpretation, we find that a higher posterior causally increases the policymaker's WTP for practical information on how to implement the policy.

Policy-Adoption Experiment. Our second experiment shows that supplying mayors with research findings affects the actual policies adopted by their municipalities. In this experiment, we assigned a randomly chosen subset of the mayors attending CNM's 2016 *Novos Gestores* convention in Brasília—the heads of the governments of 1,818 Brazilian municipalities—to a treatment group which was invited to attend an optional research information session.[5] The policy tool discussed in the session was reminder letters to taxpayers to induce them to comply with taxes. We chose this policy both because its impact is well-documented in existing, rigorous research, and because it is inexpensive and easy to implement. During the 45-minutes long information session, an experienced presenter introduced the idea of impact evaluation, described taxpayer reminder letters and their content, and presented research findings from studies on the quantitative impact of such letters on tax compliance.[6] At the end of the session, mayors were provided with a printed policy brief summarizing the information.

We find that attending the research information session increased the probability that municipalities had implemented taxpayer reminders 15-24 months later by a remarkable 10 percentage points, or 33 percent relative to the 32 percent of municipalities in the control group which already implemented the policy.[7] There is little evidence of heterogeneity in treatment effects by leader or municipality characteristics; for example, term-limited mayors appear to be equally likely to respond as mayors that face re-election incentives.

In combination, the findings from our demand/beliefs and policy-adoption experiments make clear that political leaders are interested in; update their beliefs in response to; and ultimately act on new research findings. Of course, caveats and open questions remain. Our policy-adoption experiment studies a low-cost and easy-to-implement policy: information might not be the binding constraint to more expensive or technically demanding (but effective) policies being implemented. We also do not capture the numerous less-direct channels through which research may influence policy. Finally, if policy-makers do value research information and react to it, as we argue, this raises an important question: what prevents them from acquiring such information in the status quo? We hope that future work will shed light on these questions.

This paper contributes to and bridges the literatures on state effectiveness on the one hand, and

[5]The sampling frame consists of Brazilian municipalities with populations between 5,000 and 100,000 inhabitants for which the mayor was confirmed to attend the *Novos Gestores* convention. 45 percent of all mayoral administrations in Brazil within the relevant population range went to Brasília and thus were part of our sample. There are 881 municipalities in the treatment group and 937 municipalities in the control group.

[6]The findings that were presented at the information session were based on the following studies Coleman (1996); Hasseldine *et al.* (2007); Del Carpio (2013); Fellner *et al.* (2013); Castro & Scartascini (2015); Hallsworth *et al.* (2017).

[7]We surveyed key bureaucrats in treatment and control municipalities with knowledge of the municipality's tax policies (typically in the finance department) from February to November 2018—15-24 months after the *Novos Gestores* convention—to verify whether taxpayer reminder letters were being implemented in the municipality. We additionally called mayors and tax bureaucrats to measure both beliefs about policy impacts and policy adoption. In 81 percent of the municipalities in the sample, at least one person was surveyed. There was no differential attrition between treatment and control municipalities.

the role of evidence and experts' beliefs on the other. The former has focused on selection into the state enterprise, and variation in politicians' and public sector workers' effectiveness under different incentive schemes.[8] Using a polity-level field experiment somewhat parallel to the management interventions in private firms first studied in Bloom *et al.* (2013), we instead show that information frictions at the top—heads of government's lack of knowledge of policies' effectiveness—directly constrains policy decisions.[9] Our findings make clear that it is not the case, for example, that counterfactual policies' effectiveness is widely known "on the ground", nor that political leaders are uninterested in, unconvinced by, or unable to act on new research information. This implies that policy research can help political leaders improve their constituents' lives.

By starting to unpack how political leaders' beliefs are shaped—and their consequences—we also advance an emerging body of evidence on belief formation and the role of evidence. While most such research studies beliefs in lay populations to identify systematic biases and heuristics (see Benjamin (2018) for a review), we add to the smaller body of work studying the beliefs of experts such as central bankers (Malmendier *et al.*, 2017), academics (DellaVigna & Pope, 2018), and judges (Chen *et al.*, 2016).[10] In this sense, our study is most closely related to Banuri *et al.* (2017), Nellis *et al.* (2019), and Vivalt & Coville (2019), who study how the beliefs of policy professionals—program officers, aid-agency workers, and government officials—respond to research findings and new data. Complementing their work, we study the extent to which academic research changes elected heads of government's beliefs; the extent to which they themselves *value* access to research, and how *policy adoption* ultimately responds to research findings.[11] It also complements recent research showing that citizens do change their policy preferences in response to evidence, even on controversial topics such as immigration (Grigorieff *et al.*, 2018; Haaland & Roth, 2019).

The rest of the paper proceeds as follows. Section 2 provides institutional information about Brazilian local governments and our partner organization. Section 3 presents the design and results from the demand/beliefs experiment. Section 4 discusses our second intervention, the policy-adoption experiment, and finally we conclude in Section 5.

[8] The literature on state effectiveness often views states as organizations and has focused on front-line public sector workers (see Finan *et al.* (2017) for a review), bureaucrats (see e.g. Duflo *et al.* (2013); Nath (2015); Khan *et al.* (2016, 2018); Akhtari *et al.* (2018); Bertrand *et al.* (2018); Best *et al.* (2018); Duflo *et al.* (2018); Rasul & Rogger (2018), among others), and leaders' identities (Chattopadhyay & Duflo, 2004; Jones & Olken, 2005; Besley *et al.*, 2011; Beaman *et al.*, 2012; Martinez-Bravo, 2014; Yao & Zhang, 2015; Easterly & Pennings, 2017; Martinez-Bravo, 2017; Bertrand *et al.*, 2018; Xu, 2018). For an overview of the literature on politican motives, see Persson & Tabellini (2002).

[9] In this sense the existing study closest to ours is Hoffmann *et al.* (2017). They carry out an innovative lab-in-the-field incentive-compatible choice experiment in which elected county councilors in Kenya chose among alternative water infrastructure projects. Other influential polity-level natural and field experiments such as Fujiwara & Wantchekon (2013) and Bidwell *et al.* (2018)—and related studies in political science—have randomized how electoral campaigns take place across electoral districts or villages and studied the impact on electoral outcomes.

[10] Our policy-adoption experiment builds on the influential information-provision approach pioneered by Jensen (2010) and many related studies (see, among others, Kling *et al.* (2012); Chetty & Saez (2013); Dizon-Ross (2018)).

[11] Banuri *et al.* (2017), Nellis *et al.* (2019), and Vivalt & Coville (2019) study the belief-formation of (mostly U.K. and U.S.-based) policy professionals. Like Nellis *et al.* (2019), we find evidence of fairly sophisticated processing of information, with little evidence of obvious deviations from Bayesian learning. Unlike Vivalt & Coville (2019), we do not find evidence of precision-neglect or of asymmetric responses to positive and negative news, and unlike Banuri *et al.* (2017), we do not find evidence of ideologically-driven confirmation bias. Another related paper is Beynon *et al.* (2012), who use an online experiment to study the optimal design of policy briefs.

2 Institutional Background and Context

This section provides relevant background information on municipal governments in Brazil, our partner organization, and the conferences where our experiments began.

2.1 Brazilian municipalities

Municipalities are the lowest level of government in Brazil. In total, there are 5,570 municipalities distributed across 26 states. Municipal governments are headed by elected mayors, who appoint secretaries to lead the municipal bureaucracy. Once elected, mayors serve a four-year term and can hold office up to two consecutive terms. Elections are generally considered fair, such that politicians face some electoral accountability. In addition to the executive branch, Brazilian municipalities have a legislative branch, which reviews and approves the annual budget, participates in the elaboration of local laws, and oversees the mayor's administration.

In Brazil, as in many Latin American countries, provision of services is generally devolved to municipalities, while revenue generation and collection is partially devolved. Municipal governments are responsible for key public services such as education, health, sanitation, and transportation. To cover the costs, municipalities rely in part on intergovernmental transfers. On average, 60 percent of municipalities' total revenues are transfers from state governments and the federal government. Part of the remainder is locally raised by municipalities themselves. Municipal governments are responsible for collecting local taxes, which represent on average 15 percent of municipal revenues.

In general, municipal governments are highly autonomous. The mayor negotiates the budget allocation with the city councilors and has full autonomy over its execution. The mayor's office thus holds policy-making authority over a wide range of areas. Our research information experiments will involve two such areas: early-childhood education and locally raised taxes. We describe these two areas in more detail in sections 3 and 4.

2.2 Our partner organization

This study leveraged a unique opportunity to conduct a series of large-scale experiments with thousands of local political leaders through a partnership with Brazil's National Confederation of Municipalities (CNM). CNM is a non-partisan organization that serves as a coordinating body and advocate of Brazilian municipalities' interests at the state and federal level. Over 80 percent of all Brazilian municipalities are members of CNM. Importantly for our purposes, CNM organizes a variety of conferences and conventions throughout the year, in which thousands of municipal officials from all over the country participate.

These meetings provide an unusual opportunity to reach a large population of political leaders in one place. Meeting attendees comprise mayors, vice-mayors, local legislators, and municipal secretaries. Our demand/beliefs experiment was conducted at two of CNM's annual national conventions (May 2017 and May 2018) and at 12 regional conferences held in different states (August-

December 2017).[12] Our policy-adoption experiment was conducted at CNM's biggest national conference—called *Novos Gestores*—which is held every four years in Brasília (October-November 2016). All mayors who were (re-)elected in the last municipal election are invited to attend *Novos Gestores*.

Our research-information interventions were one of the many activities that took place at these meetings. The meetings are each approximately three days long, and are structured around different training sessions and presentations by various political actors, including regional actors such as the regional associations of municipalities, and public and private municipal suppliers, as well as national ones such as CNM itself, federal government officials, congress representatives, and often the Brazilian President. In addition to attending the presentations, local policy-makers use the meetings to organize get-togethers with each other and with state and federal officials. Each national conference brings around 4,000 municipal representatives and 2,000 mayors, while the regional conferences attract around 200 local political leaders, of which approximately 50 are mayors. Thus, our experiments take place in a quite natural setting, where policy-makers are used to receiving useful information.

2.3 Identifying target policies

All information we provided to policy-makers in the experiments satisfied two main conditions. First, the policies we focused on were directly within the control, familiarity, and broadly stated interest of municipal officials. Second, the information we provided was based on rigorous research, with emphasis on studies that evaluated interventions in Latin American countries.

To identify policy areas of interest to local policy-makers, we conducted comprehensive surveys and focus groups with 60 mayors in May 2016. Substantial interest in acquiring research information was reported by mayors, especially on pre-school education, preventive health care, and management practices. Mayors were also concerned with budgetary issues, especially considering the fiscal crisis affecting state and local governments in Brazil at the time (Mulas-Granados, 2017). Based on mayors' priorities, we searched for, and systematically reviewed, research studies on Google Scholar, and the websites of J-PAL, IPA, 3ie, World Bank, IADB, and leading policy and research institutions in Brazil such as the repository of papers on IPEA, C-Micro-FGV, and on the websites of leading Brazilian scholars. We identified a number of promising options, and after coordinating with CNM, we decided to build the experimental interventions based on research information on early childhood development programs and on tax reminder letters. These policies were appealing for our purposes because they were evaluated in existing, rigorous research, and the taxpayer reminder letter policy we focus on in the policy-adoption experiment is inexpensive and relatively easy to implement. In addition, the set of studies evaluating the impact of each of the two policies varied in their attributes, allowing us to investigate how study features such as sample size and location affect policy-makers' responses.

[12]The 12 regional conferences were held in the following states: Alagoas, Bahia, Ceará, Espírito Santo, Maranhão, Mato Grosso do Sul, Minas Gerais, Paraná, Piauí, Rio Grande do Sul, Santa Catarina and São Paulo.

3 Demand/Beliefs Experiment

In this section, we describe an experiment to measure (a) whether Brazilian policy-makers exhibit demand for research information, and (b) how receiving such information affects their beliefs. The policy area this experiment focused on was Early Childhood Development (ECD) programs, a well-studied topic in social science. We find that policy-makers value research on the effect of ECD programs, and update their beliefs in a fairly sophisticated way in response.

3.1 Experimental setting and sample

We implemented the demand/beliefs experiment with 900 officials from 657 municipalities at 14 CNM meetings across Brazil in 2017 and 2018.[13] The conferences were attended by mayors, vice-mayors, municipal secretaries, and local legislators. We designed a half-hour long experiment that was self-administered by participants using tablets. Research assistants recruited conference participants during breaks in between sessions. Participation was voluntary but incentivized as described in the next section. One of the researchers and one research assistant were present throughout to monitor and answer questions.

Almost 49 percent of participants in the experiment were mayors; 28 percent were local legislators; 16 percent were municipal secretaries; and 7 percent were vice-mayors.[14] The geographical distribution of the municipalities represented is shown in Figure 2, and Table 1 displays summary characteristics. About 37 percent of represented municipalities have mayors affiliated to a leftist political party, and approximately 20 (78) percent of children aged 0 to 3 (4 to 5) years old in these municipalities attend a pre-school educational establishment. 42 percent of participants report that their municipalities have implemented ECD programs.

3.2 Experimental design

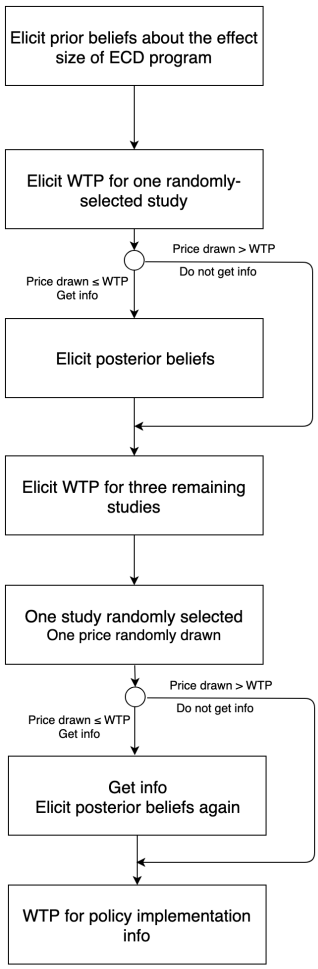
The structure of the experiment, depicted graphically in Figure 1, was as follows. We began by introducing the ECD policy. Then we elicited participants' prior beliefs about the effectiveness of the policy, and their willingness-to-pay (hereafter WTP) to learn the findings from related impact evaluation research. Next, we revealed the findings, and finally, we elicited participants' posteriors to assess the extent to which the research findings affected their beliefs. The Appendix provides the key parts of the experimental script. Below, we describe the experiment in greater detail.

Introductory Stage. We began with a short survey eliciting demographic and professional information. Next, we described ECD programs, highlighting the key outcomes on which such programs are evaluated (test scores, cognitive skills) and how those outcomes are reported (standardized effect sizes). To ease understanding of the policy and its objectives, we provided illustrative

[13] The meetings comprised two national conferences held in Brasília (May 2017 and 2018), and twelve regional *Diálogo Municipalista* conferences organized from August to December 2017 in the states of Alagoas, Bahia, Ceará, Espírito Santo, Maranhão, Mato Grosso do Sul, Minas Gerais, Paraná, Piauí, Rio Grande do Sul, Santa Catarina and São Paulo.

[14] We were able to complete the experiment with 38.8 percent of attending mayors, 48.5 percent of vice-mayors, 35.4 percent of municipal secretaries, and 40.9 percent of local legislators.

FIGURE 1: DEMAND/BELIEFS EXPERIMENT: STRUCTURE



examples of current similar programs in Brazil and presented participants with a few benchmarks for effect sizes, such as the gains in standardized test scores associated with an additional year of high school in Brazil (0.2 sd).

Eliciting priors. We began the main part of the experiment by eliciting the participant's prior beliefs. Specifically, we asked what they believed the impact of the policy on cognitive skills was likely to be if it were to be implemented in his/her own municipality.[15] Immediately after, we asked a similar question about the expected impact in two other locations. These two other locations were randomly chosen out of four locations where academics have estimated the impact of ECD programs using RCTs. These studies vary in location and sample size. They evaluate comparable ECD programs in Colombia (n=1420) (Attanasio *et al.*, 2014), Jamaica (n=130) (Grantham-McGregor *et al.*, 1991; Walker *et al.*, 2005), Michigan (n=123) (Barnett, 2011), and across multiple

[15]For simplicity and due to limited time with each participant, we elicited only point predictions (about effects on cognitive skills), rather than full probabilistic beliefs.

states in the U.S. (n=4667) (Puma *et al.*, 2010). When the relevant studies were presented to the participant, we highlighted both the study location and sample size.

Attributes	Small Sample	Large Sample
Developing Country	Jamaica, n = 130	Colombia, n = 1420
Rich Country	Michigan, n = 123	USA, n = 4667

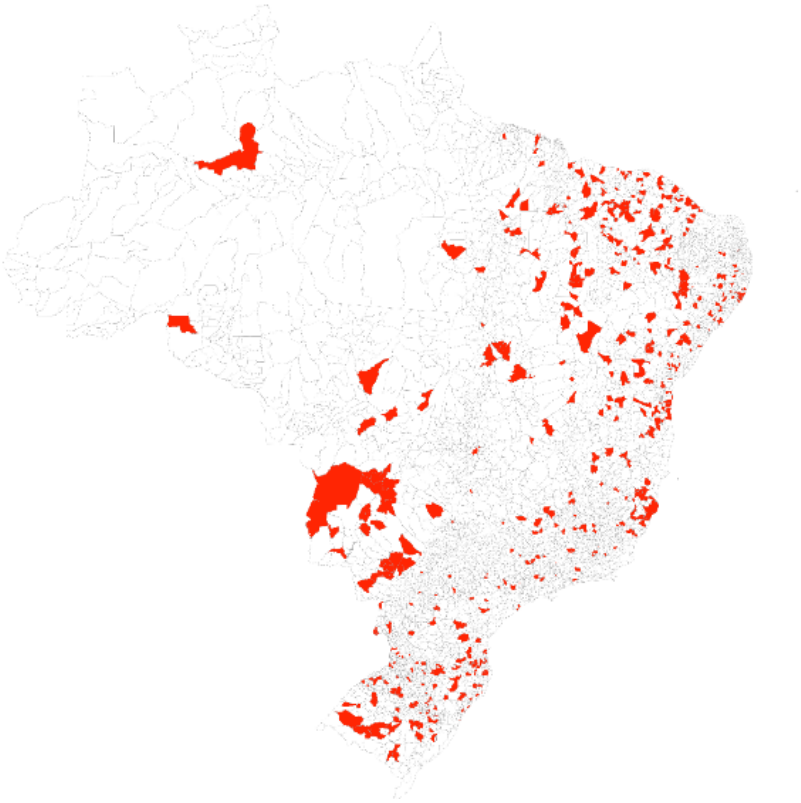
While we cannot incentivize accurate beliefs about the impact in the participant's own municipality (since we do not observe the true effect), we randomize incentives to accurately predict the effect in the other two locations (where we can compare the participant's prediction to the estimates from the research). In practice, we found that the size of the incentives has no effect on priors, WTP, or posteriors, suggesting that participants took the questions seriously even in the absence of incentives, and that making better predictions for the sake of higher payoff *within the experiment* is not an important driver of this paper's results.

WTP and Belief Updating: Round 1. After the participants reported their priors, we offered them the chance to purchase the findings (i.e. learn the estimated effect size) from one randomly-chosen study. The experimental currency in which we elicited WTP consisted of lottery tickets, which also incentivized participation. We initially endowed each participant with 100 such lottery tickets, each with a chance of winning a free trip to visit the United States (typically a visit to Boston, including a tour of the Harvard University campus). Participants could save their lottery tickets for the lucky draw or use some, or all of them, to learn the estimated effect size of the study. Following a Becker-DeGroot-Marschak elicitation procedure (BDM), we measured the participant's maximum WTP [0 to 100] to find out the results of the relevant study. We then drew a randomized price for the study. If the price was below the participant's WTP, we revealed the findings and deducted the price from the participant's stock of lottery tickets.

To ensure that we observed belief updating for most participants, while maintaining incentive-compatibility in the BDM procedure, the price was drawn from a distribution with high mass at zero. Consequently, 80-90 percent (depending on the conference) of participants received the information *regardless of their WTP*. We emphasize this sample for the belief-updating analysis, since these participants receive the information without selection. For those who received the information, we subsequently elicited posterior beliefs about the expected impact of the policy in their own municipality, and in the study location that was *not* offered for purchase in this round.

WTP and Belief Updating: Round 2. In the next stage, we presented the participant with a menu of the three studies that were not offered for purchase in Round 1, again highlighting each study's location and sample size. The participant received a fresh budget of 100 lottery tickets and was told that one of the three studies would be randomly offered for purchase. They were asked to report their WTP for *each* study, to be implemented if that study was randomly chosen for sale. We thus obtained incentive-compatible WTPs for each of the three studies. We revealed the findings of one study following the same procedure as before, and again elicited an updated posterior belief.

FIGURE 2: DEMAND/BELIEFS EXPERIMENT: SAMPLE MUNICIPALITIES



3.3 Results

We interpret the results from our experiment through the lens of a standard Bayesian-learning framework. Suppose that policy-maker i has a prior belief $S_i^{pr} \sim \mathcal{N}(\mu_i^{pr}, \Sigma_i^{pr})$, where μ_i^{pr} is the mean of i's prior and Σ_i^{pr} is the perceived variance or uncertainty of their prior about the likely effect of the ECD policy if implemented in their municipality. The effect size from the research study can be thought of as a noisy signal $S_{i,c}^I \sim \mathcal{N}(\mu^I, \Sigma_{i,c}^I)$, drawn from a distribution centered around the true value μ^I, but with variance $\Sigma_{i,c}^I$, where c indexes characteristics of the study, such as its sample size or location. Then, a Bayesian policy-maker who wants to have accurate beliefs (to minimize mean squared error) will form a posterior S_i^{po}:

$$S_i^{po} = (1-\pi)S_i^{pr} + \pi S_{i,c}^I$$

with the weights $\pi = \frac{\Sigma_i^{pr}}{\Sigma_i^{pr}+\Sigma_{i,c}^I}$. That is, a Bayesian learner's posterior will be a convex combination of their prior and the "signal" (i.e. the effect-size from the study), with weights proportional to the perceived relative precision of each component.

We can think of the key attributes of the study—location and sample size—as affecting the perceived precision or informativeness of the noisy signal. If participants think that larger-sample

studies are more informative ($\Sigma^I_{i,large} < \Sigma^I_{i,small}$), they will place greater weight on the effect size of larger-sample studies while forming their posterior beliefs. Importantly, if policy-makers value having accurate beliefs about the effectiveness of ECD policies, their WTP for signals will be higher for the signals which they will ex-post weight more strongly in their belief updating.

Priors about effect size. We start by analyzing policy-makers' priors about the effectiveness of ECD policies. The average policy-maker prior appears sensible. Appendix Table A.1 shows that the average policy-maker believes that ECD policies are more effective in rich countries (effect size of 0.46-0.49 sd) than in developing countries (effect size of 0.38-0.41 sd). On average, municipal officials believe the effect size in their own municipality (0.4 standard deviations) is very close to the average prior for the developing countries. However, this masks substantial heterogeneity in priors: the standard deviation of priors is 0.22, implying substantial disagreement across policy-makers.[16]

Willingness-to-pay for estimated effect size. After policy-makers reported their priors, we elicited their WTP to learn the research finding of one of the four (randomly assigned) studies. According to the Bayesian framework, WTP should be larger the more informative the signal is perceived to be. We estimate the following equation:

$$WTP_{ijs} = \beta_0 + \beta_1 Developing_{ijs} + \beta_2 Large_{ijs} + \varepsilon_{ijs} \quad (1)$$

where WTP_{ijs} is the WTP (in terms of lottery tickets) for the research finding of policy-maker i in round $j \in 1, 2$ for study $s \in$ Michigan, USA, Jamaica, Colombia. $Developing_s$ equals one for studies in Jamaica or Colombia and 0 otherwise. $Large_s$ equals one for the two large-sample studies (Colombia with n=1420 and USA with n=4667) and 0 otherwise (Jamaica with n=130 and Michigan with n=123). Standard errors are clustered at the individual level.

Table 2 presents the OLS results from specification (1). Column 1 pools the two rounds, while columns 2 and 3 present estimates separately for round 1 and round 2 respectively. We find that policy-makers allocate on average 45 lottery tickets (out of the 100 tickets they are endowed with each round) to learn about the effect size of a particular study. While this is a large share of their experimental endowment, it is difficult to interpret the level directly since the currency is lottery tickets, whose subjective value is unobserved. To benchmark the WTP, we calculated a money metric for the experimental currency by offering gift cards from a major retail and online chain (*Lojas Americanas*, similar to Walmart) for purchase using a similar BDM procedure to a sub-sample of participants. We found that an additional lottery ticket was exchanged for approximately 0.80 USD worth of gift cards. This benchmarking must be interpreted with caution, but suggests that the baseline WTP for the research finding of 45 lottery tickets was equivalent to 36 USD. There is substantial heterogeneity in demand: the standard deviation of WTP is 32 lottery tickets. Yet, 98 percent of participants have strictly positive WTP.[17]

[16] Of course, some of this variance in priors may reflect noise in the belief-elicitation process.

[17] Readers might wonder why participants would not simply look up the research themselves. While this may happen to some extent, we believe that unfamiliarity with research-information sources, language barriers, and difficulty interpreting academic writing are all factors that make this strategy difficult for our study participants. Our estimates may be thought of as capturing their WTP for simplified, conveniently-presented, bottom-line information.

We next analyze whether demand for research findings varies with the attributes of the research. We find that political leaders are willing to pay about 8 percent more for large-sample size studies than for smaller-sample studies. Thus, policy-makers appear to ex-ante value the statistical precision of a study. This relationship is stronger in the second round, when studies are offered side-by-side, but the second-round estimate is not statistically different from the first-round estimate (p-value 0.496). In contrast, and contrary to our priors, we do *not* find significant differences between the WTP for research findings from developing versus developed countries. This suggests that Brazilian policy-makers do not consider studies from other developing countries to be more informative—more externally valid for them—than rich-country studies.

We report participant and municipality-level correlates of WTP in Appendix Table A.2. Only three characteristics out of nineteen are significantly associated with WTP: whether the participant is male, whether their municipality has previously implemented an ECD policy, and whether they reported having previously heard about such policies despite not having implemented them. Through the lens of the framework, the latter correlations are not obvious: policy-makers with more past experience with a policy might already perceive that they have a precise prior, and therefore not value more information. Instead, we find that it is precisely the policy-makers who implement and spend municipal resources on ECD programs who have the highest WTP for related research information.

Belief Updating. Having established that political leaders value research findings, and pay more for larger-sample studies, we turn to whether and how they actually update their beliefs upon learning research findings. Note that if policy-makers purchase information purely to use it to persuade others, for instance, they might not update their own beliefs upon receiving the information.

Following the Bayesian framework, we estimate the following equation:

$$Posterior_{ijs} = \beta_1 Prior_{ij} + \beta_2 Signal_{ijs} + \varepsilon_{ij} \tag{2}$$

where $Posterior_{ijs}$ is policy-maker i's updated belief about the likely effect in their own municipality after learning the effect size from study $Signal_{ijs}$ of study s in round j. Posteriors after round 1 serve as priors for round 2, and standard errors are clustered at the individual level.

Table 3 presents the OLS estimates of specification (2). Column 1 pools the two rounds, while columns 2 and 3 present estimates separately for round 1 and round 2 respectively. Consistent with the Bayesian framework, $\hat{\beta}_1$ and $\hat{\beta}_2$ are both positive and statistically significant, and sum up to approximately 1. Participants place substantial weight on both their prior and the study finding, and do not simply accept or repeat back the research finding. This finding reduces concerns about experimenter demand effects. They place slightly greater weight on their prior when forming beliefs about their own municipality, compared to beliefs about an alternative location (Column 4 vs. Column 2). They also place more weight on their prior in the second round, when it already incorporates the finding of the first study they received. As described previously, by design, 80-90 percent of participants are assigned a zero price and receive the research information regardless of their WTP. Column 5 restricts attention to these observations, and finds very similar results as in the full sample.

In order to test whether participants update more based on large-sample or developing-country studies, we estimate:

$$\begin{aligned} Posterior_{ijs} = \beta_1 Prior_{ij} + \beta_2 Signal_{ij} \\ + \beta_3 Developing_{ijs} \times Prior_{ij} + \beta_4 Developing_{ijs} \times Signal_{ij} \\ + \beta_5 Large_{ijs} \times Prior_{ij} + \beta_6 Large_{ijs} \times Signal_{ij} + \varepsilon_{ij} \end{aligned} \quad (3)$$

where $Large_{ijs}$ and $Developing_{ijs}$ are defined as in equation (1). Under the framework, if an individual perceives a study to be more informative, they will place more weight on the signal from that study and correspondingly less weight on their prior. Therefore, to test whether participants perceive (say) large-sample studies to be more informative, we can test whether $\beta_5 < 0$ and $\beta_6 > 0$, or instead (a weaker test) whether $\beta_6 - \beta_5 > 0$.

Table 4 presents the OLS results of specification (3). Again, Column 1 pools the two rounds, while columns 2 and 3 present estimates separately for each round. We find consistent evidence that participants place greater weight on signals from large-sample studies, but not on signals from developing-country studies.[18] This lines up with the findings on WTP, and confirms that these policy-makers find larger-sample studies to be more informative, but do not consider studies from developing and rich countries to be differentially informative. The greater weight placed on large-sample studies is evident also in round 1, when one study is presented in isolation. The pattern of results holds up, and indeed is slightly strengthened, when we restrict attention to cases where the price drawn was zero in Column 5.

Caveats, Confounds and Qualifications. While we interpret the differences in WTP and belief-updating across sample size and study location as the direct effect of these two characteristics, both could be correlated in policy-makers' minds with omitted variables such as the quality of the research, the scale of implementation of the program, etc. To shed light on this, we conducted a debriefing survey with a subset of the sample (n=296). We find that 60 percent of policy-makers who preferred large-sample studies chose statistical precision as the reason. Intriguingly, a smaller share also reported preferring larger-sample studies because they are more likely to have evaluated programs implemented at scale (23 percent) and by the government (15 percent). In the case of study location, the survey results are more mixed: while individuals who preferred studies from Colombia or Jamaica reported their lower standard of living and similar state capacity as reasons, a substantial share also reported preferring the US studies, and listed a higher standard of living and similar state capacity as reasons. One interpretation is that some policy-makers in Brazil may see their municipalities as closer to developing countries, while others may see themselves as closer to rich countries.

The results on belief updating (but not WTP) have another potential confound in interpretation: the two larger-sample studies in practice estimated smaller effect sizes. This is a feature in the four studies we use, and also more generally documented in the ECD literature (Barnett, 2011).

[18] Contrary to the Bayesian learning framework, the reduction in weight on the prior is smaller than the increase in weight on the signal.

What if participants simply update more (in proportional terms) for some reason in response to small effect sizes? There is no clear theoretical reason to expect this. Nonetheless, we have some unplanned variation which may shed light on this concern: in six of the fourteen conferences where the experiment was conducted, we reported a different (smaller) effect-size for certain studies. Specifically, for the small-sample studies, we truthfully reported an estimated effect size of the study, but assessed at a much longer time period, which resulted in a smaller effect size. Appendix Table A.3 tests whether the larger weight on large-sample study signals is less pronounced in those conferences. Consistent with our initial interpretation, the weight placed on sample size does not vary significantly across these conferences.

3.4 Potential deviations from Bayesian learning

In this section, we test for potential deviations from Bayesian learning by testing for asymmetric updating, confirmation bias, and politically-motivated updating.

Confirmation bias and asymmetric updating. Confirmation bias is the tendency to acquire and interpret information in a way that confirms one's pre-existing beliefs (Nickerson, 1998). This phenomenon has been studied in a number of settings, and debates exist as to its prevalence and importance in causing polarization and making individuals immune to evidence (see e.g. Lord *et al.*, 1979; Kuhn & Lao, 1996; Nyhan & Reifler, 2010; Wood & Porter, 2019). It is natural to therefore ask if political leaders and other policy-makers exhibit confirmation bias when faced with evidence from research on policy effectiveness. Do policy-makers who start off with more positive beliefs about a particular policy under-react to negative (disconfirming) information about that policy relative to positive (confirming) information? And do policy-makers with negative priors do the reverse? We test this by estimating equations of the form:

$$Posterior_{ijs} = \beta_1 Prior_{ij} + \beta_2(Signal_{ij} - Prior_{ij}) + \beta_3(Signal_{ij} - Prior_{ij}) \times PositiveSurprise_{ij} + \varepsilon_{ij} \tag{4}$$

where $PositiveSurprise_{ij} = \mathbb{1}\{Signal_{ij} - Prior_{ij} > 0\}$ is a dummy equal to 1 when the revealed effect-size from the study is larger than the participant's prior, and 0 otherwise. $\beta_3 > 0$ implies that participants place more weight on positive news than on negative news, while $\beta_3 < 0$ would imply placing more weight on negative news than on positive news.[19]

Table 5 reports the results. Column 1 shows that, on average, policy-makers do *not* react asymmetrically to positive news relative to negative news. Columns 2 and 3 contrast the estimates for participants with prior beliefs above versus below the median respectively, while Columns 4 and 5 do the same for participants in the top versus bottom quartiles instead. We find no evidence of confirmation bias.[20] Column 6 pools together the policy-makers with priors above and below

[19] See also Vivalt & Coville (2019). An alternative way to set up the estimating equation would be as in Equation 3, interacting the prior and signal separately with $PositiveSurprise$. We choose to instead include the $Signal - Posterior$ term and its single interaction with $PositiveSurprise$ for ease of exposition and interpretation, especially once we examine politically-motivated information processing in Table 6.

[20] Columns 2 and 4 show that individuals with high prior beliefs do not react more to positive news. Columns 3 and 5 show that, if anything, policy-makers with low priors react more to positive rather than negative news, although the

the median, and tests whether individuals react more to confirming rather than disconfirming evidence; they do not.[21] Column 7 repeats this for individuals in the top and bottom quartiles, and finds a similar result. Altogether, we find no evidence for confirmation bias or asymmetric updating on average when policy-makers are presented with research evidence on policy effectiveness.

Politically-motivated information processing. Politically-motivated reasoning is thought to be a source of persistent disagreement over facts (Kahan, 2015). There is evidence that providing information on politically-charged topics can actually increase polarization, and that many individuals neglect information that is inconsistent with their group's position (e.g. Baekgaard *et al.* (2017); Nyhan & Reifler (2010)). However, much of this evidence comes from student or Mturk samples and often without incentives for truthful reporting. We have little evidence on potential biases in political leaders' belief-updating (Vivalt & Coville, 2019; Nellis *et al.*, 2019). We thus randomized messaging that (truthfully) revealed strong support for the ECD policy from past left-of-center governments (Lula and Dilma's governments from the Labor Party, PT). The partisan-support message was: "In Brazil, the PT government strongly advocated for early-childhood development policies. In 2007, Fundeb was created to include pre-school and nursery enrollment in the headcount for federal transfers. Programs for pre-school and nursery construction, such as the *pro-Infancia* program, were also launched during Lula's government, and expanded during Dilma's administration." This message is shown before the prior elicitation and briefly reiterated before the WTP elicitation. The objective of the message was to create a partisan association between ECD programs and the PT party, and to observe whether this influences belief-updating differentially across left versus right-wing policy-makers. To study effects on priors and WTP, we estimate:

$$Y_{ij} = \beta_0 + \beta_1 PartisanMessage_i + \beta_2 Leftist_i + \beta_3 PartisanMessage_i \times Leftist_i + \varepsilon_{ij} \quad (5)$$

where Y_{ij} is the policy-maker's elicited prior or WTP; $Leftist_i$ is a binary variable equal to one if the policy-maker self-reports as a leftist on an ideology scale; and $PartisanMessage_i$ is equal to one if they received the randomized partisan message and zero otherwise. β_3 captures the differential effect of the partisan message on left-wing policy-makers.

To study effects on belief-updating, we estimate, separately for left-wing and right-wing policy-makers, specifications of the form:

$$Posterior_{ijs} = \beta_1 Prior_{ij} + \beta_2(Signal_{ij} - Prior_{ij}) + \beta_3(Signal_{ij} - Prior_{ij}) \times PositiveSurprise_{ij} + \beta_4(Signal_{ij} - Prior_{ij}) \times PositiveSurprise_{ij} \times PartisanMessage_i + \varepsilon_{ij} \quad (6)$$

Table 6 reports the results. Columns 1 and 2 present effects on priors and WTP from specification (5). We find no significant effects on either priors or WTP from providing the partisan message, either on right-wing policy-makers (β_1) or differentially for leftist policy-makers (β_3), although the estimates for WTP are somewhat imprecise. Columns 3 and 4 estimate (6) separately for left-wing and right-wing policy-makers to study belief updating. We find no evidence that

estimates are imprecise.

[21] We define a variable $ConfirmingNews$ equal to 1 if an individual with an above-median prior receives a (still more) positive signal or if an individual with a below-median prior receives a (still more) negative signal, and 0 otherwise.

the partisan message causes leftists to begin to respond more to positive signals (β_4 in Column 3), nor does it cause rightists to respond more to negative signals ($-\beta_4$ in Column 4). In an alternative specification in Column 6, we define a variable $PartisanPreferredSignal$ and test whether the partisan message causes policy-makers to respond more to messages which they 'prefer' (i.e. signals which are high in the case of leftist participants and signals which are low in the case of right-wing participants). Again, we find no evidence for this. If anything, the coefficient goes in the wrong direction.

Overall, we find little evidence of politically-motivated beliefs, demand for information or processing of information among our sample of policy-makers. However, there are important caveats to this conclusion. First, it could be that the partisan message does not have an effect because individuals already knew of the support of past PT leaders for the policy. This was not our sense from field work, but we cannot rule it out. Second, there is suggestive evidence in Table 6 that, in the absence of the partisan message, right-wing policy-makers do respond less to positive news. Third, statistical power is limited in this analysis since the cross-randomization was only conducted in a sub-sample. Finally, ECD programs are generally not a highly charged political topic. Politically-motivated reasoning may well occur in other domains and contexts.

3.5 Demand/beliefs experiment: discussion

We have three main findings from the demand/beliefs experiment. First, political leaders in Brazil value learning about research on policy effectiveness. They pay more for larger sample studies, but not for developing-country studies. Second, they also change their beliefs when confronted with evidence from research: they place substantial weight on the new information. They place more weight on larger-sample studies, but again, not on developing-country studies. Third, we have little evidence for specific deviations from the Bayesian framework we considered, such as asymmetric updating, confirmation bias and politically-motivated processing of information. In short, policy-makers seem to both value evidence and update their beliefs in a quite sophisticated way.

WTP for implementation information. But does access to research lead to more effective policies being adopted? At the very end of the demand/beliefs experiment, participants were given the chance to purchase practical information on how to implement ECD policies, using a fresh budget of lottery tickets. We interpret WTP for such advice as a revealed-preference proxy for interest in implementing the policy. Since we experimentally vary bundles of study attributes provided—effect size, developing country context, and large sample—and found that these affect posteriors, we can use these attributes as instruments for participants' posterior. Appendix Table A.4 shows the results. We find that more positive beliefs about ECD programs—shaped through learning about research findings—causally increase WTP for implementation information. While this provides clean, experimental evidence on the effect of research information on demand for policy implementation via changed beliefs, in the next section we turn to a field experiment which measures policy adoption itself.

4 Policy-Adoption Experiment

In this section, we describe a nationwide field experiment to test whether supplying the heads of local governments with evidence from policy-effectiveness research influences the policies implemented in their polities. We show that informing Brazilian mayors about the effectiveness of a policy to increase tax compliance causally increases adoption of the policy in their municipality 1-2 years later.

4.1 Background: Taxpayer reminder letters

The essence of our policy-adoption experiment is to inform a treatment group of mayors about the existing research evidence on a particular policy that has been shown to increase tax compliance: reminder letters to taxpayers.

We chose this particular policy for three reasons. First, increasing tax compliance is important to mayors: they reported considerable interest in increasing tax revenues in our focus groups and scoping surveys. Over 90 percent of Brazilian municipalities raise taxes locally and enforcement of municipal taxes is under the control of municipal governments. Like in most developing countries, taxpayer compliance is a challenge in Brazil. A prominent think tank estimates that at least 20 percent of taxpayers do not comply with property taxes, for instance (De Cesare & Smolka, 2004).

Second, the effectiveness of reminder letters has been rigorously evaluated in multiple RCTs, including two in Latin America (Coleman, 1996; Hasseldine *et al.*, 2007; Del Carpio, 2013; Fellner *et al.*, 2013; Castro & Scartascini, 2015; Hallsworth *et al.*, 2017). Such interventions have been found to be surprisingly effective. For instance, Del Carpio (2013) finds that simple reminder letters increased tax compliance in Peru by 10 percent, while letters that additionally included social-norm language by emphasizing that most people pay their taxes on time increased compliance by 20 percent.

Third, reminder letters are inexpensive and relatively easy to implement, while not being obviously politically sensitive. On the one hand, this means that the policy we chose is likely positively selected in terms of the potential for changes in policy-maker beliefs to translate into policy change. On the other hand, we expect that reminder letters are likely an effective policy tool in part *because* they are low-cost and easy to implement.

Reminder letters to taxpayers are uncommon but far from unheard of in Brazil. In our endline survey for this experiment, 32 percent of control municipalities reported using some form of reminder messages to taxpayers. This sometimes involved sending letters, but also included other communication channels such as text messages, flyers, and media advertising.

4.2 Experimental setting

The policy-adoption experiment was conducted at a large CNM convention—the *Novos Gestores* meeting—for recently elected and re-elected mayors in October-November 2016. The convention is held every four years to train mayors who are about to start their four-year term the following

January. Each mayor participates in the conference for about two days, and can attend multiple training sessions led by CNM staff. The sessions cover a variety of public policy areas, such as education, economic development, health, tourism, and local taxes. Multiple sessions run in parallel throughout the conference.

The sample frame for the experiment was mayors attending the convention who represented municipalities with populations between 5,000 and 100,000. The total sample consists of 1,818 municipalities, which represents 45 percent of all mayoral administrations in Brazil in that population range. Figure 3 shows the spatial distribution of the sample municipalities.

FIGURE 3: POLICY-ADOPTION EXPERIMENT: SAMPLE MUNICIPALITIES

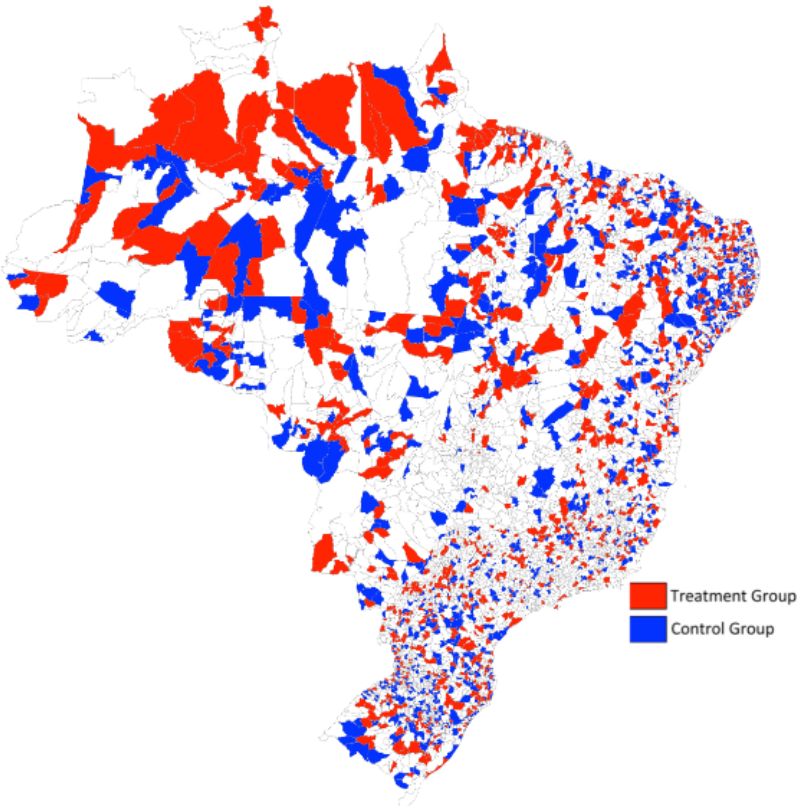


Table 7 provides descriptive statistics on the sample of mayors and municipalities for the policy-adoption experiment. We see, for example, that almost 90 percent of the mayors are men; about 60 percent have at least a bachelor degree; and 16 percent are in their second and last term in office.[22] The average municipality in the sample has a population of about 21,000 residents.

[22] This low share of mayors in their second term is explained, in part, by the political crisis Brazil was going through at the time of the most recent municipal elections (2016), which led to a decrease in the proportion of incumbent politicians winning re-election.

4.3 Experimental design

Mayors attending the conference were randomized into treatment (n=881) and control (n=937) groups.[23] All mayors were free to attend any of CNM's regular *Novos Gestores* training sessions, but only mayors in the treatment group were invited, by email and text message, to attend our research information session. The session was advertised as being on the topic of how to increase local tax revenues, and was framed as a training session organized by CNM as well as researchers at Columbia and Harvard Universities. Since participation was optional, our experiment should be thought of as having an encouragement design. Table 7 shows that the treatment and control groups are largely balanced on mayor's characteristics as well as municipal characteristics.

The information session lasted 45 minutes and was led by an experienced instructor. The instructor began by introducing and defining policy impact, cost-effectiveness, and impact evaluation research. She then provided a description of taxpayer reminder letters, including presenting an example template. Next, she presented the findings (i.e. the estimated effect sizes) of a set of rigorous studies evaluating the impact of taxpayer reminder letters. A list of reminder letter characteristics found to be effective in inducing taxpayers to pay their taxes on time—stating the tax payment deadline; mentioning the possibility of fines and audits for not paying taxes on time; and stating that most people pay their taxes on time—was emphasized, and effect sizes were provided where possible.

The information presented was simplified and the presentation was concise. We avoided jargon and regression tables. The 30-minute presentation was followed by 15 minutes for questions from the audience.[24] At the end of the session, mayors received a professionally-produced policy brief with the same information content as the presentation, including references to the cited papers.[25]

4.4 Data

To measure how the research information provided affected policy adoption and political leaders' beliefs, we conducted in-depth phone surveys of relevant municipal officials from treatment and control municipalities 15 to 24 months after the session. We attempted to reach the bureaucrat in charge of implementing tax policy in each municipality, as well as the mayor him- or herself.[26]

[23] The randomization was stratified on the mayor's education level, whether the mayor was term-limited, the average education level among public employees in the municipality, and the municipality's population size, Gini coefficient, and region. A slightly larger share of municipalities was assigned to the control group due to logistical concerns associated with our capacity to manage a large number of treatment group participants and the capacity of the room that CNM designated for our intervention.

[24] During the 15 minutes reserved for open discussions with mayors, mayors often asked interesting questions about reminder letters and other alternative policies on tax compliance: for example, whether the effects would be the same if the messages were sent by email or text messages, whether the policy could be used to encourage tax debtors to pay their balance, and whether financial incentives such as discounts or lotteries for paying taxes on time are effective policies. We avoided providing confident answers to such questions.

[25] Appendix Figures A.8, A.9, A.10, and A.11 show the policy brief.

[26] Typically, secretaries of finance are responsible for the tax division in Brazilian municipalities. Nevertheless, we specifically asked municipalities' telephone attendants to pass the call on to the person in charge of the tax division. Once we were transferred, we confirmed whether the person actually held that position or asked to get the phone number of the person in charge of implementing tax policy.

The survey was supervised by a research assistant, and conducted by a team of nine surveyors who were blinded to treatment status and the research hypotheses. When the survey ended after 10 months of phone calls, we had successfully interviewed at least one person in 81 percent of our sample of municipalities—75 percent of the chief tax bureaucrats and 50 percent of the mayors in the sample.[27] There was no differential attrition between treatment and control groups, and observable characteristics of the successfully contacted municipalities are similar across both groups, as reported in Table 7.

The survey lasted approximately 15 minutes. The key questions asked were whether the municipality sends taxpayers reminders to pay their taxes, and whether the messages feature the characteristics described in the information session and evaluated in the literature: mentioning the due date, the possibility of fines or audits, and language regarding the social norm of paying taxes on time. We also elicited beliefs about the likely impact of such a policy, even if the municipality reported not using such reminders. In addition, we asked questions that served as attention and comprehension checks as well as placebo questions on which we would expect null effects of the treatment.

In addition to the phone survey, we gathered demographic, electoral, and budgetary data from official sources for all municipalities for which such data is available.[28] It is not possible to observe tax compliance itself in the administrative data so our primary outcome is whether municipalities implemented the policy. Since concerns about experimenter demand effects or other reporting biases may arise for reports from mayors, we separately report responses from tax department bureaucrats and mayors.

4.5 Results

Participation in information sessions. 37.9 percent of the mayors in the treatment group chose to attend our session. In contrast, less than 1 percent of control group mayors attended the session. The opportunity costs of attending—foregoing the opportunity to attend other parallel training sessions or conducting meetings with other politicians and officials—were high. Moreover, some mayors did not have accurate contact information stored in the CNM system, and thus did not receive our invitation messages at all. We therefore consider 37.9 percent to be a fairly high rate of treatment group participation.

Appendix Table A.5 reports predictors of participation in the research information session. Younger and college-educated mayors are 7 and 15 percentage points more likely to participate

[27] We were not able to make any contact with 10 percent of the sample municipalities, due to not being able to locate a working phone number. This share was also balanced across treatment and control groups. On average, many hours of work were needed before we could talk to the chief tax bureaucrats and mayors over the phone, mainly collecting municipalities' phone numbers. Not all Brazilian municipalities publish or have updated contact information on their websites, so we collected phone numbers through google searches, facebook, by calling other local institutions such as hospitals and schools, etc.

[28] Demographic data is available from the Brazilian Statistical Office (IBGE). Brazil's Superior Electoral Court provides data on electoral outcomes and mayors' characteristics. Budgetary data was retrieved from the National Treasury, which compiles and releases self-reported accounting records from all Brazilian municipalities every year.

than others, but term-limited mayors are no less likely to participate than mayors in their first term. None of the municipal characteristics, such as poverty rates, inequality, or income per capita, predict participation.

Policy adoption. We find that a mayor attending the research information session leads to a 10 percentage point increase in the use of reminder letters to taxpayers—an increase of 33 percent over the proportion of control group municipalities that had started using such a reminder policy at some point in the past. Table 8 presents Treatment-on-Treated (ToT) estimates, using randomized treatment status as an instrument for participation in the information session.[29] The outcome variable is a dummy equal to one if the respondent reports that the carefully-described policy is used in their municipality and zero otherwise. Standard errors are clustered at the municipality level. In Column 1, the ToT coefficient is 10.3 percentage points (s.e.=5.3 percentage points), compared to a base of 31.7 percentage points in the control group. Adding controls in Column 2 leaves the point estimate largely unchanged. Column 3 drops respondents who failed an attention check, again leaving the coefficient unchanged.[30] Most importantly, the point estimates are very similar if we restrict attention to responses from mayors (Column 4) or tax department officials (Column 5). Given that we have little concern about tax department officials misreporting details of tax compliance policies differentially between treatment and control groups, this increases our confidence that the effects we estimate are not driven by reporting biases.

Appendix Tables A.7 and A.8 investigate heterogeneity in treatment effects by mayor's and municipal characteristics. We find suggestive evidence that leftist mayors respond more to receiving research information, but the estimates are not precise enough for statistical significance. We have little evidence for heterogeneity by term-limits for mayors, population size of municipality, and measures of municipal government human capital. Appendix Table A.9 reports effects separately for the different design components of taxpayer reminder letters, and shows that the effects are fairly similar on the probability of using letters emphasizing the due date, mentioning the threat of audits/penalties, and mentioning social norm language, although the latter is a larger effect in relative terms, since it is particularly unlikely to be used in the control municipalities. Finally, Appendix Table A.10 reports no effects on a placebo question (the use of e-procurement in municipal government), and reports no effects on the use of financial incentives for compliance with taxes—a common policy which might conceivably have been seen as a substitute for the reminder-letters policy.

Beliefs. We also measured beliefs about the effectiveness of reminder letters, which—especially given the evidence presented in Section 3—we consider a plausible mechanism through which the ultimate impact on policy adoption may arise. We asked respondents about the likely effect of the policy in their municipality, whether or not the policy was currently implemented. We compare their stated beliefs with the estimated effect sizes shared with participating mayors in

[29] Appendix Table A.6 presents the Intent-to-Treat estimates.

[30] The attention check was: "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988". Since we consider this exceedingly unlikely as text for a tax reminder, we infer that respondents who answer 'yes' to this question are simply not paying attention or following the questions.

the research information session and policy briefs provided.[31] Appendix Table A.11 reports that beliefs are more accurate in the treatment group even 15-24 months after treatment. Specifically, the absolute deviation of beliefs from the effect sizes mentioned in the research information session is 20 percent lower than in the control group. The fact that the beliefs are more accurate not just among mayors, but also among tax-department bureaucrats, implies information-flow within the municipal government, perhaps made easier by providing the participants with shareable policy briefs.

4.6 Policy-adoption experiment: discussion

This experiment has one simple but important result: when political leaders in Brazil are provided information from research on the impact of a cost-effective policy, they change the actual policies in use in their polities. This implies that, consistent with the findings from our demand/beliefs experiment, policy-makers are open to new evidence; care about policy effectiveness; and have at least some capacity and desire to translate evidence into policy change.

Two caveats to this interpretation are worth noting. First, we cannot rule out that the estimated effects are driven in part by mayors simply learning of the existence of taxpayer reminder policies, rather than due to the quantitative estimates of their impact from research. As noted above, however, taxpayer reminder policies are far from unknown in Brazil, with about a third of municipalities already using some form of such reminders. Moreover, we found evidence of more accurate beliefs in the treatment group. Thus, it seems unlikely that the impact on policy adoption would have been the same if we had merely described the policy. Second, we considered a policy that is inexpensive and relatively easy to implement. Other effective policies may have higher up-front costs, be more technically demanding, or be more politically sensitive, in which case changing beliefs about effectiveness may not as readily translate into policy change. On the other hand, our policy adoption experiment also does not capture the numerous, less direct channels through which research may ultimately influence policy practice.

5 Conclusion

Policy is important for economic development. What role can policy-effectiveness research play in spurring the spread of effective policies and the abandonment of ineffective ones? One possibility is that lack of (access to) research information is not a binding constraint on policy choice, for example because political leaders are self-interested and electoral competitive pressures too weak to motivate the effort required to change policy, or because leaders have limited real power over the policies in use. Alternatively, frictions may constrain political leaders' access to existing research.

[31] Mayors were informed that the estimated effect sizes found in the existing research studies were 10 percent, 12 percent, and 20 percent, depending on the variants of the policy (i.e. tax payment due dates, risk of audits/penalties, and social norms regarding paying taxes).

In this paper, we investigate how informing political leaders about research findings affects policy beliefs and practice. Using experiments with the elected heads of Brazil's local governments—mayors—we first show that political leaders value access to impact evaluations, and update their beliefs when informed of the research findings. Mayors (and other local policy-makers in our sample) appear to be fairly sophisticated consumers of accessible research, for example paying more for studies—such as those with a large sample size—that subsequently affect their beliefs more. In the second half of the paper, we show that providing mayors with research findings documenting positive impact of an inexpensive and easy-to-implement policy increases the probability that their municipality implements the policy by 10 percentage points. Making research information directly and easily available to policy-makers therefore appears to influence policy. This suggests that information frictions may play an important role in explaining failures to adopt effective policies.

It is arguably surprising that such information frictions persist. After all, even if political leaders themselves do not read academic journals, information frictions should generate incentives for actors interested in enhancing social welfare to access academic research and connect policy research with practice to eliminate these frictions. Empirically, the reach of think tanks and other organizations that institutionalize and scale up transmission of research findings to political leaders still appears limited in developing countries. We hope that future research will expand our understanding of how research's impact on policy practice can be better understood and enhanced.

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TABLE 1: DEMAND/BELIEFS EXPERIMENT: SUMMARY STATS AND BALANCE

VARIABLES	Mean Control	Δ Developing	Δ Large
Mayors' Characteristics			
Male	87.87	-1.3211	-4.0462***
Age	48.45	-0.0997	-0.4731
College	58.43	2.7315	0.0545
2nd Term	19.97	0.3243	-0.1866
Leftist Political Party	37.57	-1.0846	1.7034
Municipalities' Characteristics			
Population	24.39	1.4364	1.2248
College Population	484.73	-7.6688	1.7868
Public Adm College	33.11	-0.9649*	-0.8442
Poverty	26.62	0.4999	0.4229
Gini	49.91	0.4802*	0.4444
Big South	49.26	-0.8158	-1.1354
Per Capita Income	0.45	-0.0280	-0.0177
Kids in School (0-3)	19.24	-1.0415**	0.0754
Kids in School (4-5)	78.68	-0.4169	0.1541
ECD Policy Survey Characteristics			
Mayor	50.59	-0.2295	-1.4094
Prof Politician	29.88	0.7786	-0.4208
Leftist Scale	21.89	-2.4781	-1.9016
Implemented ECD	41.86	0.3214	-3.1046
Heard ECD	25.74	-0.7741	-0.0720
Observations	676	1,371	1,371
Round	1 and 2	1 and 2	1 and 2
Clusters	676	766	766

Notes: OLS results. Sample mean of control observations. Control observations are those for which dummy developing and dummy large are equal to zero, where dummy developing is equal to one for Jamaica and Colombia, and dummy large is equal to one for Colombia and US. Each cell reports the estimated coefficient of a regression of each characteristic on a dummy which is equal to one for Jamaica and Colombia and zero otherwise (Developing), and on a dummy which is equal to one for Colombia and US and zero otherwise (Large). Male-Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms), are characteristics of the mayor that runs the municipality. Population indicates municipality number of inhabitants (in thousands). College Population indicates municipality share of adults with college degrees. Public Administration College indicates the share of municipal public employees with college degrees. Poverty refers to municipalities' poverty rate. Gini refers to the Gini coefficient of municipality. Big South indicates the share of municipalities from the south, southeast and mid-west regions; and 0 are north and northeast regions. Per Capita Income indicates municipality monthly income per capita. Kids in School (0-3) indicates municipality share of kids 0-3 years old that attend pre-school education. Kids in School (4-5) indicates municipality share of kids 4-5 years old that attend pre-school education. Mayor-Heard ECD are characteristics self-reported by participants in the survey experiment. Professional Politician indicates whether the participant occupied an elective position in the previous term. Leftist Scale indicates whether the participant self-identified as leftist (0-4) on a 0-10 scale. Implemented ECD indicates whether the participant reported the municipality implemented a ECD program before. Heard ECD indicates whether the participant reported that he/she had heard about ECD programs before. Robust standard errors are clustered at the individual level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 2: DEMAND/BELIEFS EXPERIMENT: WILLINGNESS TO PAY BY STUDY CHARACTERISTICS

	(1)	(2)	(3)
LHS Variable	WTP	WTP	WTP
Large	3.7704***	2.3414	4.3478***
	(0.7908)	(2.3956)	(1.0160)
Developing	0.3332	1.5766	-0.3308
	(0.7902)	(2.3964)	(1.0038)
Observations	2,578	766	1,812
Round	1 and 2	1	2
Clusters	766	766	605
Mean LHS	44.73	48.52	43.12
SD LHS	31.84	33.12	31.15

Notes: OLS results. The dependent variable is willingness to pay, which is elicited in two different rounds. Developing is a dummy which is equal to one for Jamaica and Colombia and zero otherwise. Large is a dummy which is equal to one for Colombia and US and zero otherwise. Difference in number of clusters between columns 2 and 3 is due to a different experimental design of last CNM conference, in which only one study was offered for purchase. Mean LHS is the mean WTP on the left-hand side of each equation. SD LHS is the standard deviation of WTP on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. P-value of Large (column 2) = Large (column 3) test is .496. P-value of Developing (column 2) = Developing (column 3) test is .516.

TABLE 3: DEMAND/BELIEFS EXPERIMENT: BELIEF UPDATING

	(1)	(2)	(3)	(4)	(5)
LHS Variable	Posterior	Posterior	Posterior	Posterior	Posterior
Prior	0.6850***	0.5906***	0.8066***	0.5512***	0.6845***
	(0.0221)	(0.0295)	(0.0248)	(0.0302)	(0.0247)
Signal	0.3190***	0.3729***	0.2476***	0.4166***	0.3214***
	(0.0199)	(0.0261)	(0.0241)	(0.0301)	(0.0223)
Observations	1,188	702	486	544	929
Round	1 and 2	1	2	1	1 and 2
Beliefs About	Municipality	Municipality	Municipality	Random Study	Municipality
BDM Price = 0	No	No	No	No	Yes
Clusters	702	702	486	544	529

Notes: OLS results. The dependent variables are posterior beliefs, which are declared after successfully buying the results from a study in each round. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. Difference in clusters between columns 2 and 3 is due in part to a different experimental design of last CNM conference, in which only one study was offered for purchase. Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality (columns 1, 2, 3, and 5) or one of the four possible study locations (column 4). BDM Price = 0 indicates whether participant received the information regardless of their WTP. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 4: DEMAND/BELIEFS EXPERIMENT: BELIEF UPDATING: WEIGHT PLACED ON LARGE-SAMPLE AND DEVELOPING-COUNTRY STUDIES

	(1)	(2)	(3)	(4)	(5)
LHS Variable	Posterior	Posterior	Posterior	Posterior	Posterior
Prior	0.6434***	0.5600***	0.7715***	0.6685***	0.6701***
	(0.0374)	(0.0531)	(0.0477)	(0.0543)	(0.0408)
Signal	0.3200***	0.3738***	0.2409***	0.3351***	0.3018***
	(0.0287)	(0.0398)	(0.0386)	(0.0429)	(0.0320)
Prior*Developing	-0.0161	-0.0255	-0.0269	-0.0920	-0.0154
	(0.0401)	(0.0599)	(0.0501)	(0.0574)	(0.0451)
Signal*Developing	0.0231	0.0128	0.0487	0.0682	0.0350
	(0.0356)	(0.0516)	(0.0488)	(0.0578)	(0.0390)
Prior*Large	-0.0567	-0.0896	-0.0373	-0.0558	-0.0998*
	(0.0492)	(0.0689)	(0.0633)	(0.0713)	(0.0543)
Signal*Large	0.3377***	0.4072***	0.2736***	0.2729**	0.3880***
	(0.0736)	(0.0959)	(0.1035)	(0.1170)	(0.0845)
Observations	1,188	702	486	544	929
Round	1 and 2	1	2	1	1 and 2
Beliefs About	Municipality	Municipality	Municipality	Random Study	Municipality
BDM Price = 0	No	No	No	No	Yes
Clusters	702	702	486	544	529
P-value Prior*Dev.=Signal*Dev.	0.582	0.717	0.419	0.142	0.525
P-value Prior*Large=Signal*Large	0.001	0.002	0.051	0.071	0.000

Notes: OLS results. The dependent variables are posterior beliefs, which are declared after successfully buying the results from a study in each round. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. Developing is a dummy which is equal to one for Jamaica and Colombia and zero otherwise. Large is a dummy which is equal to one for Colombia and US and zero otherwise. Difference in clusters between columns 2 and 3 is due in part to a different experimental design of last CNM conference, in which only one study was offered for purchase. Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality (columns 1, 2, 3, and 5) or one of the four possible study locations (column 4). BDM Price = 0 indicates whether participant received the information regardless of their WTP. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 5: DEMAND/BELIEFS EXPERIMENT: TESTING FOR ASYMMETRIC UPDATING AND CONFIRMATION BIAS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Priors	Full Sample	Above the Median	Below the Median	Top Quartile	Bottom Quartile	Above/Below Median	Top/Bottom Quartile
LHS Variable	Posterior	Posterior	Posterior	Posterior	Posterior	Posterior	Posterior
Prior	1.0196***	0.9885***	1.1833***	0.9871***	1.2293***	1.0167***	1.0128***
	(0.0218)	(0.0250)	(0.0612)	(0.0280)	(0.0859)	(0.0158)	(0.0187)
Signal-Prior	0.3489***	0.3104***	-0.0306	0.3106***	-0.4659	0.3369***	0.3440***
	(0.0522)	(0.0573)	(0.3009)	(0.0609)	(0.7954)	(0.0257)	(0.0288)
Signal-Prior * Positive Surprise	-0.0406	0.0025	0.2889	-0.0199	0.7351		
	(0.0690)	(0.1051)	(0.3215)	(0.1271)	(0.8146)		
Signal-Prior * Confirming News						-0.0939	-0.1052
						(0.0620)	(0.0823)
Observations	929	548	381	440	276	929	716
Round	1 and 2	1 and 2	1 and 2	1 and 2	1 and 2	1 and 2	1 and 2
Beliefs About	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality
BDM Price = 0	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	529	375	278	316	211	529	459
P-value (Signal-Prior)=Positive Surprise (Above vs Below Median)			0.3963				
P-value (Signal-Prior)=Positive Surprise (Top vs Bottom Quartile)					0.3573		

Notes: OLS results. The dependent variables are posterior beliefs, which are declared after successfully buying the results from a study in each round. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. Positive Surprise is a dummy which is equal to one if the bought study's effect is greater than the respondent's prior about the effect. Confirming News is a dummy which is equal to one if the respondent's prior about the effect was above the median/top quartile (below the median/bottom quartile) and the bought study's effect is greater (smaller) than the respondent's prior. Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality or one of the four possible study locations. BDM Price = 0 indicates whether participant received the information regardless of their WTP. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 6: DEMAND/BELIEFS EXPERIMENT: PARTISAN SUPPORT

	(1)	(2)	(3)	(4)	(5)	(6)
Political View	Any	Any	Left	Right	Any	Any
LHS Variable	Prior	WTP	Posterior	Posterior	Posterior	Posterior
Partisan Message	-0.0215	-1.7860				
	(0.0273)	(4.1797)				
Leftist	-0.0433	-6.9875				
	(0.0411)	(6.1908)				
Partisan Message*Leftist	0.0775	10.6305				
	(0.0609)	(8.6526)				
Prior			0.9418***	1.0487***	1.0271***	1.0034***
			(0.0721)	(0.0381)	(0.0344)	(0.0282)
Signal-Prior			0.2723	0.4572***	0.4134***	0.3055***
			(0.1764)	(0.0992)	(0.0874)	(0.0411)
Signal-Prior * Positive Surprise			0.1478	-0.2038*	-0.1287	
			(0.2282)	(0.1222)	(0.1065)	
Signal-Prior * Positive Surprise * Partisan Message			-0.0356	0.0491		
			(0.1555)	(0.0822)		
Signal-Prior * Positive Surprise * Leftist					0.0918	
					(0.1309)	
Signal-Prior * Positive Surprise * Leftist * Partisan Message					-0.0404	
					(0.1535)	
Signal-Prior * Partisan-Preferred Signal						0.1671*
						(0.0895)
Signal-Prior * Partisan-Preferred Signal * Partisan Message						-0.1564
						(0.1057)
Observations	312	881	71	232	303	303
Round	1	1 and 2	1 and 2	1 and 2	1 and 2	1 and 2
Beliefs About			Municipality	Municipality	Municipality	Municipality
BDM Price = 0			Yes	Yes	Yes	Yes
Clusters	312	341	40	128	168	168
Mean LHS	0.388	49.25				

Notes: OLS results. In column 1, the dependent variable is respondents' priors. In column 2, the dependent variable is willingness to pay for studies. In columns 3-6, the dependent variable is posterior beliefs, which are declared after successfully buying the results from a study in each round. Partisan Message is a dummy variable which takes the value of 1 if the respondent received a message linking early childhood education policies to a leftist party in Brazil. Leftist is a dummy which is equal to one if the participant self-identified as leftist (0-4) on a 0-10 scale. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality or one of the four possible study locations. BDM Price = 0 indicates whether participant received the information regardless of their WTP. Mean LHS is the average of the left-hand side variable of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 7: POLICY-ADOPTION EXPERIMENT: SUMMARY STATS AND BALANCE

	at Baseline		at Endline	
VARIABLES	Mean Control	Δ Treatment	Mean Control	Δ Treatment
Mayors' Characteristics				
Male	88.26	1.41	90.01	-0.14
Age	46.76	1.32***	47.08	1.61***
College	57.74	-0.76	57.66	0.73
2nd Term	15.69	1.56	15.18	0.91
Leftist Political Party	32.98	2.10	32.76	1.36
Municipalities' Characteristics				
Population	20.86	-0.06	20.23	0.06
College Population	5.17	-0.15	5.47	-0.14
Public Adm College	32.60	0.84	33.51	-0.01
Poverty	26.41	-0.27	23.05	0.11
Gini	50.33	-0.19	49.37	0.17
Big South	51.01	-0.62	59.92	-2.36
Per Capita Income	457.64	3.42	489.23	2.78
Local Taxes Revenues (2010-15)	6.06	0.09	6.40	0.08
Joint F-test (p-value)		0.18		0.17
Follow-Up Survey Response Rate				
Municipality			80.15	1.69
Mayor			51.65	-2.28
Finance Staff			75.03	0.80

Notes: Sample means by experimental group and differences in means between groups at baseline and endline. There were 937 (751) municipalities in control group and 881 (721) in treatment group at baseline (endline). Male-Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms), are characteristics of the mayor that runs the municipality. Population indicates municipality number of inhabitants (in thousands). College Population indicates municipality share of adults with college degrees. Public Administration College indicates the share of municipal public employees with college degrees. Poverty refers to municipalities' poverty rate. Gini refers to the Gini coefficient of municipality. Big South indicates the share of municipalities from the south, southeast and mid-west regions; and 0 are north and northeast regions. Per Capita Income indicates municipality monthly income per capita. Local Tax Revenues (2010-2015) indicates the average share of municipal tax revenues on total municipal revenues from 2010 to 2015. Joint significance F-test p-value, and follow-up survey response rate—municipality, mayor and finance staff—at endline.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 8: POLICY-ADOPTION EXPERIMENT: POLICY ADOPTION: TAX REMINDERS

LHS Variable	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted
Information Session	0.1031*	0.1073**	0.1011*	0.1148	0.1076*
	(0.0531)	(0.0522)	(0.0539)	(0.0776)	(0.0649)
Observations	2,271	2,269	2,054	912	1,357
Respondent	All	All	All	Mayor	Finance Staff
Attention Check	No	No	Yes	No	No
Mayor Characteristics	No	Yes	Yes	Yes	Yes
Municipal Characteristics	No	Yes	Yes	Yes	Yes
Clusters (Municipalities)	1465	1464	1412	912	1357
Mean Control	0.317	0.317	0.298	0.367	0.283

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality, and 0 otherwise. Information Session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Attention Check refers to whether respondents that answered positively to the attention check component of the reminders policy are excluded from the model, where the attention check was "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988"'. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0) and Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms). Municipalities' characteristics included in the model are: Population above-below median (1/0); College Population above-below median (1/0); College Public Administration employees above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Big South (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions); monthly Per Capita Income above-below median (1/0); Local Tax Revenues share above-below median (1/0);. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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TABLE A.1: DEMAND/BELIEFS EXPERIMENT: PRIOR BELIEFS

Beliefs About	N	Mean	SD	Effect Size in Study
Own Municipality	900	0.40	(0.22)	-
123; Michigan	145	0.46	(0.22)	0.87
130; Jamaica	160	0.41	(0.20)	0.91
1420; Colombia	152	0.38	(0.19)	0.26
4667; USA	148	0.49	(0.21)	0.15

Notes: Sample mean and standard deviation of participants' priors. Beliefs About refers to the location about which the prior is asked. N indicates the number of observations in each location, and the last column reports the effect size described in the corresponding study.

TABLE A.2: DEMAND/BELIEFS EXPERIMENT: WILLINGNESS TO PAY: OTHER DETERMINANTS

	(1)	(2)	(3)	(4)
LHS Variable	WTP	WTP	WTP	WTP
Mayors' Characteristics				
Male	6.63**			6.34**
	(3.21)			(3.19)
Age	-1.43			-1.63
	(2.11)			(2.12)
College	1.79			3.14
	(2.16)			(2.21)
2nd Term	2.55			2.89
	(2.58)			(2.87)
Leftist Political Party	1.49			0.93
	(2.13)			(2.20)
Municipalities' Characteristics				
Population		1.95		1.29
		(2.24)		(2.22)
College Population		-0.10		0.00
		(2.78)		(2.81)
Public Adm College		1.66		0.23
		(2.30)		(2.31)
Poverty		1.82		0.56
		(5.70)		(5.61)
Gini		-0.91		-0.70
		(2.51)		(2.49)
Big South		0.36		2.23
		(4.66)		(4.66)
Per Capita Income		-0.74		-0.91
		(5.10)		(4.96)
Kids in School (0-3)		1.37		0.87
		(2.32)		(2.33)
Kids in School (4-5)		1.99		1.95
		(2.42)		(2.39)
ECD Policy Survey Characteristics				
Mayor			-0.89	-0.95
			(2.08)	(2.16)
Prof Politician			-0.69	-1.58
			(2.34)	(2.47)
Leftist Scale			0.51	0.13
			(2.51)	(2.57)
Implemented ECD			11.69***	11.90***
			(2.39)	(2.50)
Heard ECD			6.82**	6.54**
			(2.68)	(2.77)
Observations	2,578	2,578	2,578	2,578
Clusters	766	766	766	766
Mean LHS	44.73	44.73	44.73	44.73

Notes: OLS results. The dependent variable is willingness to pay, which is elicited in two different rounds. We expressed all continuous variables as indicators of above/below the median of the distribution of municipalities. Mayors' characteristics: Male (1/0); Age above-below median (1/0); College (1/0); 2nd term (1/0); Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms). Municipalities' characteristics: Population above-below median (1/0); College Population above-below median (1/0); Public Administration College above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Big south (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions); Per Capita Income above-below median; Kids in School (0-3) above-below median (1/0) of the share of kids 0-3 years old that attend pre-school education; Kids in School (4-5) above-below median (1/0) of the share of kids 4-5 years old that attend pre-school education. ECD Policy Survey characteristics: Mayor (1/0); Professional Politician (1/0); Leftist Scale (1/0); Implemented ECD (1/0) indicates whether the participant reported the municipality implemented a ECD program before; Heard ECD (1/0) indicates whether the participant reported that he/she had heard about ECD programs before. Mean LHS is the mean WTP on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.3: DEMAND/BELIEFS EXPERIMENT: BELIEF UPDATING: LARGE-SAMPLE AND DEVELOPING-COUNTRY STUDIES - DIFFERENT EFFECT SIZES

	(1)	(2)	(3)	(4)
LHS Variable	Posterior	Posterior	Posterior	Posterior
Prior	0.5871***	0.4270***	0.7073***	0.5587***
	(0.0641)	(0.1160)	(0.0699)	(0.1026)
Signal	0.3274***	0.4087***	0.2647***	0.3677***
	(0.0412)	(0.0663)	(0.0507)	(0.0607)
Prior*Developing	0.0253	0.0574	-0.0135	-0.0606
	(0.0653)	(0.1096)	(0.0792)	(0.0948)
Signal*Developing	0.0142	-0.0069	0.0384	0.0511
	(0.0462)	(0.0747)	(0.0610)	(0.0752)
Prior*Large	-0.0328	0.0062	-0.0527	0.0834
	(0.0771)	(0.1325)	(0.0935)	(0.1217)
Signal*Large	0.3337***	0.2856*	0.4207**	0.1615
	(0.1133)	(0.1560)	(0.1634)	(0.1720)
Prior*Long-run	0.0839	0.1928	0.0717	0.1336
	(0.0859)	(0.1411)	(0.0954)	(0.1258)
Signal*Long-run	0.0097	-0.0537	0.0195	-0.0461
	(0.0675)	(0.1008)	(0.0831)	(0.0942)
Prior*Developing*Long-run	-0.1258	-0.2390	-0.0788	-0.1214
	(0.0975)	(0.1549)	(0.1034)	(0.1299)
Signal*Developing*Long-run	0.1480	0.2835*	0.0804	0.2361*
	(0.0939)	(0.1452)	(0.1076)	(0.1346)
Prior*Large*Long-run	-0.0193	-0.1345	0.1562	-0.1292
	(0.1140)	(0.1730)	(0.1246)	(0.1571)
Signal*Large*Long-run	-0.0442	0.1089	-0.4122*	0.0469
	(0.1750)	(0.2398)	(0.2127)	(0.2541)
Observations	929	493	436	493
Round	1 and 2	1	2	1
Beliefs About	Municipality	Municipality	Municipality	Random Study
BDM Price = 0	Yes	Yes	Yes	Yes
Clusters	529	493	436	493

Notes: OLS results. The dependent variables is posterior beliefs, which are declared after successfully buying the results from a study in each round. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. Developing is a dummy which is equal to one for Jamaica and Colombia and zero otherwise. Large is a dummy which is equal to one for Colombia and US and zero otherwise. Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality or one of the four possible study locations. BDM Price = 0 indicates whether participant received the information regardless of their WTP. Long-run is a dummy which is equal to one (zero) for the six (eight) conferences in which the informed effect sizes were assessed in adulthood for 0.38 (0.91) for Jamaica, 0.50 (0.87) for Michigan, or just continue to be assessed shortly after the intervention was over 0.18 (0.15) for USA, 0.26 (0.26) for Colombia. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.4: DEMAND/BELIEFS EXPERIMENT: VALUATION OF IMPLEMENTATION REPORT

	(1)	(2)
LHS Variable	Implementation Report	Implementation Report
Final Posterior	14.9885***	47.6139**
	(5.4457)	(23.3722)
Observations	685	685
Instruments		Avg Signal
Clusters	685	685
Mean LHS	59.68	59.68

Notes: OLS (column 1) and 2SLS (column 2) results. Dependent variable is willingness to pay for a policy implementation report. Final Posterior is the value of the last updated belief, that being after buying one or two results. Instrument is either the received signal or the average of the received signals in the case the participant have bought two results. Mean LHS is the average policy implementation report valuation on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.5: POLICY-ADOPTION EXPERIMENT: INDIVIDUAL AND MUNICIPAL PREDICTORS OF SESSION PARTICIPATION

LHS Variable	(1) Information Session	(2) Information Session
Mayors' Characteristics		
Male	0.0168	-0.0013
	(0.0546)	(0.0559)
Age	-0.0713**	-0.0776**
	(0.0327)	(0.0335)
College	0.1551***	0.1500***
	(0.0327)	(0.0333)
2nd Term	-0.0051	-0.0005
	(0.0441)	(0.0449)
Leftist Political Party	0.0327	0.0402
	(0.0345)	(0.0350)
Constant	0.3041***	0.3532***
	(0.0619)	(0.1124)
Observations	881	878
Municipal Characteristics	No	Yes
R-Squared	0.0335	0.0416
Municipalities' Characteristics		
Population	-0.0079	-0.0164
	(0.0343)	(0.0339)
College Population	0.0634	0.0421
	(0.0466)	(0.0458)
Public Adm College	-0.0345	-0.0372
	(0.0339)	(0.0335)
Poverty	-0.1015	-0.0739
	(0.0903)	(0.0928)
Gini	0.0449	0.0413
	(0.0382)	(0.0379)
Big South	0.0258	0.0597
	(0.0662)	(0.0661)
Per Capita Income	-0.0762	-0.0642
	(0.0839)	(0.0855)
Local Tax Revenues (2010-2015)	-0.0245	-0.0119
	(0.0459)	(0.0451)
Constant	0.4343***	0.3532***
	(0.0937)	(0.1124)
Observations	878	878
Individual Characteristics	No	Yes
R-Squared	0.0084	0.0416

Notes: Linear probability results. Response variable is information session participation and takes the value of 1 for mayors that attended the information session and 0 otherwise. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0) and Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms). Municipalities' characteristics included in the model are: Population above-below median (1/0); College Population above-below median (1/0); College Public Administration employees above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Big South (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions); monthly Per Capita Income above-below median (1/0); Local Tax Revenues share above-below median (1/0);. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.6: POLICY-ADOPTION EXPERIMENT: ITT POLICY ADOPTION: TAX REMINDERS

LHS Variable	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted
Treatment Assignment	0.0402*	0.0422**	0.0392*	0.0469	0.0412*
	(0.0208)	(0.0206)	(0.0210)	(0.0321)	(0.0250)
Observations	2,271	2,269	2,054	912	1,357
Respondent	All	All	All	Mayor	Finance Staff
Attention Check	No	No	Yes	No	No
Mayor Characteristics	No	Yes	Yes	Yes	Yes
Municipal Characteristics	No	Yes	Yes	Yes	Yes
Clusters (Municipalities)	1465	1464	1412	912	1357
Mean Control	0.317	0.317	0.298	0.367	0.283

Notes: OLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Treatment Assignment is a dummy which takes the value of 1 if the mayor was assigned to the treatment group. Attention Check refers to whether those observations that answer positively to the attention check component of the reminders policy are excluded from the model, where the attention check was "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988". Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0) and Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms). Municipalities' characteristics included in the model are: Population above-below median (1/0); College Population above-below median (1/0); College Public Administration employees above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Big South (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions); monthly Per Capita Income above-below median (1/0); Local Tax Revenues share above-below median (1/0);. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.7: POLICY-ADOPTION EXPERIMENT: POLICY ADOPTION: MAYORS' HETEROGENEITIES ABOUT TAX REMINDERS

LHS Variable	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted
Characteristic	Male	Age	College	2nd Term	Leftist Party
Information Session	-0.0544	0.1515**	0.1481	0.1084*	0.0700
	(0.2007)	(0.0661)	(0.1051)	(0.0588)	(0.0691)
Information Session*Characteristic	0.1715	-0.1045	-0.0663	-0.0321	0.0913
	(0.2081)	(0.1076)	(0.1212)	(0.1373)	(0.1072)
Characteristic	0.0077	0.0191	0.0081	0.0149	-0.0234
	(0.0483)	(0.0288)	(0.0293)	(0.0398)	(0.0301)
Observations	2,271	2,271	2,271	2,271	2,271
Respondent	All	All	All	All	All
Attention Check	No	No	No	No	No
Clusters (Municipalities)	1465	1465	1465	1465	1465
Mean Control	0.317	0.317	0.317	0.317	0.317

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Information Session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Attention Check refers to whether respondents that answered positively to the attention check component of the reminders policy are excluded from the model, where the attention check was "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988"'. Mayors' characteristics included as interactions are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0) and Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms). Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.8: POLICY-ADOPTION EXPERIMENT: POLICY ADOPTION: MUNICIPALITIES' HETEROGENEITIES ABOUT TAX REMINDERS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LHS Variable	Adopted	Adopted	Adopted	Adopted	Adopted	Adopted	Adopted	Adopted
Characteristic	Population	College Pop	PubAdm College	Poverty	Gini	Big South	Pc Income	Local Taxes
Information Session	0.1289*	0.0836	0.1180	0.1266*	0.1709**	0.1001	0.0992	0.0705
	(0.0743)	(0.0749)	(0.0731)	(0.0740)	(0.0803)	(0.0877)	(0.0770)	(0.0754)
Information Session*Characteristic	-0.0515	0.0351	-0.0320	-0.0437	-0.1155	0.0130	0.0111	0.0557
	(0.1060)	(0.1061)	(0.1062)	(0.1057)	(0.1066)	(0.1099)	(0.1057)	(0.1060)
Characteristic	0.0110	0.0434	0.0317	-0.0743***	-0.0555*	0.0855***	0.0903***	0.0314
	(0.0287)	(0.0287)	(0.0288)	(0.0286)	(0.0288)	(0.0294)	(0.0286)	(0.0288)
Observations	2,271	2,271	2,271	2,271	2,271	2,271	2,271	2,269
Respondent	All	All	All	All	All	All	All	All
Attention Check	No	No	No	No	No	No	No	No
Clusters (Municipalities)	1465	1465	1465	1465	1465	1465	1465	1464
Mean Control	0.317	0.317	0.317	0.317	0.317	0.317	0.317	0.317

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Information Session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Attention Check refers to whether respondents that answered positively to the attention check component of the reminders policy are excluded from the model, where the attention check was "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988"'. Municipalities' characteristics included in the model are: Population above-below median (1/0); College Population above-below median (1/0); College Public Administration employees above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Big South (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions); monthly Per Capita Income above-below median (1/0); Local Tax Revenues share above-below median (1/0);. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.9: POLICY-ADOPTION EXPERIMENT: POLICY ADOPTION: TAX REMINDERS INFORMATION COMPONENTS

	(1)	(2)	(3)	(4)	(5)
LHS Variable	On Time	Audit	Social Norm	Before Due	Letter
Information Session	0.1019**	0.0703	0.1048***	0.0881*	0.0853**
	(0.0518)	(0.0469)	(0.0370)	(0.0511)	(0.0408)
Observations	2,269	2,269	2,269	2,269	2,269
Respondent	All	All	All	All	All
Attention Check	No	No	No	No	No
Mayor Characteristics	Yes	Yes	Yes	Yes	Yes
Municipal Characteristics	Yes	Yes	Yes	Yes	Yes
Clusters (Municipalities)	1464	1464	1464	1464	1464
Mean Control	0.313	0.224	0.113	0.293	0.156

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the information component of the policy was adopted in municipality, and 0 otherwise. On Time refers to a reminder message highlighting the tax payment deadline. Audit refers to a reminder message highlighting the risks of audits for not paying taxes on time. Social Norm refers to a reminder message highlighting the social norm of paying taxes. Before due refers to sending the reminder message before taxes' due date. Letter refers to sending the reminder message using a hard copy letter. Information Session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Attention Check refers to whether respondents that answered positively to the attention check component of the reminders policy are excluded from the model, where the attention check was "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988" Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0) and Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms). Municipalities' characteristics included in the model are: Population above-below median (1/0); College Population above-below median (1/0); College Public Administration employees above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Big South (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions); monthly Per Capita Income above-below median (1/0); Local Tax Revenues share above-below median (1/0);. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.10: POLICY-ADOPTION EXPERIMENT: POLICY ADOPTION: FINANCIAL INCENTIVES AND E-PROCUREMENT

	(1)	(2)
LHS Variable	Financial Incentives	E-procurement
Information Session	0.0077	0.0254
	(0.0553)	(0.0643)
Observations	2,206	1,694
Respondent	All	All
Attention Check	No	No
Mayor Characteristics	Yes	Yes
Municipal Characteristics	Yes	Yes
Clusters (Municipalities)	1451	1190
Mean Control	0.602	0.449

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Information Session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Attention Check refers to whether respondents that answered positively to the attention check component of the reminders policy are excluded from the model, where the attention check was "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988". Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0) and Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms). Municipalities' characteristics included in the model are: Population above-below median (1/0); College Population above-below median (1/0); College Public Administration employees above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Big South (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions); monthly Per Capita Income above-below median (1/0); Local Tax Revenues share above-below median (1/0);. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.11: POLICY-ADOPTION EXPERIMENT: BELIEFS AND CONFIDENCE: TAX REMINDERS AND FINANCIAL INCENTIVES

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
LHS Variable	Belief-10%	Belief-10%	Belief-10%	Belief-10%	Belief-10%	Belief-10%
Information Session	-1.6003**	-1.7238*	-1.5180*	0.1747	-0.7834	0.6749
	(0.6271)	(0.9202)	(0.7959)	(0.6287)	(0.9431)	(0.8018)
Mean Control	6.775	6.824	6.742	7.172	6.857	7.382
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
LHS Variable	Belief-12%	Belief-12%	Belief-12%	Belief-12%	Belief-12%	Belief-12%
Information Session	-1.3648***	-1.3052*	-1.4196**	0.3033	-0.3511	0.6387
	(0.5170)	(0.7322)	(0.6816)	(0.5239)	(0.7727)	(0.6811)
Mean Control	6.968	6.859	7.041	7.006	6.719	7.198
Panel C	(1)	(2)	(3)	(4)	(5)	(6)
LHS Variable	Belief-20%	Belief-20%	Belief-20%	Belief-20%	Belief-20%	Belief-20%
Information Session	-0.6139	-0.1079	-0.9931	0.7405	0.9865	0.5974
	(0.6884)	(0.9862)	(0.9245)	(0.7336)	(1.0530)	(0.9529)
Mean Control	9.122	8.389	9.616	7.904	7.860	7.933
Panel D	(1)	(2)	(3)	(4)	(5)	(6)
LHS Variable	Confidence	Confidence	Confidence	Confidence	Confidence	Confidence
Information Session	0.0820	0.1072	0.0843	-0.0114	-0.3072*	0.1946
	(0.1063)	(0.1697)	(0.1367)	(0.1122)	(0.1753)	(0.1418)
Mean Control	-0.0171	0.109	-0.102	-0.00291	0.126	-0.0894
Observations	2,184	859	1,325	2,154	844	1,310
Respondent	All	Mayor	Finance Staff	All	Mayor	Finance Staff
Policy	Tax Reminders	Tax Reminders	Tax Reminders	Financial Incentives	Financial Incentives	Financial Incentives
Attention Check	No	No	No	No	No	No
Mayor Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Clusters (Municipalities)	1439	859	1325	1431	844	1310

Notes: 2SLS estimation results. In panels A, B and C, the dependent variable is the absolute difference between self-reported beliefs about effect sizes of policy on local tax revenues, and the informed effect size of the reminder letters policy during the information session. In panel D, the dependent variable is self-reported confidence level about beliefs in a likert-scale of 1 to 5 (standardized to mean 0 and standard deviation 1). Information Session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Attention Check refers to whether respondents that answered positively to the attention check component of the reminders policy are excluded from the model, where the attention check was "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988". Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0) and Leftist Political Party (1/0, mayors belonging to a center-leftist party according to historical political platforms). Municipalities' characteristics included in the model are: Population above-below median (1/0); College Population above-below median (1/0); College Public Administration employees above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Big South (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions); monthly Per Capita Income above-below median (1/0); Local Tax Revenues share above-below median (1/0);. Robust standard errors clustered at the municipality level are in parenthesis.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

FIGURE A.1: DEMAND/BELIEFS EXPERIMENT:
ECD: DESCRIPTION

What is the program? The **early childhood development (ECD) program** consists of play sessions for children from low-income backgrounds, aged 4 months to 5 years old. The intervention is similar to the "Criança Feliz" program in Brazil, and can be implemented through either home visits or at child care centers.

The program is conducted by a public school teacher or a trained health worker. The activities in the play sessions include describing and naming objects in the environment to the children, responding to the child's actions and vocalizations, playing educational games, and using picture books and songs that help in language acquisition.

The program is usually targeted at low-income children and, for example, might involve 1 day per week of activities for 2 years.

Notes: Survey experiment script.

FIGURE A.2: DEMAND/BELIEFS EXPERIMENT:
ECD: GOAL AND MEASURE

What is the goal of such a program? The goal of the program is to increase children's cognitive skills. **Cognitive skills** are the ability to think and understand. They are important for students' academic performance.

How do we measure cognitive skills? Prova Brasil is an example of a tool that measures cognitive skills.

To help you understand the units in which cognitive skills are measured, here is a helpful number: students who have completed one more year of high school have cognitive skills that are 0.2 points higher on average, measured on a test scale such as the Prova Brasil.

So, purely as an example, suppose some policy increased cognitive skills by 0.2 points. That would be a similar-sized effect as one additional year of high school.

Notes: Survey experiment script.

FIGURE A.3: DEMAND/BELIEFS EXPERIMENT:
ECD: BENCHMARKS

Policies vary by the extent to which they affect children's cognitive skills. Some policies have no effect, or even hurt. Others might have a large positive effect. The table below helps you understand how the increase in cognitive skills due to different policies compare to the gain of one additional year of high school.

The policy	The policy increases cognitive skills by:
one additional year of high school	0 0.1 **0.2** 0.3 0.4 0.5 0.6 0.7 0.8 0.9 1
	Cognitive points
Assigning a more experienced teacher to the classroom	0 **0.1** 0.2 0.3 0.4 0.5 0.6 0.7 0.8 0.9 1
A program similar to Bolsa Familia	0 0.1 **0.2** 0.3 0.4 0.5 0.6 0.7 0.8 0.9 1
Replacing group teaching by individual teaching	0 0.1 0.2 0.3 0.4 **0.5** 0.6 0.7 0.8 0.9 1

Notes: Survey experiment script.

FIGURE A.4: DEMAND/BELIEFS EXPERIMENT: PRIOR ELICITING: OWN MUNICIPALITY

Q14. Suppose the Early Childhood Development program is implemented, targeting children from low-income families in **your municipality**.

What do you expect the increase in cognitive skills to be if the program is implemented in your municipality?

0	0,1	0,2	0,3	0,4	0,5	0,6	0,7	0,8	0,9	1

Notes: Survey experiment script.

FIGURE A.5: DEMAND/BELIEFS EXPERIMENT: PRIOR ELICITING: OTHER CONTEXT

Context 2: **Ypsilanti, a city in the state of Michigan, USA**

The early childhood development program was implemented for **123** children from low-income families in **Ypsilanti, a city in the state of Michigan, USA**. What do you think the effect of the program was there?

Note that we will compare your prediction against the answer found by researchers who conducted a scientific study of the program among **123** children from low-income families in **Ypsilanti, a city in the state of Michigan, USA**. The researchers compared the cognitive skills of children randomly assigned to the program with the cognitive skills of those randomly assigned not to receive the program.

The closer your prediction is to the result found by the researchers, the greater the reward you will be given. If your prediction is exactly right, you will receive **100** lottery tickets. If your prediction is as far as possible from the correct answer, you will receive **0** lottery tickets.

Notes: Survey experiment script.

FIGURE A.6: DEMAND/BELIEFS EXPERIMENT: BDM INSTRUCTIONS

So far, we have asked you to share your opinions about the likely effect of the program on children's cognitive skills.

Now, we will offer you the chance to learn the results of studies which have evaluated the program. These studies seek to measure what the true effect of the program was in a certain context. We will give you the chance to purchase access to this answer as described below.

First, we will give you 100 lottery tickets for the lucky draw to win a free trip to USA to visit Harvard University.

Then, you will have the chance to spend some of these tickets to learn about the program effect evaluated by the studies. You will have to tell us the maximum number of lottery tickets out of the 100 that you are willing to give up in order to learn the result of the research.

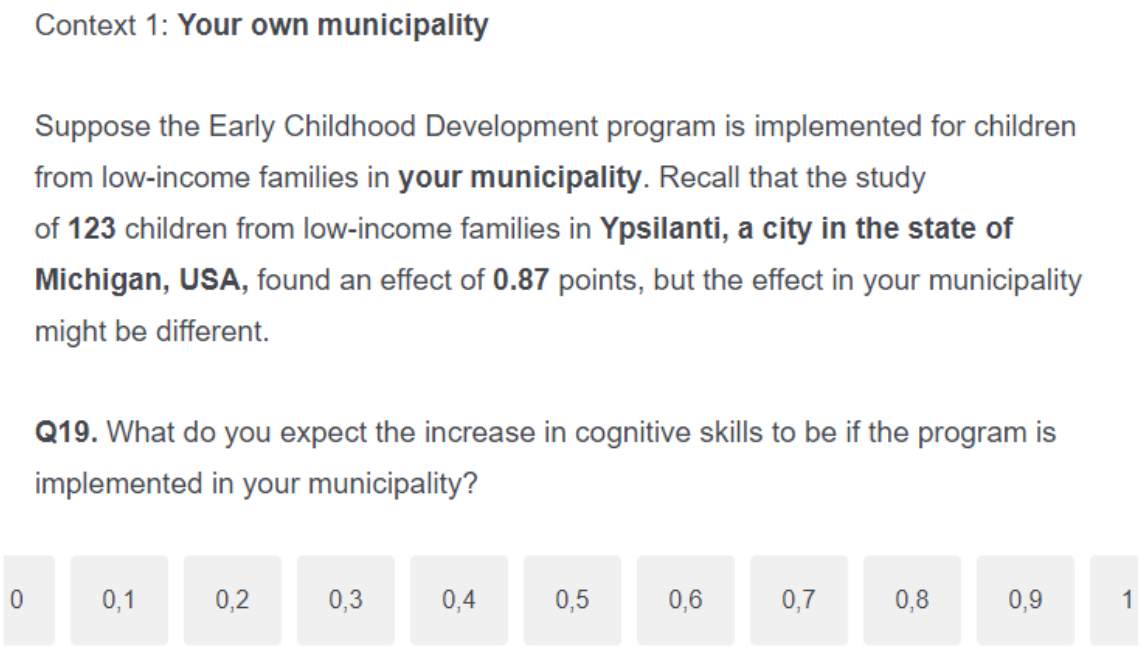
After you tell us the maximum number of tickets you are willing to pay, the computer will randomly select a "price" for the information, which will be between 0 and 100 lottery tickets.

If the price randomly chosen by the computer is above the amount you are willing to pay, you will not pay anything, and you will not learn the result, but you will retain all 100 lottery tickets.

If the price selected by the computer is below the maximum amount you indicated, you will pay only the selected price by the computer, and you will keep the remaining lottery tickets.

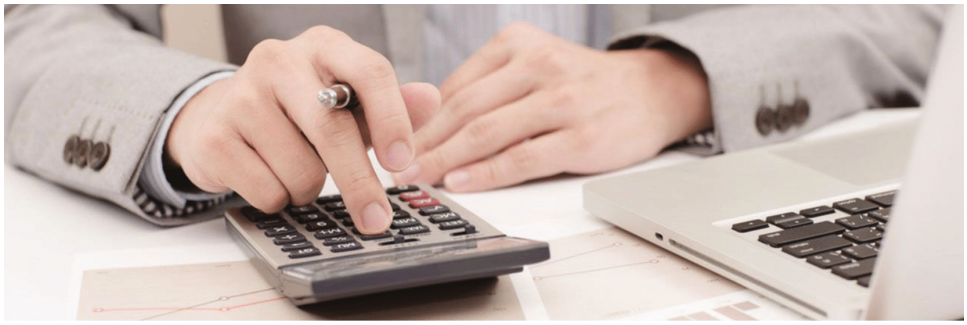
Notes: Survey experiment script.

FIGURE A.7: DEMAND/BELIEFS EXPERIMENT: POSTERIOR ELICITING: OWN MUNICIPALITY



Notes: Survey experiment script.

FIGURE A.8: POLICY-ADOPTION EXPERIMENT: POLICY BRIEF: PAGE 1



HOW TO INCREASE COMPLIANCE WITH LOCAL TAXES

A Policy Brief Based on Scientific Research

INTRODUCTION

Raising tax revenue locally is an important task for municipal governments in Brazil. Local taxes increase the municipal budget, but also provide untied funds which the municipality can spend in line with its own priorities. But municipalities in Brazil face a serious challenge when it comes to collecting local taxes: many businesses and individuals who owe tax payments do not comply with the tax laws by paying the full amounts on time.

Governments throughout the world, including Brazil, have tried many innovative methods to solve this problem. But what works, and what does not? This policy brief provides simple results from scientific research on how governments can increase compliance with taxes.

A LOW-COST AND EFFECTIVE WAY TO INCREASE TAX COMPLIANCE: REMINDER LETTERS

Research conducted in Latin America has revealed one very simple and inexpensive action that has proven to be effective in increasing compliance: **sending taxpayers reminder letters before the due date of the taxes**.[1] For example, an academic researcher worked with two municipal governments in Peru, and found that property tax compliance increased by 10% simply by sending a letter to taxpayers which reminded them of the tax payment deadline![2] Similar results have been found in other studies, including in the United States, Austria and the United Kingdom.[3]

1 Taxpayers are those legally responsible to pay taxes. For instance, taxpayers of the urban property tax (IPTU) are the owners of the property (or the tenants if is explicitly stated in the lease agreement). Taxpayers of the services of any nature tax (ISSQN), are the professionals or businesses that provide the service.
2 Del Carpio (2013)
3 Coleman (1996), Hallsworth et al. (2014), Fellner et al. (2013)

Notes: Taxpayer reminder letters policy brief.

FIGURE A.9: POLICY-ADOPTION EXPERIMENT: POLICY BRIEF: PAGE 2

Research can also guide how to make the reminder letters even more effective. An important policy lesson is that **the letter should emphasize that most people pay their taxes on time**. The same study in Peru found that tax compliance increased by 20% if the reminder letter also included a sentence like "The vast majority of your neighbors pay their taxes on time!" or "75% of your neighbors pay their taxes on time!" Such a message highlights that paying taxes on time is a "social norm", and those who don't pay are deviating from the desirable social norm.

There is one final lesson from research on how to increase the effectiveness of tax reminder letters: **highlight the threat of audits or penalties due to not paying taxes on time**. For example, a study in Argentina found that sending out a letter to property owners (who are supposed to pay property taxes) emphasizing the possible fines and audits due to evading taxes increased tax compliance by 12%.[4]

An important point to keep in mind is that reminder letters are inexpensive to send. All that is needed is for the municipal tax authorities to know the addresses of potential taxpayers. In many cases, letters are already being sent to such taxpayers.

Simply by choosing the correct content of the letter, to remind taxpayers of the payment deadline, to emphasize social norms, and to highlight the threat of audits or penalties, governments have been able to increase tax compliance and revenues, and reduce tax evasion. This can be a very cost-effective policy, and is moreover easy to implement compared to most other strategies to increase tax revenues.[5]

POLICY LESSONS

To summarize, this brief provides a total of **three policy lessons:**

- Send letters to taxpayers reminding them of the deadline to pay taxes.
- Emphasize in the letter that most people pay their taxes on time.
- Highlight the potential bad consequences of avoiding taxes: fines and audits.

An example letter is provided on Page 3 of this policy brief. Contact the Project team at **contato@pesquisadoresdeharvardcnm.com** to receive an electronic copy of the letter.

4 Castro and Scartascini (2013)
5 A cost-effective action is one that produces good results with a small cost.

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Notes: Taxpayer reminder letters policy brief.

FIGURE A.10: POLICY-ADOPTION EXPERIMENT: POLICY BRIEF: PAGE 3

EXAMPLE
REMINDER
LETTER FOR
TAX PAYMENT

A Policy Brief
Based on
Scientific
Research

FEATURING PAYMENT DEADLINE, SOCIAL NORMS, AND THREAT OF PENALTIES

Dear Sir/Madam,

Your municipal tax payments are due by **01 November 2016.**

Our statistics show that the **vast majority of your neighbors will pay their taxes on time.** We greatly appreciate your doing the same.

Don't forget to report your taxes accurately and in a timely manner to avoid the **risk of an audit,** which is a time-consuming and costly process that may lead to substantial financial and other penalties if your tax reporting is found to be wrong.

It is easy to pay your taxes. Please follow the enclosed instructions for more information.

If you have already paid your taxes, thank you very much! If not, please act now.

Yours faithfully,
Name of Tax Authority

How to Increase Compliance with Local Taxes • 3

Notes: Taxpayer reminder letters policy brief.

FIGURE A.11: POLICY-ADOPTION EXPERIMENT: POLICY BRIEF: PAGE 4

REFERENCES OF THE ACADEMIC RESEARCH

How to Increase Compliance with Local Taxes

• Castro, L. and Scartascini, C. (2015). "Tax Compliance and Enforcement in the Pampas. Evidence from a Field Experiment." Journal of Economic Behavior & Organization, 116: 65-82.

Examines provincial property taxpayers in Argentina. Finds a strong effect of reminding taxpayers about the possibility of being audited if they do not pay their taxes.

• Coleman, S. (1996). "The Minnesota Income Tax Compliance Experiment: State Tax Results." MPRA Paper No. 4827, University of Munich.

Examines income taxpayers in Minnesota. Finds a strong effect of a reminder letter emphasizing social norms, and of a letter reminding about the possibility of audits.

• Del Carpio, L. (2013). "Are the Neighbors Cheating? Evidence from a Social Norm Experiment on Property Taxes in Peru." Princeton, NJ: Princeton University Working Paper.

Examines municipal taxpayers in Peru. Finds a strong effect of reminder letters and an even stronger effect of a letter emphasizing social norms.

• Fellner, G., Sausgruber, R., and Traxler, C. (2013). "Testing Enforcement Strategies in the Field: Threat, Moral Appeal, and Social Information." Journal of the European Economic Association, 11: 634-660.

Examines TV license taxpayers in Austria. Finds a strong effect of reminder letter and even stronger effect of a letter reminding about the possibility of audits.

• Hallsworth, M., List, J.A., Metcalfe, R.D., and Vlaev, I. (2014). "The Behavioralist as Tax Collector: Using Natural Field Experiments to Enhance Tax Compliance." NBER Working Paper No. 20007.

Examines income taxpayers in the UK. Finds a strong effect of reminder letter and social norm letter.

• Hasseldine, J., James, S., and Toumi, M. (2007). "Persuasive Communications: Tax Compliance Enforcement Strategies for Sole Proprietors." Contemporary Accounting Research, 24: 171-194.

Examines payers of business tax in the UK. Finds a strong effect of a letter reminding about the possibility of audit.

Notes: Taxpayer reminder letters policy brief.