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

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Abstract

Can 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 their mayors. 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 personally 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 policy-makers value research information, change their beliefs when presented with it, and that this can drive policy change. Information frictions may thus help explain failures to adopt effective policies.

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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 take a first step towards answering these questions by providing evidence from two experiments. 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 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. 764 municipal officials (primarily mayors) from 579 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 of a taxpayer reminder letter policy on tax compliance. We then measured not just beliefs about policy effectiveness, but the actual use of such reminder letters at the municipality level 15 to 24 months later. In combination, the 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 intermediate steps of policy-maker demand and belief change that are one pathway through which research may impact policy.

Brazil's municipalities are an excellent setting to investigate how research affects policy practice for two 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.[2] Second, there are 5,570 municipalities in Brazil, and our collaboration with CNM gives us direct access to their leadership. CNM is a non-partisan organization whose membership comprises thousands of municipal governments. It seeks to provide training and technical support to mayors and municipal managers, and advocates for their interests at the federal level. Working with CNM allowed us to carry out experiments at the polity level.

[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.

Beliefs experiment. Our first experiment finds that the political leaders of Brazil's municipalities exhibit significant personal demand for research and change their beliefs in response to research findings. The policy context for the experiment is Early Childhood Development (ECD) programs, whose impacts on children's test scores have been estimated in existing research. We make use of four 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 program 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 personal 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] To deal with selection into receiving the study findings, a subset of participants receive the findings for free. 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. The entire experiment was self-administered privately by the participant using a tablet.

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 (out of their own pockets) to find out the results of an impact evaluation: about USD 36 on average, 0.4-0.9 percent of a mayor's monthly salary (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 different responses to different studies are in part driven by attributes participants expect to correlate with sample size and 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 report statistical precision as a reason.

Our experiment is not designed to test a model of rational learning. Since we do not measure probabilistic beliefs and only provide participants with point estimates from the studies, we do not know how much participants *should* update their beliefs. We do, however, provide suggestive calculations that policy-maker sensitivity to sample size is lower than a Bayesian model would pre-

[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.

dict. We also document substantial heterogeneity in how much individual policy-makers update in response to information, including about a quarter who do not update at all. Although limited in statistical precision, we find little evidence for two types of motivated reasoning. Specifically, participants on average do not display confirmation bias—they do not interpret information in a way that tends to reinforce their prior and lead to polarization—nor do they respond asymmetrically to good versus bad news regarding the policy (relative to their prior).

Altogether, the findings of our first experiment suggest that on average political leaders value research information and place substantial weight on it, at least once such information is made (easily) accessible. In line with this interpretation, we find that a higher posterior causally increases the policy-maker's WTP for practical information on how to implement the policy. However, important caveats apply. First, the study measures WTP out of the policy-maker's private budget, rather than out of a municipal budget. It thus captures what the policy-maker is personally willing to give up to acquire such information, but not whether they would be willing or able to spend out of government budgets, which may have other, higher-value uses or binding restrictions. Second, it only measures very short-run effects on beliefs. Third, the WTP and overall responsiveness to information may be affected by experimenter demand effects, even though the experiment was privately self-administered on a tablet. The following experiment partially addresses these weaknesses by studying actual policy adoption, a higher-stakes and longer-run outcome.

Policy-Adoption Experiment. In our second experiment, we invited a randomly chosen subset of the mayors attending CNM's 2016 *Novos Gestores* convention in Brasília—the heads of 1,818 municipal governments—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-minute long information session, an experienced local 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.

37.9 percent of the randomly-invited mayors in the treatment group chose to attend the information session. This is arguably a fairly high attendance rate, given that contact information was out-of-date for some mayors, and considering the meaningful opportunity cost: professional networking with other politicians, or attending parallel sessions on other topics which did not emphasize research findings. Younger and college-educated mayors were more likely to attend, while term-limited mayors were no less likely to attend than mayors in their first term.

Attending the research-information session increased the probability that municipalities had

[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).

implemented taxpayer reminders 15-24 months later by 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 attend the information session and to adopt reminder letters as mayors who face re-election incentives.

We interpret the effects on policy adoption as being driven by the provision of research information on policy effectiveness. Consistent with this, we find persistent effects on beliefs about the effectiveness of reminder letters, and evidence that beliefs change not just for the treated mayors, but also among their tax bureaucrats. However, a number of alternative channels and interpretations are important to acknowledge. First, it could be that adoption occurred simply due to learning of the existence of tax reminders. While this is possible, recall that tax reminders are not entirely unknown: a third of the control group already uses them. Second, it could be that the intervention simply raised the salience of tax compliance as a policy goal, leading to the adoption of an already-known policy of reminder letters. Here, it is worth noting that we do not find any effects on the adoption of another commonly-used tax policy, financial incentives for compliance. Third, it could be that some other effective policy was crowded out by the adoption of the reminder letters. We discuss this issue in more detail in Section 4, but note that it does not contradict the conclusion that providing research information changed policy. Finally, it could be that a direct policy recommendation from the experimenters would have similar effects, even shorn of any underlying evidence.

The two experiments have similar structures but different strengths. In the policy-adoption experiment, mayors must pay for the information with their time, belief changes are measured over 15-24 months, we capture belief spillovers to local bureaucrats, and actual policy adoption is observed, albeit for a simple and low-cost policy. But this experiment does not shed light on what type of research information is more or less compelling, nor does it allow us to rigorously study belief-updating due to an inability to measure prior beliefs. In contrast, the beliefs experiment studies belief changes over only a matter of minutes. But it allows us to learn that policy-makers respond to studies differently based on sample size but not location, and to shed light on heterogeneity in belief updating as well as explore deviations from Bayesian learning. While the magnitudes of effects are difficult to compare between the two complementary studies, the findings are qualitatively consistent. Policy-makers are interested in research information; it changes their beliefs; and these changed beliefs can translate into policy change.

Numerous open questions remain. The two experiments studied different policies, which introduces a gap in our argument. Future efforts might measure policy-maker beliefs and information-demand across more policy topics, and examine whether information is a binding constraint not just for the adoption of inexpensive and simple policies such as reminder letters, but also for more

[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. In 81 percent of the municipalities in the sample, at least one public official was surveyed. There was no differential attrition between treatment and control municipalities.

challenging and expensive (but effective) policies such as ECD programs. Understanding the credibility of different information sources is another important question for research. In this project, a trusted partner organization and researchers from reputed universities (Columbia, Harvard and PUC-Rio) organized an information session. Other sources through which research information is encountered, such as local think tanks, academics or media sources, may be received differently. This paper also does not capture the numerous less-direct channels through which research may influence policy, such as by gradually changing ways of thinking, influencing donors and other non-state actors, or informing citizens. 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 already? In the absence of direct outreach from researchers, as in our project, how do policy-makers discover and parse research findings? 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 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 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 constrain 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 (2019) 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.* (2019), Nellis *et al.* (2019), Rogger & Somani (2019) and Vivalt & Coville (2020), who study how the beliefs of policy professionals—program officers, aid-agency workers, and bureaucrats—respond

[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, 2019); Akhtari *et al.* (2018); Bertrand *et al.* (2020); 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.*, 2020; Xu, 2018). For an overview of the literature on politician 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.* (2019)—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 (2019)).

to research findings and new data. Like these papers, we document substantial belief updating among policymakers in response to providing objective evidence. Our main contributions relative to those papers are to study heads of government, to shed light on the kinds of studies such policymakers value and place more weight on, to measure demand for such information, and—most importantly—to provide evidence that research evidence actually translates into changes in policies adopted.[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.*, 2016; 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 beliefs experiment. Section 4 discusses our second intervention, the policy-adoption experiment, and finally we conclude in Section 5.

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 were conducted.

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 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

[11] Banuri *et al.* (2019), Nellis *et al.* (2019), and Vivalt & Coville (2020) study the belief-formation of (mostly U.K. and U.S.-based) policy professionals, while Rogger & Somani (2019) study the beliefs of bureaucrats in Ethiopia. Like Vivalt & Coville (2020), we find some evidence of precision-neglect, but unlike them, we do not find evidence of asymmetric responses to positive and negative news. Like Rogger & Somani (2019), we find little heterogeneity in belief-updating by policy-maker or municipality characteristics. They emphasize heterogeneity by organizational management practices, which we are unable to observe. Another related paper is Beynon *et al.* (2012), who use an online experiment to study the optimal design of policy briefs.

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 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 two or three days long, and are structured around different training sessions conducted by CNM staff and other experts, 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 sometimes the President of Brazil. In addition to attending the presentations, local policy-makers use the meetings to network 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.

[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.

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 consulting 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. We chose two distinct policies for the two experiments, since the beliefs experiment was largely conducted in between the intervention and the endline survey of the policy-adoption experiment. Since we did not want to contaminate the policy-adoption experiment, we were forced to choose a new policy topic when the opportunity to conduct the beliefs experiment arose.

How might the policies and research information we provide relate to mayors' objectives and constraints? The policy tool whose surprising effectiveness we describe in the policy-adoption experiment, tax reminder letters, has the potential to increase municipal revenues, easing the budget constraint the mayor faces. Reminder letters are themselves a quite inexpensive tool, with a relatively low opportunity cost in terms of municipal resources. There are also good reasons to think that mayors would care about the effectiveness of ECD programs. In addition to any prosocial motivations, there is evidence from Brazil that voters reward or punish mayors based on their performance. For example, voters are less likely to re-elect mayors who failed to improve test scores in municipal schools (Firpo *et al.*, 2017), or those who were exposed as being corrupt (Ferraz & Finan, 2008). Mayors also appear to engage in competition with their neighboring municipalities on school performance (Terra & Mattos, 2017). Given that mayors have a limited budget, it seems reasonable that information on effectiveness (and therefore cost-effectiveness) could be valuable to mayors.

3 Beliefs Experiment

In this section, we describe an experiment to measure (a) whether Brazilian policy-makers demand 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 substantially in response.

3.1 Experimental setting and sample

We implemented the beliefs experiment with 764 officials from 579 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 privately self-administered by participants using tablets. The experiment was not announced in advance to participants. Instead, research assistants recruited conference participants during breaks in between sessions, as described in the next section. One of the researchers and one research assistant were present throughout to monitor and answer questions.

Almost 50 percent of participants in the experiment were mayors; 26 percent were local legislators; 18 percent were municipal secretaries; and 6 percent were vice-mayors. The geographical distribution of the municipalities represented is shown in Figure 1, 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.

We recruited 39 percent of attending mayors, 49 percent of vice-mayors, 35 percent of municipal secretaries, and 41 percent of local legislators. Participation was limited by the number of tablets available and the limited breaks in the conference schedule, but participants may also have selected into the study based on their interest in the participation incentive (lottery tickets), or their interest in education policy. The latter would potentially bias our estimates of demand upwards. Appendix Table A.1 shows that participating mayors were 8 percentage points more likely to be from leftist parties than non-participants, but were otherwise similar on a range of other characteristics.

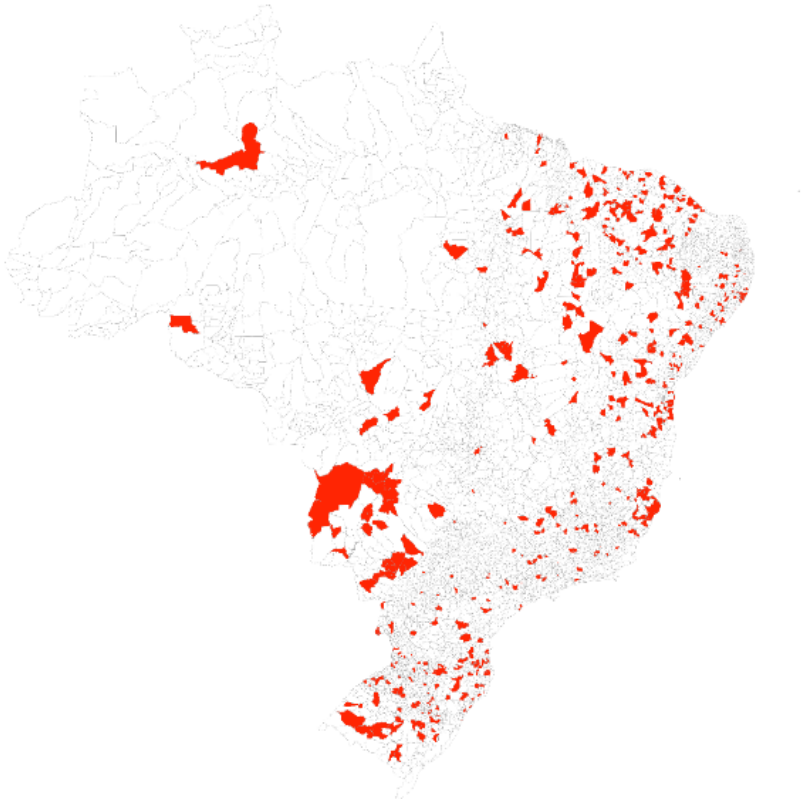
3.2 Experimental design

The structure of the experiment, depicted graphically in Figure 2, 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 B 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 pro-

[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. In addition, another group of 134 municipal officials from 117 municipalities also completed a survey on the advantages and disadvantages of the different studies used in this experiment.

FIGURE 1: BELIEFS EXPERIMENT: SAMPLE MUNICIPALITIES

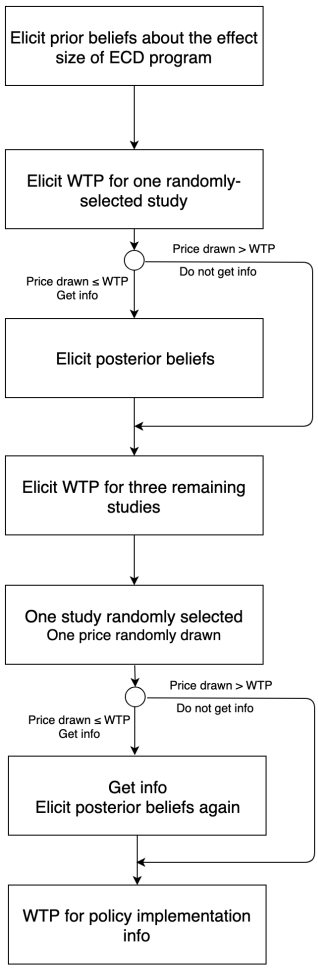


grams are evaluated (test scores, cognitive skills) and how those outcomes are reported (standardized effect sizes). To ease understanding of the policy and its objectives, we provided illustrative examples of current similar programs in Brazil and presented participants with a few benchmarks for effect 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.[14] 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 states in the U.S. (n=4667) (Puma *et al.*, 2010). When the relevant studies were presented to the

[14]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. This is an important limitation of the study, which we return to later.

FIGURE 2: BELIEFS EXPERIMENT: STRUCTURE



participant, we highlighted both the study location and sample size.[15]

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

[15] We did not use the labels "Developing Country" or "Rich Country", nor "Small Sample" or "Large Sample". We simply presented the location and the sample size.

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 deal with the issue of selection into seeing a study result based on one's WTP, 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*. Whenever presenting results on belief updating, we also present results for this sub-sample, which receives the information without selection. This approach also has the advantage that we get to observe belief-updating for most participants.

For those who received the information, we subsequently elicited posterior beliefs about the expected impact of the policy in their own municipality, and in a study location that was *not* offered for purchase in this round. We do not ask for an updated posterior from participants who do not receive a study's results. As is standard in lab experiments on belief updating, we assume that beliefs do not change over the matter of minutes in the absence of new information (e.g. see the papers reviewed in Benjamin (2019) or Vivalt & Coville (2020); Mobius *et al.* (2011); Eil & Rao (2011)).[16]

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. Having this second round allows us to observe a second instance of belief-updating per participant, increasing statistical power. It also allows us to learn how the weight placed on research information diminishes from the first to the second study on the topic.

[16]This assumes away the possibility that simply being asked a second time would cause a systematic shift in beliefs, for instance due to thinking harder. Under our assumption, the change in belief from prior to posterior is the treatment effect of learning the information.

3.3 Results

We interpret the results through the lens of a simplified 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. While we cannot test the assumptions of this model—particularly the normally distributed probabilistic beliefs—since we only measure point beliefs, this framework provides a useful benchmark for the belief-updating we study.

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 ($\Sigma_{i,large}^I < \Sigma_{i,small}^I$), 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, if a bit optimistic. Appendix Table A.2 shows that the average policy-maker believes that ECD policies are more effective in rich countries (effect size of 0.45-0.50 sd) than in developing countries (effect size of 0.37-0.42 sd). On average, municipal officials believe the effect size in their own municipality (0.42 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.[17] Since we only elicit point beliefs rather than probabilistic beliefs, we do not have a measure of the uncertainty in each policy-maker's beliefs.

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. If policy-makers value accurate beliefs, 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

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

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, between 0.4 percent and 0.9 percent of a mayor's monthly wage. There is substantial heterogeneity in demand: the standard deviation of WTP is 32 lottery tickets. Yet, 98 percent of participants have strictly positive WTP.[18] WTP declines from round 1 to round 2: the second study a policy-maker is offered is valued 11 percent less than the first.

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 9 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 0.484). In contrast, and contrary to our priors, we do *not* find significant differences between the WTP for research findings from Colombia and Jamaica versus Michigan or across the US. This suggests that, on average, 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.3. Only three characteristics out of twenty are significantly associated with WTP in this exploratory analysis: 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 inevitable: policy-makers with more past experience with a policy might have a more precise prior, and therefore not value additional information. Instead, we find that it is precisely the policy-

[18] 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.

makers who implement and spend municipal resources on ECD programs who have the highest WTP for related research information. Presumably, this is because having accurate beliefs about such programs is more valuable to them. Term-limited mayors and those with a higher margin of electoral victory (who presumably face less electoral competition), in contrast, do not have higher WTP for 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:

$$Posterior_{ijs} = \beta_1 Prior_{ij} + \beta_2 Signal_{ijs} + \varepsilon_{ij} \quad (2)$$

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 framework on average, $\hat{\beta}_1$ and $\hat{\beta}_2$ are both positive and statistically significant, and sum up to approximately 1. Participants place about two-thirds weight on their prior and one-third on the study finding on average, and do not simply accept or repeat back the research finding. This finding perhaps reduces concerns about experimenter demand effects. They place similar weight on the study finding when forming beliefs about their own municipality, compared to beliefs about an alternative location (Column 4 vs. Column 2). They place more weight on their prior in the second round, when it already incorporates the finding of the first study they received. Put differently, the weight placed on a study's findings falls by 33 percent from the first to the second study a policy-maker learns about. 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.[19]

Under the assumption that beliefs do not change absent any new information, we can also report the effect of receiving each study. In round 1, the average posteriors after receiving each study are 0.49 (Michigan), 0.51 (Jamaica), 0.37 (Colombia) and 0.35 (US). Compared to the average prior in each case, this implies treatment effects of +0.072 for Michigan, +0.098 for Jamaica, -0.034 for Colombia, and -0.073 for the large-sample US study respectively.

Appendix Tables A.5 and A.6 report an exploratory analysis of heterogeneity in belief updating by mayors' and municipalities' characteristics. College-educated and leftist mayors place less

[19]One concern is that the prior may be measured with noise, and that such measurement error will attenuate the coefficient on Prior. We can address this issue by instrumenting for the prior in Round 2 using the revealed study in Round 1. Appendix Table A.4 contrasts the weights on the priors in updating using the OLS specification (Col 4) and a 2SLS specification where the prior is instrumented (Col 5). The coefficients on Prior are very similar, suggesting that the attenuation bias problem is not severe in practice.

weight on their priors and more weight on the study finding. Older mayors do the reverse: they update their beliefs less when faced with research information. While mayors who have implemented ECD programs had higher WTP for studies, as described above, they do not update more based on them. Finally, re-election incentives and political competition do not have a systematic relationship with updating. Just as term-limited mayors and those with larger electoral margins of victory did not have lower WTP, they also do not place lower weight on the research findings. Of course, these findings cannot be interpreted causally, and they should be treated as suggestive at best.

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

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

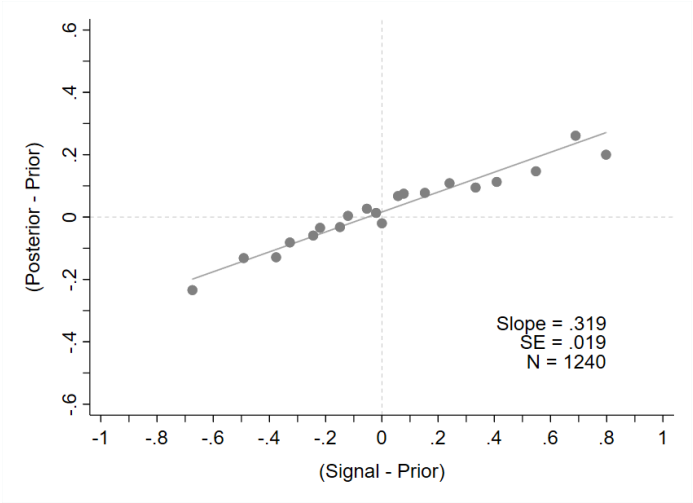
where $Large_{ijs}$ and $Developing_{ijs}$ are defined as in equation (1). Under the framework, if an individual perceives a study to be more informative, they will place more weight on the signal from that study and correspondingly less weight on their prior. Therefore, to test whether participants 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. 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.

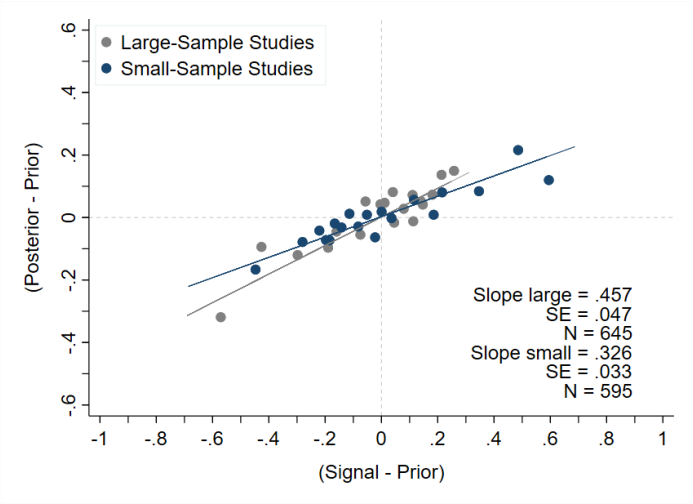
Figure 3 depicts the observed belief updating using binned scatter plots.[20] The y-axis plots the size and direction of updating $(Posterior - Prior)$ for a given news shock due to the signal $(Signal - Prior)$ on the x-axis. Panel (a) includes all instances of updating, pooling across studies and rounds, and adds a linear OLS fit. A few points are noteworthy. First, the relationship does appear to be linear, in line with the Bayesian model and our empirical specification in (2). Second, there is no evidence of asymmetric (optimistic) updating, which would show up as a kink at the origin with a steeper slope to the right of zero. The other panels in turn depict updating separately for large and small-sample studies (Panel (b)) and for rich and developing-country studies (Panel (c)). The stronger updating response to large-sample studies is evident, as is the similar average response to rich and developing-country studies.

[20] Appendix Figure A.1 presents the corresponding figures separately by study.

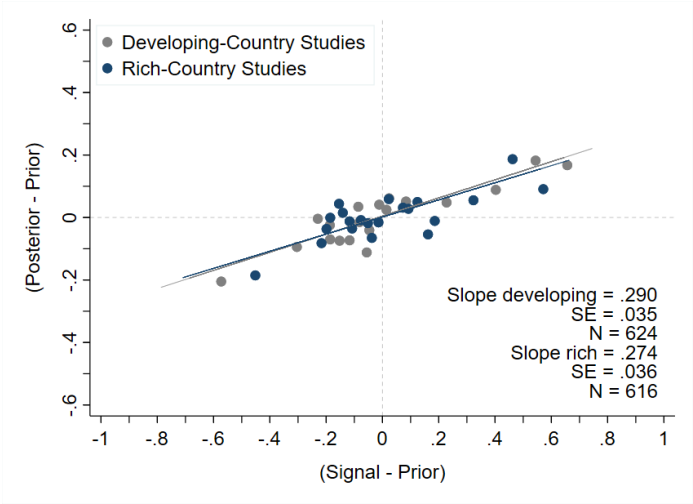
FIGURE 3: BELIEFS EXPERIMENT: BELIEF UPDATING



(a) Full sample



(b) Large vs small-sample studies



(c) Developing vs rich-country studies

Notes: Comparison between the difference in respondent's perceptions after buying a study (i.e. posterior beliefs minus prior beliefs) and the difference in respondent's perceptions before buying a study (i.e. signal minus prior beliefs), averaged over 20 bins of rounds 1 and 2. Prior is the belief of the respondent about the effect, right before buying a 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. Panel (a) shows statistics for full sample. Panels (b) and (c) compare statistics between large- versus small-sample studies (controlling for the location attribute of the study) and between developing- versus rich-country studies (controlling for the sample size attribute of the study), respectively. Large- (small-) sample studies include Colombia and US (Jamaica and Michigan); while developing- (rich-) country studies include Colombia and Jamaica (Michigan and US). The slope and robust standard errors clustered at the individual level are based on a linear regression with a constant term.

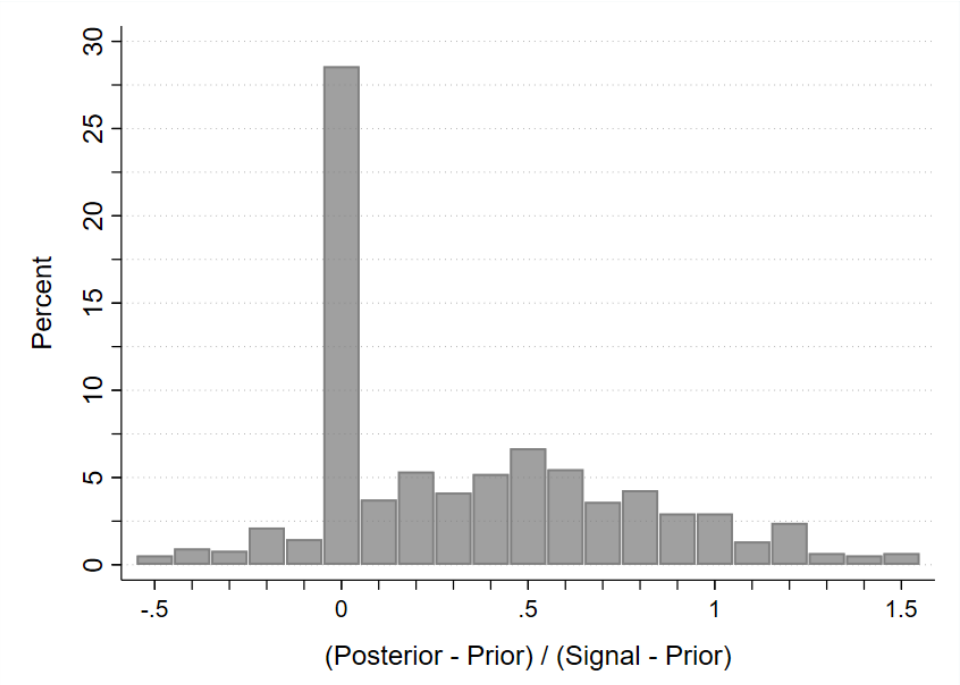
Figure 4 plots the histogram of the belief-updating responses. Specifically, for each instance of updating, we calculate $\pi = (Posterior - Prior)/(Signal - Prior)$ and then average these responses within each individual. The figure reveals substantial heterogeneity in the weight placed on the research findings. 28 percent of policy-makers appear to ignore the study result and do not update their beliefs at all ($\pi = 0$). 43 percent of policy-makers have updating weights strictly between 0 and 1. 15 percent update in the wrong direction ($\pi < 0$) while 13 percent overreact ($\pi > 1$). This distribution appears quite similar to that found in Vivalt & Coville (2020), who present participants at a World Bank impact evaluation workshop with a hypothetical study in a belief-updating exercise. They also find a substantial share of participants who do not update ($\pi = 0$), and about 55 percent of participants displaying ($0 \leq \pi \leq 1$). The average updating weight in our sample, about 0.37 in the first round, is also comparable to the median weight of 0.5 found by Vivalt & Coville (2020).

What explains the approximately one quarter of participants who do not respond to the information? One possibility is simply inattention or effort-minimization by participants. However, attention checks ensured that participants at least briefly registered the study findings, and participants were required to actively report a posterior during each belief elicitation. The interpretation through the lens of the model would instead be that these policy-makers have very confident priors, and therefore think that research is uninformative. This is possible, but is at least somewhat inconsistent with 98 percent of participants having a positive WTP for study results. Another possible factor is rounding issues in belief measurement in our experiment. Beliefs could only be reported at intervals of 0.1 sd. Thus, underlying belief updates from, for example, 0.46 to 0.54 will appear to involve no update at all, if both are rounded to 0.5. These rounding issues can also inflate the share who appear to overreact, since updating from 0.44 to 0.46 may be measured as an update of 0.1. This is a major caveat to the interpretation of the individual-level updating distribution.[21] This concern is likely to matter less when measuring average responses across many participants, which most of our analysis focuses on. Nonetheless, Appendix Table A.4 provides the belief-updating regressions while consecutively dropping participants who never update ($\pi = 0$), or excluding those with $\pi \leq 0$ or $\pi \geq 1$.

One natural question is whether the patterns of belief updating we observe are quantitatively sensible and in line with rational Bayesian updating on average. Since we do not measure the precision of beliefs or provide participants with the precision of the signal, we cannot calculate how much a Bayesian *should* update. Therefore, we cannot say with confidence whether the extent of updating is about right, or instead too much or too little on average. Nor do we judge whether it is appropriate for policy-makers to place equal weight on results from rich and developing countries. Our results along these lines are purely descriptions of how policy-makers *do* update.

[21] There is at least one other factor that may explain updating in the wrong direction or overreaction. Participants may consider different studies to not just be more or less noisy signals, but also to have some known bias, e.g. thinking that programs in the US are better implemented, such that one should subtract some fixed number from the effect size, or conversely thinking that effects in developing countries will be larger since they are further away from the efficient frontier. Then, seeing a US result slightly greater than one's prior regarding one's own municipality could actually cause one to update in the opposite direction.

FIGURE 4: BELIEFS EXPERIMENT: BELIEF UPDATING DISTRIBUTION



Notes: Distribution of the share of the difference in respondent's perceptions after buying a study (i.e. posterior beliefs minus prior beliefs) and the difference in respondent's perceptions before buying a study (i.e. signal minus prior beliefs), averaged within respondents' rounds 1 and 2.

However, with additional assumptions, it is possible to shed some light on a related question: is the response to sample size in line with a Bayesian model? Let us first consider the updating weights placed on the different kinds of studies. Suppose that priors, signals and posteriors are all normal, and that the precision of signals depends only on sample size. Then one can show that, for a Bayesian, the ratio of the updating weights placed on two studies j and k should be closely related to their sample sizes: $\Pi_j/\Pi_k = n_j/n_k$, where $\Pi_i \equiv \frac{\pi_i}{1-\pi_i}$ is the ratio of the optimal weights placed on the signal and the prior and n_i denotes the sample size of study i.[22] To compare this to the actual empirical weights, we can calculate the average weight placed on the signal for each study by estimating (2) separately by study (in Round 1, and normalizing the weights on the prior and signal to sum to one). Comparing the two developing-country studies, we find that $\Pi_{\text{Colombia}}/\Pi_{\text{Jamaica}} = 1.8 < 10.9 = n_{\text{Colombia}}/n_{\text{Jamaica}}$. This implies that the weight placed on the larger-sample Colombia study relative to the smaller-sample Jamaica study is less than

[22] The steps in the argument are as follows. The ratio of the variance of the signals coming from two studies j and k is $\Sigma_j^I/\Sigma_k^I = n_k/n_j$, where n denotes sample size. Recall that the optimal weight placed by a Bayesian on a signal from study j is $\pi_j = \frac{\Sigma^{pr}}{\Sigma^{pr}+\Sigma_j^I}$. Rearranging, we get that $\Sigma^{pr} = \Pi_j \cdot \Sigma_j^I$, where we have defined $\Pi_i \equiv \pi_i/(1-\pi_i)$. Since the priors are the same across studies due to randomization, it must be that $\Pi_j/\Pi_k = \Sigma_k^I/\Sigma_j^I$. Since $\Sigma_k^I/\Sigma_j^I = n_j/n_k$, it follows that $\Pi_j/\Pi_k = n_j/n_k$. The empirical analogues for this expression are restricted to updating in Round 1, when randomization ensures that priors are the same on average.

what Bayesian learning would justify. We find a similar qualitative pattern in the case of the two rich-country studies: $\Pi_{\text{US}}/\Pi_{\text{Michigan}} = 5.4 < 37.9 = n_{\text{US}}/n_{\text{Michigan}}$. In both cases, policy-makers under-react to variation in sample sizes relative to a Bayesian. It is important to emphasize that we cannot say whether this is because too little weight is placed on the large-sample study, or too much on the small-sample study, or both.[23] Our finding is consistent with Vivalt & Coville (2020), who show that policy-makers under-react to the size of confidence intervals.

Next, consider the difference in WTP between the large and small-sample studies. Is this quantitatively justified by the subsequent differences in updating weights? Suppose WTP is proportional to the expected reduction in the policy-maker's mean-squared prediction error. Then, one can show that $WTP_s \propto \pi_s \Sigma^{pr}$. For a Bayesian, we should therefore have $WTP_i/WTP_j = \pi_i/\pi_j$. Empirically, comparing the large-sample and small-sample studies (again normalizing the weights on prior and signal), we have $WTP_{\text{large}}/WTP_{\text{small}} = 1.09 < 1.56 = \pi_{\text{large}}/\pi_{\text{small}}$. Thus, while policy-makers' WTP does respond to sample size, the sensitivity of WTP to sample size is lower than that justified by the sensitivity of belief updating (which itself may be too low, as described above). Again, we cannot say whether this is because the WTP for the large-sample studies is too small, or instead because the WTP for small-sample studies is too high. For instance, it could be that the baseline WTP is inflated because it captures not just concern for the informativeness of the study, but also some experimenter demand effects or just confusion.

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=294). 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.

One glaring weakness is that we only consider studies from three countries. What we interpret as a "rich-country effect" could instead be a "USA effect": Brazilian policy-makers might not value research from other rich countries. Similarly, it could be that they would place much greater weight on evidence from Brazil, and consider it much more relevant than findings from Colombia or Jamaica. We were limited in our ability to explore these questions due to a lack of comparable

[23] There is another way in which the observed updating clearly departs from the Bayesian learning framework: large-sample studies lead to an increase in weight placed on the signal but the reduction in weight placed on the prior is not equal and opposite but is instead smaller, as shown in Table 4.

studies from more countries, including Brazil.

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). What if participants simply update more (in proportional terms) in response to small effect sizes, say due to concerns about greater publication bias in small studies, or because large effect sizes seem implausible? 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 alone, we reported the estimated effect sizes at a much longer time horizon (without flagging this discrepancy), which resulted in a smaller effect size. Appendix Table A.7 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.

WTP for implementation information. But does access to research lead to more effective policies being adopted? At the very end of the 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 beliefs. Appendix Table A.8 shows the results. We find that more positive beliefs about ECD programs—shaped through learning about research findings—causally increase WTP for implementation information. This provides clean, experimental evidence on the effect of research information on demand for policy implementation via changed beliefs.

3.4 Tests of motivated reasoning

In this section, we test for specific forms of motivated reasoning in belief updating: asymmetric updating and confirmation bias.[24]

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

[24]It is worth noting that we will test for evidence *consistent* with such motivated belief-updating. However, similar empirical patterns could also be generated by Bayesian learners with non-Gaussian priors. Since we largely find null effects, we conclude that we do not observe evidence for motivated reasoning.

policy relative to positive (confirming) information? And do policy-makers with negative priors do the reverse? Alternatively, do policy-makers systematically respond more to positive information relative to negative information, thereby ending up over-optimistic about policies, as argued by Vivalt & Coville (2020)? We test these hypotheses 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.[25]

Table 5 reports the results. Column 1 shows that, on average, policy-makers do *not* react asymmetrically to positive news relative to negative news, as evidenced also by Figure 2(a). But this comparison may be confounded: the two large-sample studies find smaller effects, and are therefore more likely to lead to negative news. If large-sample studies are given greater weight in updating (as we showed previously), this would tend to counteract any tendency to under-weight bad news. Columns 2 and 3 therefore test for asymmetric updating separately for the large-sample and small-sample studies. The point estimates again do not indicate substantial asymmetric updating, unlike in Vivalt & Coville (2020), although the estimate for large-sample studies in particular is quite imprecise. Column 4 tests for confirmation bias. To do so, 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 coefficient on the $ConfirmingNews$ variable is negative, implying the opposite of confirmation bias. Altogether, we find no evidence for confirmation bias or asymmetric updating on average when policy-makers are presented with research evidence on policy effectiveness.

3.5 Beliefs experiment: discussion

We have two main findings from the beliefs experiment. First, political leaders in Brazil value learning about research on policy effectiveness. 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 not on developing-country studies. While attending to sample size indicates a degree of sophistication, we provide suggestive calculations that the sensitivity to sample size is lower than expected from a Bayesian learner.

The experiment has some clear weaknesses. We contrast the effects of a limited number of studies from only three countries. The WTP measure is rather artificial, and comes out of the

[25] See also Vivalt & Coville (2020). 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. This is particularly valuable when we examine politically-motivated information processing in Appendix Table E. 1.

policy-maker's private budget, rather than the likely more-relevant municipal budget, which may have other higher-value uses. We establish effects on beliefs only over a very short period of time and cannot speak to whether the effects persist. The experiment may also generate demand effects, with participants feeling some social pressure to place weight on the study results (although they completed the experiment privately on a tablet, rather than face-to-face with an experimenter). The policy-adoption experiment described in the next section, in contrast, provides evidence of longer-lasting changes in beliefs and measures effects on actual municipal policy.

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 affects not just beliefs, but also adoption of the policy 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. Mayors can attend multiple training sessions led by CNM expert staff on topics ranging from municipal financial planning and budgets to public policy areas, such as urban development, education, health and tourism. Multiple sessions run in parallel throughout the conference, except for a limited number of plenary sessions. The conference itself ran in stages, with mayors from different regions attending on different days due to capacity constraints. Each mayor attended for two days.

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 municipalities in that population range. Figure 5 shows the spatial distribution of the sample municipalities.

Table 6 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.[26] The average municipality in the sample has a population of about 21,000 residents.

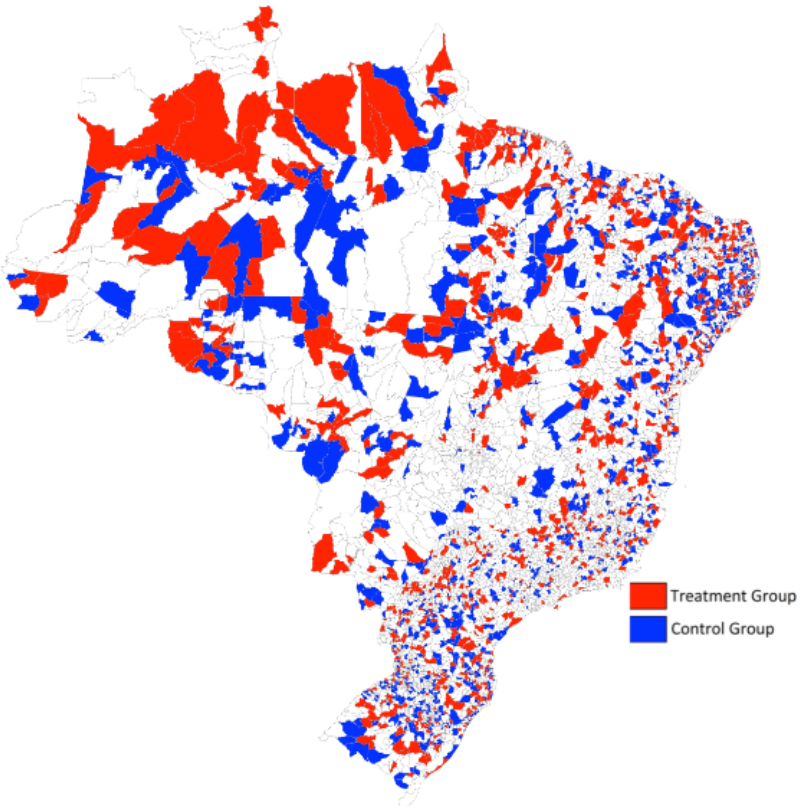
4.3 Experimental design

Mayors attending the conference were randomized into treatment (n=881) and control (n=937) groups.[27] 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 sessions. 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 6 shows that the treatment and control groups are largely balanced on mayor's characteristics as well as municipal characteristics.

[26] 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.

[27] 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.

FIGURE 5: POLICY-ADOPTION EXPERIMENT: SAMPLE MUNICIPALITIES



The information sessions lasted 45 minutes and were led by an experienced local instructor, without foreign researchers present. 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.[28] At the end of the session, mayors received a professionally-produced policy brief

[28]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.

with the same information content as the presentation, including references to the cited papers.[29]

The session was offered 3-4 times during each stage of the conference. Treated mayors could therefore choose to attend when their opportunity cost of time was lowest. Judging from our (unsystematic) field observations, it appeared that the most common counterfactual to attending our information session was networking with other mayors. For other mayors, the opportunity cost was instead attending one of the other simultaneous sessions at the conference. Half of our sessions clashed with a plenary session which taught mayors about municipal finances and budgets and emphasized proper financial planning and fiscal responsibility. The other half clashed with slots during which mayors could have drop-in office hours with the partner organization, or could instead attend a variety of parallel sessions, each of which were themselves offered twice during each stage. We did not clash with other plenary sessions designed around public policy (social policies, urban development policies, and economic policies). No other session at the conference emphasized research information or impact evaluation.

To summarize, attending our information sessions came at the expense of some combination of professional networking, training on municipal budgeting and finances, or sessions on a variety of topics not including impact evaluation, research evidence or economic, social or urban development policies. While our treatment induces greater policy adoption of tax reminder letters, as we will show, it may come at a cost in terms of reduced professional networking or worse knowledge or performance on a diffuse range of outcomes we do not observe. This does not change our conclusion, however, that providing research information did lead to a change in policy.

4.4 Data

To measure how the research information provision affected political leaders' beliefs and ultimate policy adoption, 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 themselves.[30]

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. 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.[31] There was no differential attrition between treatment and control groups,

[29] Appendix C presents the policy brief.

[30] 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.

[31] On average, many hours of work were needed before we could talk to the chief tax bureaucrats and mayors over

and observable characteristics of the successfully contacted municipalities are similar across both groups, as reported in Table 6.

The survey lasted approximately 15 minutes. The key outcomes asked about 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: the due date, the possibility of fines or audits, and language regarding the social norm of paying taxes on time. An important secondary outcome measured in the survey was beliefs about policy effectiveness. Specifically, we elicited quantitative beliefs about the likely impact of such a policy, in terms of percent changes in tax compliance, even if the municipality reported not using such reminders. In addition, we asked questions that served as attention and comprehension checks as well as questions about one potential policy substitute to reminder letters (namely financial incentives for tax payers) and placebo questions on which we would expect null effects of the treatment (the use of e-procurement platforms).

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.[32] 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 meaningful, although difficult to quantify. 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.9 reports predictors of participation in the research-information session. Younger and college-educated mayors are 7 and 15 percentage points more likely to participate 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

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.

[32] 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.

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 7 presents Treatment-on-Treated (ToT) estimates, using randomized treatment status as an instrument for participation in the information session.[33] 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.[34] 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.11 and A.12 report an exploratory analysis of heterogeneity in treatment effects on policy adoption by mayoral and municipal characteristics. No clear evidence of heterogeneity emerges, partly due to limited statistical power. Term-limited mayors are not substantially less likely to adopt reminder letters. The coefficients on interactions of treatment with age and margins of electoral victory suggest that mayors of above-median age and victory margins are less likely to adopt reminder letters, while leftist mayors are more likely to adopt, but none of these estimates are statistically significant even without correcting for multiple hypotheses testing. Appendix Table A.13 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.14 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—is 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 main estimated effect size of 12 percent shared with participating mayors in the research-information session and policy briefs provided. Unlike in our beliefs experiment, it was not possible to provide benchmarks and comprehension checks during the short phone survey, so

[33] Appendix Table A.10 presents the Intent-to-Treat estimates.

[34] 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 measures must necessarily be treated with some caution. Panel A of Table 8 shows that attending the information session—instrumented using treatment assignment—increased the 'accuracy' of beliefs even 15-24 months after treatment. Specifically, the absolute deviation of beliefs from the effect size mentioned in the research-information session is 20 percent lower than in the control group. Comparing Columns 4 and 5 reveals that beliefs became more accurate not just among mayors, but also among tax-department bureaucrats, implying information-flow within the municipal government. This was perhaps made easier by providing the participating mayors with shareable policy briefs.

Panel B of Table 8 instead estimates the effect of belief accuracy on policy adoption, now instrumenting for belief accuracy using treatment assignment. Of course, this requires making the debatable assumption that the treatment only affects adoption through beliefs. The estimates imply that increasing belief accuracy by 1 percentage point (i.e. reducing the absolute deviation by 1 pp on a base of about 7 pp) increases adoption by 8 percentage points (se 5 pp). Of course, the effects may operate also through other channels such confidence, salience etc., as discussed below. These magnitudes must therefore be treated as descriptive. It is also worth noting that the relevant beliefs were presumably those at the time of the policy-adoption decision, which we do not observe.

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 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.

Some 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. We also found evidence of more accurate beliefs in the treatment group, although we cannot rule out that effects would have been similar had we simply provided a policy recommendation stripped of any evidence. 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. Third, we estimate the effect of providing research information in a particular context: an information session designed by researchers at reputed foreign universities, at a conference organized by a trusted local organization. Research findings received from other sources, such as local think tanks, academics or media sources, may be differently received. Similarly, policy-makers seeking to unearth relevant research information themselves may have difficulty finding and interpret-

ing relevant and high-quality information. 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.

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 policies which have been proven to be effective.

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. Moreover, policy-makers might face the problem of information overload, with numerous motivated actors attempting to persuade them by providing them with selective pieces of evidence and information. 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: BELIEFS EXPERIMENT: SUMMARY STATS AND BALANCE

Variables	Mean Control	Δ Developing	P-Value	Δ Large	P-Value
Mayors' Characteristics					
Male	87.83	-1.32	0.38	-4.04	0.00
Age	48.46	-0.11	0.82	-0.49	0.28
College	58.31	2.80	0.20	0.13	0.95
2nd Term	19.88	0.44	0.82	-0.07	0.97
Electoral Margin Victory	14.65	0.31	0.68	0.36	0.55
Leftist Political Party	37.39	-0.99	0.64	1.80	0.42
Municipalities' Characteristics					
Population	24.41	1.45	0.48	1.24	0.40
College Population	4.849	-0.07	0.52	0.02	0.87
Public Adm College	33.14	-0.97	0.09	-0.85	0.17
Poverty	26.62	0.48	0.55	0.41	0.61
Gini	49.91	0.48	0.09	0.44	0.13
Big South	49.26	-0.75	0.74	-1.07	0.63
Per Capita Income	452.4	-8.79	0.40	1.02	0.93
Kids in School (0-3)	19.22	-1.04	0.03	0.08	0.87
Kids in School (4-5)	78.65	-0.41	0.54	0.16	0.83
ECD Policy Survey Characteristics					
Mayor	50.45	-0.16	0.94	-1.33	0.55
Prof Politician	29.97	0.74	0.72	-0.47	0.81
Leftist Scale	21.66	-2.37	0.19	-1.79	0.35
Implemented ECD	41.69	0.40	0.85	-3.02	0.17
Heard ECD	25.82	-0.81	0.68	-0.11	0.95
Observations	674	1,368		1,368	
Round	1 and 2	1 and 2		1 and 2	
Clusters (Individuals)	674	764		764	

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

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

LHS Variable	(1) WTP	(2) WTP	(3) WTP
Large	3.8221	2.3554	4.4182
	(0.7912)	(2.3944)	(1.0152)
Developing	0.3783	1.5948	-0.2735
	(0.7907)	(2.3951)	(1.0039)
Observations	2,573	764	1,809
Round	1 and 2	1	2
Clusters (Individuals)	764	764	604
Mean LHS	44.62	48.39	43.03
SD LHS	31.77	33.06	31.09

Notes: OLS results. The dependent variable is willingness to pay, which is elicited in two different rounds. Developing is a dummy which is = 1 for Jamaica and Colombia, 0 otherwise. Large is a dummy which is = 1 for Colombia and US, 0 otherwise. Difference in number of 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. 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-value of Large (column 2) = Large (column 3) test is .484. P-value of Developing (column 2) = Developing (column 3) test is .524.

TABLE 3: BELIEFS EXPERIMENT: BELIEF UPDATING

LHS Variable	(1) Posterior	(2) Posterior	(3) Posterior	(4) Posterior	(5) Posterior
Prior	0.6824	0.5902	0.7902	0.6528	0.6813
	(0.0214)	(0.0295)	(0.0237)	(0.0280)	(0.0224)
Signal	0.3230	0.3749	0.2607	0.3622	0.3209
	(0.0194)	(0.0261)	(0.0234)	(0.0293)	(0.0203)
Observations	1,240	700	540	543	1,131
Round	1 and 2	1	2	1	1 and 2
Beliefs About	Municipality	Municipality	Municipality	Random Study	Municipality
Received Study for Free	No	No	No	No	Yes
Clusters (Individuals)	755	700	540	543	731

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 a 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. In the rows below the coefficients, 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). Received Study for Free indicates whether participant received the information regardless of their WTP. Difference in clusters between columns 2, 3 and 4 is due in part to a different experimental design of last CNM conference, in which only one study was offered for purchase. Robust standard errors clustered at the individual level are in parentheses.

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

LHS Variable	(1) Posterior	(2) Posterior	(3) Posterior	(4) Posterior	(5) Posterior
Prior	0.6388	0.5600	0.7509	0.6685	0.6420
	(0.0368)	(0.0531)	(0.0471)	(0.0543)	(0.0384)
Signal	0.3306	0.3780	0.2653	0.3351	0.3280
	(0.0284)	(0.0397)	(0.0384)	(0.0429)	(0.0299)
Prior * Developing	-0.0093	-0.0247	-0.0106	-0.0920	-0.0083
	(0.0389)	(0.0599)	(0.0477)	(0.0574)	(0.0414)
Signal * Developing	0.0091	0.0082	0.0189	0.0682	0.0039
	(0.0349)	(0.0515)	(0.0472)	(0.0578)	(0.0367)
Prior * Large	-0.0535	-0.0904	-0.0307	-0.0563	-0.0663
	(0.0480)	(0.0690)	(0.0600)	(0.0714)	(0.0501)
Signal * Large	0.3233	0.4068	0.2413	0.2744	0.3510
	(0.0712)	(0.0963)	(0.0942)	(0.1176)	(0.0745)
Observations	1,240	700	540	543	1,131
Round	1 and 2	1	2	1	1 and 2
Belief About	Municipality	Municipality	Municipality	Random Study	Municipality
Received Study for Free	No	No	No	No	Yes
Clusters (Individuals)	755	700	540	543	731
P-value Prior*Dev.=Signal*Dev.	0.791	0.755	0.742	0.142	0.869
P-value Prior*Large=Signal*Large	0.001	0.002	0.064	0.069	0.001

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 a 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 = 1 for Jamaica and Colombia, 0 otherwise. Large is a dummy which is = 1 for Colombia and US, 0 otherwise. In the rows below the coefficients, 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). Received Study for Free indicates whether participant received the information regardless of their WTP. Difference in clusters between columns 2, 3 and 4 is due in part to a different experimental design of last CNM conference, in which only one study was offered for purchase. Robust standard errors clustered at the individual level are in parentheses.

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

LHS Variable	(1) Posterior	(2) Posterior	(3) Posterior	(4) Posterior
Study Characteristic	All	Large	Small	All
Prior	0.9957	1.2310	0.9508	1.0165
	(0.0217)	(0.0572)	(0.0236)	(0.0150)
Signal-Prior	0.3075	0.6476	0.2429	0.3406
	(0.0499)	(0.1003)	(0.0937)	(0.0235)
Signal-Prior * Positive Surprise	0.0193	0.1166	0.0999	
	(0.0659)	(0.2089)	(0.1077)	
Signal-Prior * Confirming News				-0.1179
				(0.0573)
Observations	1,131	582	549	1,131
Round	1 and 2	1 and 2	1 and 2	1 and 2
Beliefs About	Municipality	Municipality	Municipality	Municipality
Received Study for Free	Yes	Yes	Yes	Yes
Clusters (Individuals)	731	513	484	731

Notes: OLS results. The dependent variables are posterior beliefs, which are declared after successfully buying the results from a study in each round. Study Characteristic indicates the sample of studies used in the model (large sample studies—Colombia and US, small sample studies—Michigan and Jamaica, or all studies—Colombia, Jamaica, Michigan and US). Prior is the belief of the respondent about the effect, right before buying a 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 = 1 if the bought study's effect is greater than the respondent's prior about the effect. Confirming News is a dummy which is = 1 if the respondent's prior about the effect was above the median (or below the median) and the bought study's effect is greater (smaller) than the respondent's prior, 0 otherwise. In the rows below the coefficients, Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality or one of the four possible study locations. Received Study for Free indicates whether participant received the information regardless of their WTP. Robust standard errors clustered at the individual level are in parentheses.

TABLE 6: POLICY-ADOPTION EXPERIMENT: SUMMARY STATS AND BALANCE

	at Baseline			at Endline		
Variables	Mean Control	Δ Treatment	P-Value	Mean Control	Δ Treatment	P-Value
Mayors' Characteristics						
Male	88.26	1.41	0.34	90.01	-0.14	0.93
Age	46.76	1.32	0.01	47.08	1.61	0.00
College or more	57.74	-0.76	0.74	57.66	0.73	0.78
2nd Term	15.69	1.56	0.37	15.18	0.91	0.63
Electoral Margin Victory	16.73	0.36	0.68	16.61	0.46	0.63
Leftist Political Party	32.98	2.10	0.35	32.76	1.36	0.58
Municipalities' Characteristics						
Population	20.86	-0.06	0.94	20.23	0.06	0.95
College Population	5.17	-0.15	0.25	5.47	-0.14	0.31
Public Adm College	32.50	0.89	0.21	33.32	0.25	0.74
Poverty	26.41	-0.27	0.76	23.05	0.11	0.91
Gini	50.33	-0.19	0.54	49.37	0.17	0.61
Big South	51.01	-0.62	0.79	59.92	-2.36	0.36
Per Capita Income	457.64	3.42	0.75	489.23	2.78	0.81
Local Taxes Revenues (2010-15)	6.06	0.09	0.68	6.40	0.08	0.75
Joint F-test			0.18			0.20
Attrition						
Municipality				19.85	-1.69	0.36
Mayor				48.35	2.28	0.33
Finance Staff				24.97	-0.80	0.69

Notes: Sample means of control group and differences in means with respect to treatment group at baseline and endline. There were 937 (751) municipalities in the control group and 881 (721) in the treatment group at baseline (endline). The first block of variables reports characteristics of the mayor that runs the municipality. Leftist Political Party (= 1 for mayors belonging to a center-leftist party according to historical political platforms, 0 otherwise). The second block of variables reports characteristics of the municipality. Population is the municipality's number of inhabitants (in thousands). College Population is the municipality's share of adults with college degrees. Public Administration College is the share of municipal public employees with college degrees. Poverty is the municipality's poverty rate. Gini is the municipality's Gini coefficient. Big South is = 1 for municipalities in the south, southeast and mid-west regions, 0 otherwise. Per Capita Income is the municipality's monthly income per capita. Local Tax Revenues (2010-2015) indicates the average share of municipal tax revenues in total municipal revenues from 2010 to 2015. Joint significance F-test, and follow-up survey attrition rate—municipality, mayor and finance staff—at endline.

TABLE 7: POLICY-ADOPTION EXPERIMENT: POLICY ADOPTION: TAX REMINDERS

LHS Variable	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted
Information Session	0.1031	0.1065	0.1024	0.1177	0.1094
	(0.0531)	(0.0526)	(0.0546)	(0.0791)	(0.0653)
Observations	2,271	2,239	2,027	898	1,341
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	1447	1395	898	1341
Mean Control	0.317	0.314	0.294	0.364	0.280

Notes: 2SLS estimation results. The dependent variable is a dummy which is = 1 if respondent says the policy was adopted in municipality, 0 otherwise. Information Session is a dummy which is = 1 if the municipality's mayor attended the information session about tax reminders, 0 otherwise. This last variable is instrumented with treatment assignment. In the rows below the coefficients, 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". We express all continuous variables as indicators of above-below the median of the distribution of municipalities. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0); Electoral Margin of Victory above-below median (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.

TABLE 8: POLICY-ADOPTION EXPERIMENT: ACCURACY OF BELIEFS AND POLICY ADOPTION: TAX REMINDERS

Panel A	(1)	(2)	(3)	(4)	(5)
LHS Variable	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs
Information Session	1.3975	1.3541	1.5031	1.1923	1.5125
	(0.5209)	(0.5201)	(0.5589)	(0.7396)	(0.6839)
Mean Control	-6.980	-6.983	-6.998	-6.869	-7.060
Panel B	(1)	(2)	(3)	(4)	(5)
LHS Variable	Adopted	Adopted	Adopted	Adopted	Adopted
Accuracy of Beliefs	0.0856	0.0935	0.0819	0.1344	0.0799
	(0.0500)	(0.0537)	(0.0483)	(0.1084)	(0.0562)
Mean Control	0.310	0.306	0.285	0.357	0.271
Observations	2,172	2,141	1,936	842	1,299
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)	1434	1416	1360	842	1299

Notes: 2SLS estimation results where Treatment Assignment is the instrument for Information Session (in Panel A) and for Accuracy of Beliefs (in Panel B). In Panel A, the dependent variable—Accuracy of Beliefs—is the absolute difference multiply by -1 between self-reported beliefs about effect sizes of tax reminders on local tax revenues and the 12 percent informed effect size of the reminder letters policy during the information session. Information Session is a dummy which is = 1 if the municipality's mayor attended the information session about tax reminders, 0 otherwise. In Panel B, the dependent variable is a dummy which is = 1 if respondent says the policy was adopted in municipality, 0 otherwise. Accuracy of Beliefs is the absolute difference multiplied by -1 between self-reported beliefs about effect sizes of tax reminders on local tax revenues and the 12 percent informed effect size of the reminder letters policy during the information session. In the rows below the coefficients of the last panel, 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". We expressed all continuous variables as indicators of above-below the median of the distribution of municipalities. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0); Electoral Margin of Victory above-below median (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.

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TABLE A.1: BELIEFS EXPERIMENT: SELECTION INTO THE EXPERIMENT - MAYORS

Variables	Mean Non-Participants	Δ Participants	P-Value
Mayors' Characteristics			
Male	87.12	1.08	0.70
Age	48.61	-0.72	0.42
College	52.05	4.65	0.28
2nd Term	21.80	-4.08	0.22
Electoral Margin Victory	14.99	-1.19	0.32
Leftist Political Party	38.36	7.74	0.06
Municipalities' Characteristics			
Population	20.89	-2.80	0.30
College Population	4.09	-0.11	0.49
Public Adm College	32.25	0.39	0.74
Poverty	34.15	0.61	0.42
Gini	51.19	0.02	0.96
Per Capita Income	378.03	-5.55	0.57
Kids in School (0-3)	19.84	0.45	0.63
Kids in School (4-5)	83.30	-1.01	0.33
Joint F-test			0.44
Mayors	367	235	

Notes: Sample means of the mayors that did not participate in the beliefs experiment and differences in means with respect to participants. Differences in means were calculated including conference fixed effects. We restrict the analysis to the 12 regional conferences for which CNM shared the list of attendees. There were 367 mayors that did not participate in the beliefs experiment and 235 that did participate. The first block of variables reports characteristics of the mayor that runs the municipality. Leftist Political Party (= 1 for mayors belonging to a center-leftist party according to historical political platforms, 0 otherwise). The second block of variables reports characteristics of the municipality. Population is the municipality's number of inhabitants (in thousands). College Population is the municipality's share of adults with college degrees. Public Administration College is the share of municipal public employees with college degrees. Poverty is the municipality's poverty rate. Gini is the municipality's Gini coefficient. Per Capita Income is the municipality's monthly income per capita. Kids in School (0-3) is the share of kids 0-3 years old in the municipality that attend pre-school education. Kids in School (4-5) is the share of kids 4-5 years old in the municipality that attend pre-school education. Joint significance F-test is reported.

TABLE A.2: BELIEFS EXPERIMENT: PRIOR BELIEFS

Beliefs About	Mean	SD	Effect Size in Study
Own Municipality	0.42	(0.22)	-
123; Michigan	0.45	(0.22)	0.87
130; Jamaica	0.42	(0.21)	0.91
1420; Colombia	0.37	(0.19)	0.26
4667; USA	0.50	(0.22)	0.15

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

TABLE A.3: BELIEFS EXPERIMENT: WILLINGNESS TO PAY: OTHER DETERMINANTS

LHS Variable	(1) WTP	(2) WTP	(3) WTP	(4) WTP
Mayors' Characteristics				
Male	6.76			6.57
	(3.16)			(3.13)
Age	-0.81			-0.92
	(2.10)			(2.12)
College	1.16			2.60
	(2.16)			(2.21)
2nd Term	1.19			1.33
	(2.60)			(2.89)
Margin Victory	0.71			1.18
	(2.12)			(2.09)
Leftist Political Party	0.93			0.40
	(2.15)			(2.22)
Municipalities' Characteristics				
Population		2.14		1.52
		(2.23)		(2.21)
College Population		-0.03		-0.27
		(2.78)		(2.80)
Public Adm College		1.77		0.29
		(2.30)		(2.31)
Poverty		1.86		2.45
		(5.70)		(5.65)
Gini		-1.03		-1.14
		(2.51)		(2.50)
Big South		0.44		3.24
		(4.65)		(4.66)
Per Capita Income		-0.76		-0.01
		(5.09)		(5.05)
Kids in School (0-3)		1.10		0.62
		(2.32)		(2.33)
Kids in School (4-5)		2.00		2.05
		(2.42)		(2.40)
ECD Policy Survey Characteristics				
Mayor			-1.07	-1.00
			(2.08)	(2.17)
Prof Politician			-0.50	-1.44
			(2.34)	(2.48)
Leftist Scale			0.06	0.44
			(2.50)	(2.56)
Implemented ECD			11.45	11.99
			(2.39)	(2.49)
Heard ECD			6.84	6.88
			(2.68)	(2.79)
Observations	2,542	2,573	2,573	2,542
Clusters (Individuals)	754	764	764	754
Mean LHS	44.27	44.62	44.62	44.27

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); Electoral Margin of Victory above-below median (1/0); and 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.

TABLE A.4: BELIEFS EXPERIMENT: BELIEF UPDATING: DROPPING INATTENTIVE TYPES & DEALING WITH MEASUREMENT ERROR (IV)

	Dropping Inattentive Types			Measurement Error IV	
	(1)	(2)	(3)	(4)	(5)
LHS Variable	Posterior	Posterior	Posterior	Posterior	Posterior
Prior	0.6824	0.6002	0.5297	0.8149	0.7929
	(0.0214)	(0.0245)	(0.0242)	(0.0261)	(0.0375)
Signal	0.3230	0.4114	0.4626	0.2381	0.2532
	(0.0194)	(0.0218)	(0.0206)	(0.0251)	(0.0311)
Observations	1,240	944	553	438	438
Round	1 and 2	1 and 2	1 and 2	2	2
Beliefs About	Municipality	Municipality	Municipality	Municipality	Municipality
Received Study for Free	No	No	No	Yes	Yes
Instrument Prior	No	No	No	No	Yes
Drops Never Updaters	No	Yes	Yes	No	No
Includes Only $0 <$ Average Update < 1	No	No	Yes	No	No
Clusters (Individuals)	757	562	323	438	438

Notes: OLS results (columns 1-4) and 2SLS (column 5). The first three columns compare belief updating for the full sample (Col 1), dropping individuals who never update (Col 2), and keeping only individuals with updating weights $0 < \pi < 1$ (Col 3). The next two columns report an attempt to deal with measurement error in the priors. Specifically, Col 4 shows the usual updating regression for Round 2. Col 5 instead instruments for the prior in Round 2 using the randomized study provided in Round 1. The very similar coefficients on Prior in Cols 4 and 5 suggest measurement error plays a limited role in attenuating the coefficient. In all regressions, the dependent variables are posterior beliefs. Prior is the belief of the respondent about the effect, right before receiving some study. Signal is the received study's effect size. In the rows below the coefficients, Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality or one of the four possible study locations. Received Study for Free indicates whether participant received the information regardless of their WTP. Average Update is defined as (Posterior - Prior) / (Signal - Prior). Robust standard errors clustered at the individual level are in parentheses.

TABLE A.5: BELIEFS EXPERIMENT: BELIEF UPDATING: HETEROGENEOUS EFFECTS - MAYOR CHARACTERISTICS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
LHS Variable	Post	Post	Post	Post	Post	Post	Post	Post	Post
Characteristic		Male	Age	College	2nd Term	Margin Victory	Leftist Scale	Implem ECD	Heard ECD
Prior	0.6747	0.6985	0.6232	0.7575	0.6961	0.6792	0.7034	0.6561	0.6710
	(0.0301)	(0.0817)	(0.0436)	(0.0480)	(0.0335)	(0.0422)	(0.0362)	(0.0422)	(0.0338)
Signal	0.3240	0.3065	0.3636	0.2492	0.3062	0.3237	0.2899	0.3198	0.3363
	(0.0287)	(0.1051)	(0.0433)	(0.0409)	(0.0312)	(0.0407)	(0.0328)	(0.0425)	(0.0335)
Prior * Characteristic		-0.0278	0.0982	-0.1329	-0.1057	-0.0071	-0.1043	0.0461	0.0023
		(0.0880)	(0.0591)	(0.0615)	(0.0741)	(0.0609)	(0.0649)	(0.0603)	(0.0740)
Signal * Characteristic		0.0200	-0.0759	0.1209	0.0932	-0.0014	0.1248	0.0011	-0.0337
		(0.1093)	(0.0569)	(0.0564)	(0.0782)	(0.0580)	(0.0654)	(0.0574)	(0.0640)
Observations	623	623	623	623	623	614	623	623	623
Respondent	Mayor	Mayor	Mayor	Mayor	Mayor	Mayor	Mayor	Mayor	Mayor
Round	1 and 2	1 and 2	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	Municipality	Municipality
Received Study for Free	No	No	No	No	No	No	No	No	No
Clusters (Individuals)	377	377	377	377	377	371	377	377	377

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. Mayors' characteristics included as interactions are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd term (1/0); Electoral Margin of Victory above-below median (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. In the rows below the coefficients, Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality or one of the four possible study locations. Received Study for Free indicates whether participant received the information regardless of their WTP. Robust standard errors clustered at the individual level are in parentheses.

TABLE A.6: BELIEFS EXPERIMENT:
BELIEF UPDATING: HETEROGENEOUS EFFECTS - MUNICIPALITY CHARACTERISTICS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
LHS Variable	Post	Post	Post	Post	Post	Post	Post	Post	Post
Characteristic	Pop	College Pop	College Adm	Poverty	Gini	Big South	Income pc	Kids in School 0-3	Kids in School 4-5
Prior	0.7081	0.6736	0.6416	0.6543	0.6878	0.7081	0.7088	0.7144	0.7110
	(0.0322)	(0.0275)	(0.0296)	(0.0336)	(0.0316)	(0.0282)	(0.0268)	(0.0276)	(0.0308)
Signal	0.2850	0.3202	0.3666	0.3341	0.3104	0.3169	0.3133	0.3032	0.2756
	(0.0299)	(0.0247)	(0.0258)	(0.0324)	(0.0293)	(0.0249)	(0.0238)	(0.0264)	(0.0282)
Prior * Characteristic	-0.0455	0.0189	0.0938	0.0464	-0.0097	-0.0563	-0.0678	-0.0733	-0.0486
	(0.0432)	(0.0434)	(0.0423)	(0.0435)	(0.0428)	(0.0430)	(0.0440)	(0.0428)	(0.0427)
Signal * Characteristic	0.0664	0.0091	-0.1031	-0.0182	0.0229	0.0131	0.0254	0.0434	0.0819
	(0.0392)	(0.0399)	(0.0388)	(0.0404)	(0.0391)	(0.0396)	(0.0408)	(0.0389)	(0.0384)
Observations	1,240	1,240	1,240	1,240	1,240	1,240	1,240	1,240	1,240
Respondent	All	All	All	All	All	All	All	All	All
Round	1 and 2	1 and 2	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	Municipality	Municipality
Received Study for Free	No	No	No	No	No	No	No	No	No
Clusters (Individuals)	755	755	755	755	755	755	755	755	755

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. Municipalities' characteristics included as interactions are: 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. In the rows below the coefficients, Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality or one of the four possible study locations. Received Study for Free indicates whether participant received the information regardless of their WTP. Robust standard errors clustered at the individual level are in parentheses.

TABLE A.7: BELIEFS EXPERIMENT: BELIEF UPDATING: LARGE-SAMPLE AND DEVELOPING-COUNTRY STUDIES - DIFFERENT EFFECT SIZES

LHS Variable	(1) Posterior	(2) Posterior	(3) Posterior	(4) Posterior
Prior	0.5470	0.4248	0.6987	0.5587
	(0.0588)	(0.0891)	(0.0702)	(0.1026)
Signal	0.3619	0.4219	0.2860	0.3677
	(0.0379)	(0.0540)	(0.0507)	(0.0607)
Prior * Developing	0.0083	0.0251	-0.0077	-0.0606
	(0.0577)	(0.0855)	(0.0763)	(0.0948)
Signal * Developing	-0.0110	-0.0255	0.0102	0.0511
	(0.0424)	(0.0616)	(0.0593)	(0.0752)
Prior * Large	0.0206	0.0442	-0.0467	0.0834
	(0.0706)	(0.1049)	(0.0930)	(0.1217)
Signal * Large	0.3123	0.3091	0.4067	0.1615
	(0.0951)	(0.1196)	(0.1624)	(0.1720)
Prior * Long-run	0.1128	0.1950	0.0354	0.1336
	(0.0811)	(0.1198)	(0.0971)	(0.1258)
Signal * Long-run	-0.0145	-0.0669	0.0338	-0.0461
	(0.0650)	(0.0930)	(0.0850)	(0.0942)
Prior * Developing * Long-run	-0.1068	-0.2067	-0.0624	-0.1214
	(0.0898)	(0.1387)	(0.1019)	(0.1299)
Signal * Developing * Long-run	0.1797	0.3021	0.1047	0.2361
	(0.0894)	(0.1386)	(0.1064)	(0.1346)
Prior * Large * Long-run	-0.0581	-0.1725	0.1354	-0.1292
	(0.1060)	(0.1526)	(0.1227)	(0.1571)
Signal * Large * Long-run	-0.0704	0.0854	-0.4099	0.0469
	(0.1553)	(0.2175)	(0.2032)	(0.2541)
Observations	1,131	645	486	493
Round	1 and 2	1	2	1
Context	Municipality	Municipality	Municipality	Random Study
Received Study for Free	Yes	Yes	Yes	Yes
Clusters (Individuals)	731	645	486	493

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. Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality or one of the four possible study locations. Received Study for Free 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: 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.

TABLE A.8: BELIEFS EXPERIMENT: VALUATION OF IMPLEMENTATION REPORT

	OLS	IV
	(1)	(2)
LHS Variable	Implementation Report	Implementation Report
Final Posterior	16.4909	41.1943
	(5.2677)	(21.0608)
Observations	737	737
Instrument Final Posterior	No	Yes
Clusters (Individuals)	737	737
Mean LHS	59.72	59.72
SD LHS	33.69	33.69

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. In column 2, this last variable is instrumented with 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. SD LHS is the standard deviation of policy implementation report valuation on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses.

TABLE A.9: POLICY-ADOPTION EXPERIMENT: INDIVIDUAL AND MUNICIPAL PREDICTORS OF SESSION PARTICIPATION

LHS Variable	(1) Information Session	(2) Information Session	(3) Information Session
Mayors' Characteristics			
Male	0.0157		-0.0014
	(0.0546)		(0.0560)
Age	-0.0719		-0.0771
	(0.0328)		(0.0336)
College	0.1616		0.1562
	(0.0328)		(0.0333)
2nd Term	-0.0007		0.0057
	(0.0448)		(0.0456)
Electoral Margin Victory	0.0265		0.0231
	(0.0326)		(0.0330)
Leftist Political Party	0.0314		0.0379
	(0.0347)		(0.0352)
Municipalities' Characteristics			
Population		-0.0079	-0.0141
		(0.0343)	(0.0340)
College Population		0.0634	0.0492
		(0.0466)	(0.0463)
Public Adm College		-0.0345	-0.0442
		(0.0339)	(0.0337)
Poverty		-0.1015	-0.0753
		(0.0903)	(0.0926)
Gini		0.0449	0.0462
		(0.0382)	(0.0382)
Big South		0.0258	0.0593
		(0.0662)	(0.0660)
Per Capita Income		-0.0762	-0.0663
		(0.0839)	(0.0854)
Local Tax Revenues (2010-2015)		-0.0245	-0.0166
		(0.0459)	(0.0454)
Constant	0.2876	0.4343	0.3376
	(0.0631)	(0.0937)	(0.1124)
Observations	874	878	871

Notes: Linear probability results. Response variable is information session participation and is = 1 for mayors that attended the information session, 0 otherwise. We expressed all continuous variables as indicators of above-below the median of the distribution of municipalities. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0); Electoral Margin of Victory above-below median (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.

TABLE A.10: POLICY-ADOPTION EXPERIMENT: ITT POLICY ADOPTION: TAX REMINDERS

LHS Variable	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted
Treatment Assignment	0.0402	0.0418	0.0395	0.0477	0.0419
	(0.0208)	(0.0208)	(0.0212)	(0.0325)	(0.0251)
Observations	2,271	2,239	2,027	898	1,341
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	1447	1395	898	1341
Mean Control	0.317	0.314	0.294	0.364	0.280

Notes: OLS estimation results. The dependent variable is a dummy which is = 1 if respondent says the policy was adopted in municipality, 0 otherwise. Treatment Assignment is a dummy which is = 1 if the mayor was assigned to the treatment group. In the rows below the coefficients, Attention Check refers to whether respondents 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". We expressed all continuous variables as indicators of above-below the median of the distribution of municipalities. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0); Electoral Margin of Victory above-below median (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.

TABLE A.11: POLICY-ADOPTION EXPERIMENT:
POLICY ADOPTION: HETEROGENEOUS EFFECTS TAX REMINDERS - MAYOR CHARACTERISTICS

LHS Variable	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted	(6) Adopted
Characteristic	Male	Age	College	2nd Term	Margin Victory	Leftist Party
Information Session	-0.0544	0.1648	0.1481	0.1084	0.1445	0.0700
	(0.2007)	(0.0666)	(0.1051)	(0.0588)	(0.0758)	(0.0691)
Information Session * Characteristic	0.1715	-0.1263	-0.0663	-0.0321	-0.0802	0.0913
	(0.2081)	(0.1065)	(0.1212)	(0.1373)	(0.1065)	(0.1072)
Characteristic	0.0077	0.0241	0.0081	0.0149	0.0126	-0.0234
	(0.0483)	(0.0287)	(0.0293)	(0.0398)	(0.0287)	(0.0301)
Observations	2,271	2,271	2,271	2,271	2,241	2,271
Respondent	All	All	All	All	All	All
Attention Check	No	No	No	No	No	No
Clusters (Municipalities)	1465	1465	1465	1465	1448	1465
Mean Control	0.317	0.317	0.317	0.317	0.314	0.317

Notes: 2SLS estimation results. The dependent variable is a dummy which is = 1 if respondent says the policy was adopted in municipality, 0 otherwise. Information Session is a dummy which is = 1 if the municipality's mayor attended the information session about tax reminders, 0 otherwise. This last variable is instrumented with treatment assignment. In the rows below the coefficients, 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". We expressed all continuous variables as indicators of above-below the median of the distribution of municipalities. Mayors' characteristics included as interactions are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0); Electoral Margin of Victory above-below median (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.

TABLE A.12: POLICY-ADOPTION EXPERIMENT: POLICY ADOPTION: HETEROGENEOUS EFFECTS TAX REMINDERS - MUNICIPALITY CHARACTERISTICS

LHS Variable	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted	(6) Adopted	(7) Adopted	(8) Adopted
Characteristic	Population	College Pop	PubAdm College	Poverty	Gini	Big South	Pc Income	Local Taxes
Information Session	0.1158	0.0764	0.1439	0.1344	0.1958	0.1001	0.0743	0.0567
	(0.0713)	(0.0840)	(0.0741)	(0.0679)	(0.0787)	(0.0877)	(0.0803)	(0.0790)
Information Session * Characteristic	-0.0276	0.0458	-0.0805	-0.0794	-0.1758	0.0130	0.0488	0.0689
	(0.1068)	(0.1084)	(0.1060)	(0.1081)	(0.1063)	(0.1099)	(0.1066)	(0.1063)
Characteristic	0.0069	0.0408	0.0397	-0.0777	-0.0312	0.0855	0.0707	0.0415
	(0.0287)	(0.0290)	(0.0288)	(0.0291)	(0.0288)	(0.0294)	(0.0289)	(0.0289)
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 is = 1 if respondent says the policy was adopted in municipality, 0 otherwise. Information Session is a dummy which is = 1 if the municipality's mayor attended the information session about tax reminders, 0 otherwise. This last variable is instrumented with treatment assignment. In the rows below the coefficients, 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". We expressed all continuous variables as indicators of above-below the median of the distribution of municipalities. Municipalities' characteristics included as interactions 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.

TABLE A.13: POLICY-ADOPTION EXPERIMENT: POLICY ADOPTION: TAX REMINDERS INFORMATION COMPONENTS

LHS Variable	(1) On Time	(2) Audit	(3) Social Norm	(4) Before Due	(5) Letter
Information Session	0.1014	0.0720	0.0990	0.0884	0.0752
	(0.0522)	(0.0471)	(0.0374)	(0.0515)	(0.0413)
Observations	2,239	2,239	2,239	2,239	2,239
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)	1447	1447	1447	1447	1447
Mean Control	0.310	0.220	0.112	0.290	0.155

Notes: 2SLS estimation results. The dependent variable is a dummy which is = 1 if respondent says the policy was adopted in municipality, 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 is = 1 if the municipality's mayor attended the information session about tax reminders, 0 otherwise. This last variable is instrumented with treatment assignment. In the rows below the coefficients, 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". We expressed all continuous variables as indicators of above-below the median of the distribution of municipalities. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0); Electoral Margin of Victory above-below median (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.

TABLE A.14: POLICY-ADOPTION EXPERIMENT: POLICY ADOPTION: FINANCIAL INCENTIVES AND E-PROCUREMENT

LHS Variable	(1) Financial Incentives	(2) E-procurement
Information Session	0.0033	0.0153
	(0.0557)	(0.0644)
Observations	2,177	1,675
Respondent	All	All
Attention Check	No	No
Mayor Characteristics	Yes	Yes
Municipal Characteristics	Yes	Yes
Clusters (Municipalities)	1434	1178
Mean Control	0.600	0.447

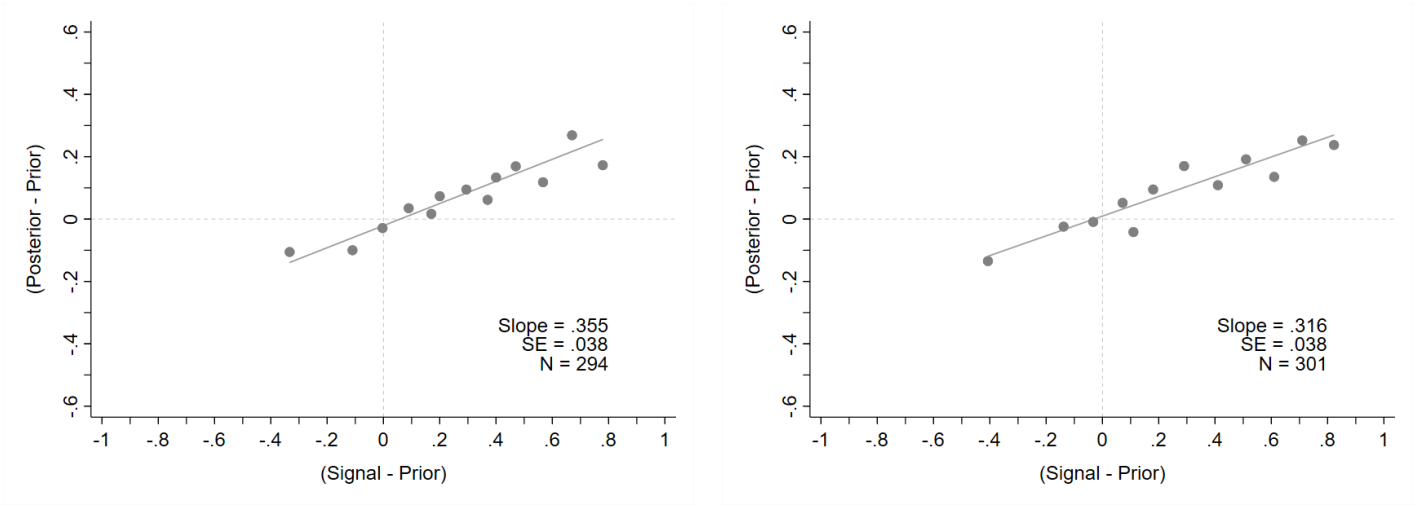
Notes: 2SLS estimation results. The dependent variable is a dummy which is = 1 if respondent says the policy was adopted in municipality, 0 otherwise. Information Session is a dummy which is = 1 if the municipality's mayor attended the information session about tax reminders, 0 otherwise. This last variable is instrumented with treatment assignment. In the rows below the coefficients, 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". We expressed all continuous variables as indicators of above-below the median of the distribution of municipalities. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0); Electoral Margin of Victory above-below median (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.

TABLE A.15: POLICY-ADOPTION EXPERIMENT: ACCURACY OF BELIEFS AND CONFIDENCE: TAX REMINDERS AND FINANCIAL INCENTIVES

	Tax Reminders			Financial Incentives		
Panel A - ToT	(1)	(2)	(3)	(4)	(5)	(6)
LHS Variable	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs
Information Session	1.2931	1.2289	1.3797	-0.4037	0.1659	-0.6523
	(0.5209)	(0.7433)	(0.6815)	(0.5309)	(0.7959)	(0.6845)
Mean Control	-6.971	-6.869	-7.039	-7.028	-6.741	-7.219
	Tax Reminders			Financial Incentives		
Panel B - ITT	(1)	(2)	(3)	(4)	(5)	(6)
LHS Variable	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs	Accuracy of Beliefs
Treatment Assignment	0.5144	0.5022	0.5382	-0.1577	0.0661	-0.2510
	(0.2073)	(0.3058)	(0.2670)	(0.2074)	(0.3204)	(0.2641)
Mean Control	-6.971	-6.869	-7.039	-7.028	-6.741	-7.219
	Tax Reminders			Financial Incentives		
Panel C - ToT	(1)	(2)	(3)	(4)	(5)	(6)
LHS Variable	Confidence	Confidence	Confidence	Confidence	Confidence	Confidence
Information Session	0.0740	0.0848	0.0822	-0.0164	-0.3352	0.1979
	(0.1073)	(0.1730)	(0.1368)	(0.1129)	(0.1783)	(0.1419)
Mean Control	-0.019	0.109	-0.105	-0.002	0.129	-0.089
	Tax Reminders			Financial Incentives		
Panel D - ITT	(1)	(2)	(3)	(4)	(5)	(6)
LHS Variable	Confidence	Confidence	Confidence	Confidence	Confidence	Confidence
Treatment Assignment	0.0295	0.0348	0.0321	-0.0064	-0.1335	0.0762
	(0.0429)	(0.0717)	(0.0536)	(0.0443)	(0.0712)	(0.0548)
Mean Control	-0.019	0.109	-0.105	-0.002	0.129	-0.089
Observations	2,155	845	1,310	2,125	830	1,295
Respondent	All	Mayor	Finance Staff	All	Mayor	Finance Staff
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)	1422	845	1310	1414	830	1295

Notes: 2SLS (panels A and C) and OLS (panels B and D) estimation results. In panels A and B, the dependent variable is the absolute difference multiply by -1 between self-reported beliefs about effect sizes of the policy on local tax revenues and the 12 percent informed effect size of the reminder letters policy during the information session. In panels C and 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 is = 1 if the municipality's mayor attended the information session about tax reminders, 0 otherwise. This last variable is instrumented with Treatment Assignment. Treatment Assignment is a dummy which is = 1 if the mayor was assigned to treatment group, 0 otherwise. In the rows below the coefficients of the last panel, 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". We expressed all continuous variables as indicators of above-below the median of the distribution of municipalities. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College (1/0); 2nd Term (1/0); Electoral Margin of Victory above-below median (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.

FIGURE A.1: BELIEFS EXPERIMENT: BELIEF UPDATING BY STUDY



(a) Michigan

(b) Jamaica

(c) Colombia

(d) US

Notes: Comparison between the difference in respondent's perceptions after buying some study (i.e. posterior beliefs minus prior beliefs) and the difference in respondent's perceptions before buying some study (i.e. signal minus prior beliefs), averaged over bins of rounds 1 and 2. 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. Panel (a) shows statistics for Michigan study. Panel (b) shows statistics for Jamaica study. Panel (c) shows statistics for Colombia study. Panel (d) shows statistics for US study. The slope and robust standard errors clustered at the individual level are based on a linear regression with a constant term.

B Beliefs Experiment: Script

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.

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.

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

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

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.

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.

BELIEFS EXPERIMENT:
POSTERIOR ELICITING: OWN MUNICIPALITY

Context 1: **Your own municipality**

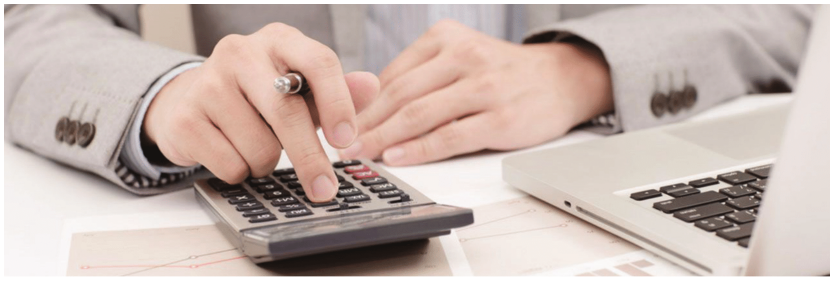
Suppose the Early Childhood Development program is implemented for children from low-income families in **your municipality**. Recall that the study of **123** children from low-income families in **Ypsilanti, a city in the state of Michigan, USA,** found an effect of **0.87** points, but the effect in your municipality might be different.

Q19. 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

C Policy-Adoption: Policy Brief

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)

How to Increase Compliance with Local Taxes • 1

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.

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

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.

D Policy-Adoption: Selection

TABLE D. 1: POLICY-ADOPTION EXPERIMENT: SELECTION INTO THE EXPERIMENT

Variables	Mean Sample Frame	Δ Sample	P-Value	Δ Information Session	P-Value
Mayors' Characteristics					
Male	88.70	0.25	0.78	0.53	0.77
Age	48.91	-1.51	0.00	-2.00	0.00
College or more	53.78	3.59	0.01	13.58	0.00
2nd Term	20.94	-4.49	0.00	-3.57	0.12
Electoral Margin Victory	17.31	-0.41	0.44	0.74	0.49
Leftist Political Party	34.36	-0.37	0.78	3.06	0.26
Municipalities' Characteristics					
Population	21.51	-0.68	0.19	0.46	0.67
College Population	5.25	-0.16	0.05	-0.03	0.83
Public Adm College	32.03	0.90	0.03	1.34	0.11
Poverty	25.61	0.66	0.20	-0.87	0.41
Gini	50.18	0.06	0.74	-0.11	0.76
Big South	54.15	-3.43	0.01	-0.56	0.84
Per Capita Income	463.67	-4.37	0.49	9.00	0.48
Local Taxes Revenues (2010-15)	6.42	-0.31	0.03	-0.18	0.54
Municipalities	4,026	1,818		334	

Notes: Means of the sample frame of municipalities and differences in means with respect to experimental sample and information session attendees. The first block of variables reports characteristics of the mayor that runs the municipality. Leftist Political Party (= 1 for mayors belonging to a center-leftist party according to historical political platforms, 0 otherwise). The second block of variables reports characteristics of the municipality. Population is the municipality's number of inhabitants (in thousands). College Population is the municipality's share of adults with college degrees. Public Administration College is the share of municipal public employees with college degrees. Poverty is the municipality's poverty rate. Gini is the municipality's Gini coefficient. Big South is = 1 for municipalities in the south, southeast and mid-west regions, 0 otherwise. Per Capita Income is the municipality's monthly income per capita. Local Tax Revenues (2010-2015) indicates the average share of municipal tax revenues in total municipal revenues from 2010 to 2015.

E Politically-Motivated Updating

Politically-motivated reasoning is thought to be a source of persistent disagreement over facts (Kahn, 2016). 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.* (2019); Nyhan & Reifler (2010)). However, much of this evidence comes from studies without incentives for truthful reporting (with Thaler (2019) being a notable exception). More importantly, we have little evidence on potential biases in political leaders' belief-updating. 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).[35] 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:

$$Y_{ij} = \beta_0 + \beta_1 PartisanMessage_i + \beta_2 Leftist_i + \beta_3 PartisanMessage_i \times Leftist_i + \varepsilon_{ij} \quad \text{(E.1)}$$

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:

$$Posterior_{ijs} = \beta_1 Prior_{ij} + \beta_2 (Signal_{ij} - Prior_{ij}) + \beta_3 (Signal_{ij} - Prior_{ij}) \times PositiveSurprise_{ij} + \beta_4 (Signal_{ij} - Prior_{ij}) \times PositiveSurprise_{ij} \times PartisanMessage_i + \varepsilon_{ij} \quad \text{(E.2)}$$

Table E. 1 reports the results. Columns 1 and 2 present effects on priors and WTP from specification (E.1). 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 (E.2) separately for left-wing and right-wing policy-makers to study belief updating. We find no evidence that 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

[35] 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.

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 E. 1 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. Fourth, ECD programs are generally not a highly charged political topic. Politically-motivated reasoning may well occur in other domains and contexts. Finally, if citizens enagage in politically-motivated reasoning, this may affect the policy choices of even unbiased policy-makers.

TABLE E. 1: 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)	(6.1909)				
Partisan Message*Leftist	0.0749	8.5497				
	(0.0617)	(8.6162)				
Prior			0.9071	0.9982	0.9798	1.0049
			(0.0632)	(0.0394)	(0.0343)	(0.0247)
Signal-Prior			0.2226	0.3413	0.3153	0.2857
			(0.1639)	(0.0923)	(0.0810)	(0.0349)
Signal-Prior * Positive Surprise			0.3666	-0.0799	-0.0252	
			(0.2181)	(0.1131)	(0.0984)	
Signal-Prior * Positive Surprise * Partisan Message			-0.1752	0.0475		
			(0.1326)	(0.0695)		
Signal-Prior * Positive Surprise * Leftist					0.2641	
					(0.1040)	
Signal-Prior * Positive Surprise * Leftist * Partisan Message					-0.1842	
					(0.1302)	
Signal-Prior * Partisan Preferred Signal						0.2038
						(0.0737)
Signal-Prior * Partisan Preferred Signal * Partisan Message						-0.1908
						(0.0894)
Observations	311	876	99	363	462	462
Round	1 and 2	1 and 2	1 and 2	1 and 2	1 and 2	1 and 2
Beliefs About			Municipality	Municipality	Municipality	Municipality
Received Study for Free			Yes	Yes	Yes	Yes
Clusters (Individuals)	311	339	68	259	327	327
Mean LHS	0.388	48.96				

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 variables are 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. 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. Preferred Signal is a dummy which is equal to one if the participant self-identified as leftist (or rightist) and the bought study's effect is greater (smaller) than the respondent's prior. In the rows below the coefficients, Beliefs About specifies which location the beliefs are elicited for, either the respondent's own municipality or one of the four possible study locations. Received Study for Free 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.