Pontifícia Universidade Católica
do Rio de Janeiro

Juan Francisco Santini

Essays on State Effectiveness: Information, Tax Collection and Hierarchies

Tese de Doutorado

Thesis presented to the Programa de Pós–graduação em Economia of PUC-Rio in partial fulfillment of the requirements for the degree of Doutor em Economia.

Advisor: Prof. Cláudio Abramovay Ferraz do Amaral

Rio de Janeiro
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Abstract

Santini, Juan Francisco; Ferraz, Cláudio (Advisor). **Essays on State Effectiveness: Information, Tax Collection and Hierarchies**. Rio de Janeiro, 2019. 215p. Tese de doutorado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

This thesis consists of three chapters. In the first one we investigate whether the provision of information about research findings regarding effectiveness of policies cause political leaders to enact policy change. To do so, we use two types of experiments to measure elected heads of government: (1) demand for research and (2) policy responses to supply of research. We find that policymakers are willing to pay relatively high amounts to learn the results of impact evaluations, update their posterior over the expected impact of a policy in the "correct" direction if informed of research findings, and pay more for types of studies that subsequently affect their beliefs more. Correspondingly, providing information about research findings indicating positive, cost-effective impact of a policy increases the probability that mayors implement the policy in their own municipality by 10 percentage points. In the second chapter, we study the role that revenue shocks play on government investment in fiscal capacity. Using a difference-in-difference event-study design, we analyze local budget adjustments to an exogenous revenue shock in formula transfers to Brazilian municipalities. We find that positive revenue shocks translate into additional spending, while the adjustment after a negative shock depends on local characteristics. On average, municipalities increase tax collection, but this effect disappears in jurisdictions with low-educated mayors, which rather tend to cut expenditures. We show that hiring tax related workers is the main mechanism behind the increase in tax revenues. In the third chapter, we follow the theory of knowledge-based hierarchies to study the internal organization of municipal governments in Brazil. Using detailed matched employer-employee data, we construct bureaucrats' hierarchies within municipalities and show that the empirical patterns match the theoretical predictions. We then present suggestive evidence linking the organization structure in which bureaucrats operate and public sector productivity.

Keywords

State Capacity Information Revenue Shocks Organizations Bureaucracies

Resumo

Santini, Juan Francisco; Ferraz, Cláudio. **Ensaios sobre a Efetividade do Estado: Informação, Arrecadação Tributária e Hierarquias**. Rio de Janeiro, 2019. 215p. Tese de Doutorado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Esta tese é composta por três capítulos. No primeiro, nós investigamos se o fornecimento de informações sobre resultados de pesquisas sobre a efetividade de políticas públicas faz com que líderes políticos implementem mudanças em políticas públicas. Para isso, nós usamos dois tipos de experimentos para medir: (1) demanda por pesquisas e (2) ações de políticas públicas em resposta ao fornecimento de pesquisas. Nós concluímos que formuladores de políticas públicas estão dispostos a pagar quantias relativamente altas para descobrir os resultados de avaliações de impacto, atualizam suas crenças sobre os impactos esperados de uma política pública na direção "correta" se informados sobre resultados de pesquisa, e pagam mais por tipos de estudos que subsequentemente afetam mais suas crenças. Da mesma forma, fornecer informações sobre resultados de pesquisas indicando impactos positivos e custo-efetivos de uma política pública aumenta a probabilidade de que prefeitos implementem esta política em seu próprio município em 10 pontos percentuais. No segundo capítulo, nós estudamos o papel que choques de receita desempenham em investimentos governamentais em capacidade fiscal. Usando um desenho de estudo de eventos em um modelo de diferenças-em-diferenças, analisamos os ajustes do orçamento municipal a um choque exógeno das receitas de transferências constitucionais. Na média, municípios aumentam a coleta de impostos, mas esse efeito desaparece em jurisdições com prefeitos de baixa escolaridade, que, em vez disso, tendem a cortar despesas. Mostramos que a contratação de trabalhadores da área fiscal é o principal mecanismo por trás do aumento de receitas tributárias. No terceiro capítulo, nós seguimos a teoria de hierarquias baseadas em conhecimento para estudar a organização interna de governos municipais no Brasil. Usando dados detalhados de empregados e empregadores, nós construímos hierarquias de burocratas dentro de municípios e mostramos que os padrões empíricos se correspondem às previsões teóricas. Nós, então, apresentamos evidências sugestivas ligando a estrutura organizacional em que burocratas operam com a produtividade do setor público.

Palavras-chave

Capacidade do Estado Informação Choques de Receita Organizações Burocracias

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Do Research Findings Influence Policy? Experimental Evidence from Brazilian Municipalities

1.1
Introduction

Recent decades have seen an explosion of program-evaluation research in economics.[1] But how interested in and open to academic research *are* politicians? And, insofar as they "consume" research, can and do they act on new findings?[2] These are questions of fundamental importance for the entire social-science ecosystem. Despite the money and effort devoted to evaluating the impact of various policies, we have little understanding of whether the political agency 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 or discontinue* policies that they otherwise wouldn't 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 leverage a unique collaboration with the National Confederation of Brazilian Municipalities (*Confederação Nacional de Municípios*, or CNM) to provide direct, experimental evidence on these questions. We first report results from a series of demand-for-research experiments conducted with 900 municipal officials (mayors, vice-mayors, council members, and municipal secretaries, or "ministers") at 14 CNM meetings during 2017 and 2018. In addition to local political leaders' willingness-to-pay (WTP) to learn the findings from rigorous studies of the effectiveness of various policies, these experiments allow us to assess how such research findings affect the leaders' beliefs. To estimate the ultimate impact on actual policy adoption, we use a larger-scale supply-of-research experiment with 1,818 Brazilian municipalities that began at a massive CNM convention in 2016. A randomly-selected treatment group of 881 mayors was invited to attend an hour-long research information session.

[1] For example, more than 2000 studies have been registered with the American Economic Association's registry for randomized controlled trials (RCTs) since its launch in May 2013.

[2] A view among some practitioners and academics is that lack of knowledge is rarely a binding constraint on policy optimization (Sachs, 2005).

A presenter informed the audience about the findings of a set of RCTs showing positive effects on tax compliance of a taxpayer reminder-letter policy. We then measured policy adoption at the municipality level 15 to 24 months later using surveys of both finance-department officials and mayors. In combination, the demand-for- and supply-of-research experiments allow us estimate both the extent to which research findings can influence policy in Brazil, and the political leader, polity, and research-study level attributes that mediate such influence.

Brazil's municipalities are an ideal setting in which to investigate how research affects policy use for two reasons. First, Brazilian mayors hold a role analogous to that of many countries' head of state: they are directly elected and individually wield considerable de jure power over policy choices within the areas municipalities control, 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 taxes and service taxes, in addition to the federal and state transfers they receive. Second, there are 5,570 municipalities in Brazil—the 1,818 that make up our sample for the supply-of-research experiment constitute 45 percent of municipalities in the 5,000–100,000 population range we focus on[3]—and our collaboration with CNM gives us direct access to their leadership. This enabled us to carry out policy change experiments at polity level, which to our knowledge has not been possible to do before.[4]

Our demand experiment begins by eliciting the participant's belief about the likely impact of a carefully-described Early Childhood Development (ECD) program on children's test scores.[5] We then present the participant with the study design—mentioning the study's location and sample size—of a randomly-selected study out of a set of four comparable published papers which evaluated such ECD programs using RCTs.[6] We elicit the participant's WTP to learn the result of the study using an incentive-compatible procedure, and then randomize whether the individual actually receives the result (conditional on

[3]96 percent of Brazilian municipalities have less than 100,000 inhabitants.

[4]In this sense the existing study closest to ours is Hoffmann et al. (2017). They carry out an innovative lab-in-the-field style-but-incentive-compatible choice experiment in which elected county councilors in Kenya chose among alternative water infrastructure projects. Our supply-of-research procedure is instead a "natural" field experiment (List, 2009), in which political leaders make policy decisions in the manner that they normally do, except with access to better information about the effectiveness of a particular policy.

[5]We chose this policy because its impact is well-documented in existing, rigorous research in multiple settings.

[6]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.

their WTP). After revealing the results of the study, we elicit the participant's posterior beliefs about the likely effect of the policy if implemented in their municipality. We also elicit incentivized beliefs about the effect in other contexts, where the policy was actually implemented and evaluated. Finally, we offered the participant the opportunity to pay for advice on how to implement the relevant ECD program in practice.

We find that municipal officials to begin with hold widely varying beliefs about the impact of the policy we focus on, and that they are willing to pay an arguably fairly high amount—about USD 36 on average—to find out the results of an impact evaluation. The average WTP is higher for studies with a large sample size, and among officials from municipalities that implemented a similar program in the past, but not for studies from a location that is closer to Brazil's income level. Finding out the results of an RCT leads municipal officials to update their beliefs about impact: their posterior is a weighted average of their prior and the revealed study's findings, with more weight on the prior when forming beliefs about the impact in the official's own municipality. The officials are willing to pay more—and update their beliefs more in response to—studies with a large sample size.[7] Finally, when instrumenting for officials' posterior with whether she was randomized into finding out the relevant study's findings, we find that a higher posterior increases the official's WTP for practical implementation information. These findings from the demand-for-research experiment suggest that supplying municipal officials with research findings should influence their policy decisions; the hypothesis we investigate in our second experiment.

In the supply-of-research experiment, we assigned a randomly chosen subset of the mayors registered for CNM's 2016 *Novos Gestores* convention in Brasília to a treatment group, and a comparable group of registered mayors to a control group.[8] Both groups were asked to conduct a brief survey eliciting characteristics of the mayor and her municipality, as well as the extent to which she believed a policy of sending taxpayers letters encouraging them to pay their taxes on time would increase municipal revenues. We chose this policy both because its impact—like that of the ECD program used in the demand-for-research experiments—is well-documented in existing, rigorous research, and because it is cheap and easy to implement. Only the treatment group

[7]We also find suggestive evidence that officials from poorer municipalities within Brazil update their beliefs more when expose to information from developing country studies.

[8]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.

was invited to attend a research information session. During the information session, an experienced and charismatic presenter gave a 30-minute slideshow-based presentation of the background studies and their findings, for example highlighting the estimated impact of letters respectively drawing attention to the tax payment deadline, the risk of audit if not paying, and social norms regarding paying taxes.[9] At the end of the session, mayors were given printed information materials and told how practical implementation advice could be obtained. 15-24 months after the convention, we attempted to reach all municipalities in the treatment and the control group by phone to elicit beliefs about impact and whether or not an encouragement letter policy had been implemented in the municipality.[10]

We find that providing mayors with information about research findings documenting the positive impact of a policy increases the probability that they implement the policy by a remarkable 10 percentage points, or 32 percent relative to the 31 percent of municipalities in the control group which had implemented the policy at some point in the past.[11] The effect of supportive research findings is biggest on implementation of unusual variants of the policy, such as taxpayer encouragement letters focusing on the social norm of paying taxes—underscoring the importance of information transmission itself.[12]

In combination, the findings from our demand-for-research and supply-of-research experiments make clear that political leaders are interested in; update their beliefs in response to; and ultimately act on information about research findings.

This paper contributes to three related but distinct strands of the literature on state effectiveness. The body of research viewing states and sub-state polities as enterprises—with hiring systems, incentive structures, and other operational features that influence organizational effectiveness—has focused primarily on front-line public sector workers (for an excellent survey, see Finan et al., 2017), and, to a lesser extent, bureaucrats (Duflo et al., 2013,

[9]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 and Scartascini (2015); Hallsworth et al. (2017).

[10]From February to November 2018, we called mayors and key bureaucrats with knowledge of each municipality's tax policies (typically secretaries of finance). In 81 percent of the municipalities in the sample, at least one person was surveyed. There was no differential attrition between treatment and control municipalities.

[11]Relative to the proportion of control group municipalities that implemented the policy we focus on during our data period, the impact of the supply-of-research treatment on policy take-up was likely massive. Our survey asked the respondents only if they were currently operating the policy.

[12]The unusual variants finding is one of several indicating that our results are not due to experimenter demand effects. We also asked about a placebo variant of the policy. We find no evidence of the placebo having differential effects between treatment and control municipalities.

ming; Nath, 2015; Khan et al., 2016, 2018; Best et al., 2017; Bertrand et al., 2018; Rasul and Rogger, 2018) and leaders' identities (Chattopadhyay and Duflo, 2004; Jones and Olken, 2005; Besley et al., 2011; Beaman et al., 2012; Martinez-Bravo, 2014; Yao and Zhang, 2015; Easterly and Pennings, 2017; Martinez-Bravo, 2017; Xu, 2018; Bertrand et al., 2018). We instead focus on information frictions constraining leaders' decisionmaking. Using what to our knowledge is the first polity level field experiment[13]—the closest analogue to which is randomized management interventions in private firms (Bloom et al., 2013)—we show that political leaders' lack of knowledge of policies' effectiveness directly affects their policy decisions. 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 or unable to act on new research information.

By starting to unpack how political leaders' beliefs are shaped, we also advance an emerging body of evidence on belief formation. Beliefs about policies' effectiveness—even those of academic experts—are often inaccurate (DellaVigna and Pope, 2018a). We first evaluate the extent to which political leaders—the decisionmakers who hold policymaking authority—in developing countries—where information acquisition is especially challenging (Delavande et al., 2011)—on average update their beliefs in response to new research findings.[14] However, the impact of new knowledge on decisions depends not only on how decisionmakers update their beliefs if exogenously exposed to the research findings; it also depends on the distribution of willingness-to-pay for information acquisition, and how much decisionmakers of different levels of WTP update their beliefs in response to acquired information. Our demand-for-research experiment to our knowledge provides the first direct evidence on the extent to which policitical leaders are interested in new research, how much leaders of different levels of WTP update their beliefs in response to new findings, and how this depends on characteristics of the research.[15]

Finally, this paper contributes to the body of research on what motivates politicians that began with Downs (1957) (see Persson and Tabellini, 2002 for a great overview of the literature). Two of our findings are especially

[13]The pioneering studies by Fujiwara and Wantchekon (2013) and Bidwell et al. (2018)—and related studies in political science—randomize how electoral campaigns take place across electoral districts or villages and the study the impact on electoral outcomes. Our focus is on policy decisions in seated municipal governments.

[14]Our supply-of-research experiment builds on the influential *information provision* methodology used by Chetty et al. (2009) and Jensen (2010) and many subsequent studies (see, among others, Kling et al., 2012; Chetty and Saez, 2013; Dizon-Ross, 2018). Beynon et al. (2012) use an online experiment with a self-selected sample of policy practitioners to study the optimal design of policy briefs.

[15]See also DellaVigna and Pope (2018b).

informative (Besley and Case, 1995): first, that providing information about research findings to mayors influences policy take-up, and second, that this effect appears not to vary with whether the political leader is up for reelection or not. In combination, these two findings are hard to reconcile both with citizen-candidate type models in which politicians make policy decisions that reflect their own preferences (Besley and Coate, 1997), and with conventional distributive politics theories in which politicians care only about getting reelected (Myerson, 1993; Lizzeri and Persico, 2001; Golden and Min, 2013). In this sense our findings are encouraging, pointing towards local political leaders in Brazil attaching weight to the welfare of their citizens (see also Finan and Mazzoco, 2016; Hoffmann et al., 2017).[16] Our findings underscore the general importance of information frictions in politicians' policy choices.[17]

The rest of the paper proceeds as follows. Section 1.2 provides institutional information about Brazilian local governments and our partner organization. Section 1.3 presents the design and results from the demand-for-research experiments. Section 1.4 discusses our second intervention, the supply-of-research experiment, and finally we conclude in section 1.5.

1.2 Institutional Background and Context

This section provides relevant background information on municipal governments in Brazil, our partner organization, and the conferences where the interventions we use to study how policymakers value, process, and make use of policy-research information took place.

[16]Insofar as providing supportive research findings increased mayors' beliefs not only that the policy we focused on was effective—as we show—but also that implementing it would—perhaps as a consequence—increase incumbents' probability of getting reelected in the next election (see Banerjee et al., 2018), these two findings in combination contrast even with models in which politicians, in addition to the welfare of their constituents, put some weight on getting (re)elected (Finan and Mazzoco, 2016; Hoffmann et al., 2017).

[17]From the vantage point of traditional economic theories, 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 strong incentives for anyone interested in enhancing social welfare who has access to academic outlets to "connect" policy research and practice. While organizations such as The Abdul Latif Jameel Poverty Action Lab (J-PAL), Innovations for Poverty Action (IPA), and the Center for Effective Global Action (CEGA) have helped institutionalize and scale up information transmission only relatively recently, think tanks and similar organizations have operated in the research-to-policy space for many decades. Part of the explanation for the persistence of information frictions in the policy space may be that arbitrageurs need not only to exist but also to have significant capacity to arbitrage away information frictions (Shleifer and Summers, 1990).

1.2.1 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 appoints 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 fair. This means that, despite the general limitations of democratic political systems, there is a system in place in Brazil to reward and punish politicians for their performance. In addition to the executive branch, Brazilian municipalities have a legislative branch, which reviews and approves the annual budget, participates in the elaboration of local laws, and oversees the mayor's administration.

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

In general, municipal governments are highly autonomous. The mayor negotiates the budget allocation with the city councilors and has full autonomy over its execution. The mayor's office thus holds policymaking 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 1.3 and 1.4.

1.2.2 Our partner organization

This study leveraged a unique opportunity to conduct a series of large-scale experiments with thousands of local policymakers 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 policymakers in an environment that is familiar to the policymakers. Meeting attendees comprise mayors, vice-mayors, local legislators, and municipal secretaries. Our demand-for-research experiments were 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).[18] Our supply-of-research experiment was conducted at CNM's best-attended 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 information interventions were one of the many activities that took place at these meetings. The meetings are each approximately three days long, and are structured around different training sessions and presentations by various political actors, including regional actors such as regional associations of municipalities, and public and private municipal suppliers, as well as national ones such as CNM itself, federal government officials, congress representatives, and often the Brazilian President. In addition to attending the presentations, local policymakers use the meetings to organize get-togethers with each other and with state and federal officials. Each national conference brings around 4,000 municipal representatives and 2,000 mayors, while the regional conferences attract around 200 local policymakers, of which approximately 50 are mayors. Thus, our experiments take place in a quite natural setting, where policymakers are used to receiving useful information.

1.2.3 Identifying target policies

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

To identify policy areas of interest to local policymakers, 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-K education, preventive health care, and management prac-

[18]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.

tices. 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 negotiations 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 impact evaluated in existing, rigorous research, and the taxpayer reminder letter policy we focus on in the supply-of-research experiment is especially cheap and 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 policymakers' responses.

1.3 How Heads of Government Value and Process Research Information

In this section we show that Brazilian policymakers value academic research information and update their beliefs in response to new findings. We do so using demand-for-research experiments carried out at national and regional meetings of mayors and their staff.

1.3.1 Experimental setting

We implemented demand-for-research experiments at 14 CNM meetings during 2017 and 2018. 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 its national meetings CNM is mainly interested in mayors participating, but in the regional conferences vice-mayors, municipal secretaries and local legislators are also encouraged to participate.

We designed a series of 25-minutes-long survey experiments that were administered on tablets. A team of research assistants recruited local policymakers during breaks in between sessions at the conferences. Participation was voluntary but incentivized as discussed in the next section. Participants took

the surveys by themselves, with supervision by one of the researchers and one research assistant throughout and guidance where necessary. Figure A.1 shows a common setting for our demand-for-research experiments.

The survey began with a set of questions in which the participant reported his/her position in the municipality, whether he/she had worked as a local policymaker in the previous term, gender, political party, and ideology. Then, and only then, we introduced the specific policy the experimental component of the process focused on: early childhood development (ECD) programs.

1.3.2 The demand-for-research experiments

The structure of the experiment is rather simple. We first introduce the policy, then elicit participants' priors about the effectiveness of the policy and their willingness-to-pay (WTP) to learn research findings on the impact of the policy. Next, we randomly reveal the findings, and finally, we elicit participants' posteriors to assess the extent to which research findings affect policymakers' beliefs.[19]

After the participant background questions, the survey followed with the introduction of the policy. We described early-childhood development programs, highlighting the key outcomes through which the impact of such programs is measured (test scores, cognitive skills) and how those outcomes are generally 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 the participants with a few benchmarks for effect sizes.[20] At the end of the introductory stage we asked comprehension questions about what they had read. Participants were allowed to move forward only after having answered correctly, or receiving assistance and further clarifications from the research team.

We began the main part of the experiment by eliciting the participant's priors. Specifically, we asked her what she believed would be the impact of the policy on children's cognitive skills if the policy were to be implemented in her own municipality. Immediately after, we asked a similar question about the expected impact in two other locations. These two other locations were randomly chosen for the participant from a menu of careful studies that estimate the impact of four ECD programs. The studies are Grantham-McGregor et al. (1991); Walker et al. (2005); Puma et al. (2010); Barnett

[19]Figure A.2 in Appendix describes the structure of the experiment.

[20]Figures A.3 A.4 and A.5 in Appendix show a print of the description of the program, and the provided examples and benchmarks.

(2011); Attanasio et al. (2014). They use different sample sizes, and estimate the impact in respectively Jamaica (first two studies), the U.S. as a whole, Michigan, and Colombia. Table 1.1 presents these studies' characteristics. When the relevant studies were presented to the participant, we highlighted the study location and sample size.

After the participant reported her prior, we offered her one of the two (randomly chosen) studies for purchase. We endowed each participant with 100 lottery tickets, each with a chance of winning a free trip (flights and four hotel nights) to visit the Harvard University campus in the United States. Lottery tickets served two purposes in the experiment: as incentives for participation and as experimental currency. Participants could save all lottery tickets for the trip 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 randomized a price for the study. If the price was below the participant's WTP, we revealed the findings, and vice versa. Most participants received the findings because the price was drawn from a distribution with high mass at zero. Subsequently, those that acquired the information were again asked about their expected impact of the policy in their own municipality and in the study location of the study that was *not* offered for purchase.

In the next stage we followed a similar procedure to record a second (updated) posterior. We now presented the participant with all three remaining studies, again highlighting each study's location and sample size. The participant received another 100 lottery tickets and was told that she would be able to purchase one of the three studies. In this part the participant could thus compare the main attributes of the studies before declaring anything. However, we emphasized that the participant would only (potentially) gain access to one of the three studies, and therefore asked her to report her WTP for each study. We then randomized which study would apply for the participant in question and proceded with the BDM procedure as in the earlier step. We registered the updated posterior of the effect of the ECD policy in the participant's own municipality for those who got access to the findings.

We also cross-randomized two additional components. The first was whether we incentivize (or not) the belief elicitation regarding the randomly chosen other location. The studies provide the correct effect size in each of the locations. Thus, in the incentivized arm we offered additional lottery tickets depending on how close the declared prior was to the true effect size. Throughout the paper we do not differentiate between incentivized and non-

incentivized elicitation of beliefs. We return to this distinction in Section 1.3.5. The second cross-randomization was whether the participant was informed (correctly) that Brazil's past left-of-center governments strongly supported the use of ECD programs.[21] This helps us assess the extent of politically-motivated bias in information processing. Throughout the paper we do not differentiate between whether the partisan message was shown. We return to the partisan-support message and its implications for information processing in Section 1.3.5.

Finally, we offered participants the opportunity to purchase an implementation report with further information about the "what", "why", and "how" of ECD programs, and where to find additional information on how to implement such a policy in practice. With this last exercise we attempted to capture their WTP for policy adoption guidance.

1.3.3 Sample and baseline balance

Participants eligible for the experiment were all mayors, vice-mayors, council members, and municipal secretaries that attended any of the 14 CNM's conferences in which the demand-for-research experiment was implemented. Almost 49 percent of the policymakers that participated in the demand-for-research experiments were mayors, 28 percent local legislators, 16 percent municipal secretaries, and 7 percent vice-mayors.[22] Table 1.2 displays summary characteristics of the municipalities run by these policymakers. We see, for instance, that 88 percent of municipalities are run by men mayors, around 37 percent have mayors affiliated to a leftist political party, and approximately 20 (78) percent of the children 0 to 3 (4 to 5) years old attend a pre-K educational establishment. On the other hand, from the background questions we know that 44 percent of the participants reported that their municipalities had implemented ECD programs previously.[23]

[21] The message was: "In Brazil, the PT government strongly advocated for early-childhood development policies. In 2007, Fundeb was created to include pre-K and nursery enrollment in the headcount for federal transfers. Programs for pre-K and nursery construction, such as the *pro-Infancia* program, were also launched during Lula's government, and expanded during Dilma's administration." A similar message was shown before WTP elicitation.

[22] We are unable to determine the exact proportion of participants that completed the survey out of the population of eligibles at the conferences. CNM did not share the list of total attendees of all conferences and its respective positions in the municipal government. However, from the conferences that we do have attendees' information, we were able to complete the experiment with 38.8% of mayors, 48.5% of vice-mayors, 35.4% of municipal secretaries and 40.9% of local legislators.

[23] Aside from our individual-level survey questions, we only have individual-level information of participants occupying an elective position. Thus, we choose to present summary statistics at the municipality-level.

The characteristics of the participants that chose to take part in the experiment and their municipalities are important to understand sample selection. Unfortunately, due to confidentiality restrictions we do not have information of all conferences' attendees and its municipalities. Consequently in table A.1 we present mean differences at the municipality-level between survey participants and non-participants, restricting the sample to the conferences in which we have attendees' information. The table shows that municipalities are unbalanced in five characteristics. Survey participants, on average, belong to municipalities where the mayor have a higher probability of being affiliated to a leftist party and is more educated, and where the share of municipal bureaucrats with a college degree is larger. Furthermore, experimental respondents are policymakers from municipalities where children attend pre-K education at slightly higher rates. Most importantly, however, for the design of the experiment is that the research attributes have been randomly assigned to participants. Namely, that the study location and sample size of the offered study for purchase be orthogonal to participant's characteristics. To investigate this we regress each participant's individual and municipal characteristic that we have available on a dummy equals to one if the offered study is from a developing country, 0 otherwise, and on a dummy equals to one if the study has a large sample size, 0 otherwise. Table 1.3 shows the results pooling all the studies that were randomly offered for purchase to the participant. Out of the nineteen variables that we consider, only one correlates with the sample size attribute and three with the location of the study. This indicates that the randomization generated by the software was relatively successful. Nonetheless, we contemplate the small unbalance in the location attribute when interpreting our main results.

1.3.4 Results

We outline in Appendix A.3 a very simple framework to help interpret the results. We use a standard bayesian framework and link the research findings attributes—external validity, sample size and effect size—with the parameters in the policymakers' decision making. Through the lens of this framework, we outline predictions on how such attributes shape the demand for research-findings and the beliefs about the effectiveness of the policy. We use the random assignment of studies to policymakers, and thus studies' attributes, to test such predictions. In Section 1.3.5 we consider deviations from this simple bayesian framework.

Priors about Effect Size. We start by analyzing policymakers' priors

about the effectiveness of ECD policies. We ask policymakers about their priors regarding the effectiveness in their municipality as well as in an alternative location (one of the four studies' locations). Appendix figures A.6 and A.7 present the script on how they were asked. The average policymaker prior is very sensible. Table 1.4 shows that the average prior suggests that ECD policies are believed to be more effective in a developed country context, reporting effect size of around 0.46-0.49, rather than in a developing country with a reported effect size of 0.38-0.41. Interestingly, on average, policymakers believe the effect size in their municipality is in between Colombia and Jamaica. Thus positioning themselves as in a developing country context. The next step is to test whether policymakers are sophisticated in the processing of new information. We draw predictions from a simple bayesian framework presented in Appendix A.3. The framework has predictions about the willingness-to-pay for research information and the posterior about the effectiveness of ECD policies.

Willingness-to-Pay for Effect Size. After policymakers revealed their priors they were asked about their willingness-to-pay (hereafter WTP) to access the research findings of one of the four (randomly assigned) studies. According to the framework, WTP gets larger the more precise is the information signal. We test this prediction using the following specification:

$$WTP_{ijs} = \beta_0 + \beta_1 Developing_{ijs} + \beta_2 Large_{ijs} + \varepsilon_{ijs} \tag{1-1}$$

WTP_{ijs} is the WTP for the research findings (i.e. the effect size found in the study). According to the way policymakers were asked, this is the maximum a policymaker is willing to spend from the 100 lottery tickets (i.e. experimental currency) that were given to him or her to either learn about the effect size of a particular study s or participate in a lottery to travel to the US. As explained before, the same policymaker i is offered twice, in round 1 and round 2, indexed by j, the opportunity to buy research information about different studies s. The main attributes that we regard from the studies capture the external validity and sample size. $Developing_s$ equals one for studies in Jamaica or Colombia and 0 otherwise. $Large_s$ equals one for the two large-sample studies and 0 otherwise. Standard errors are clustered at the individual level.

We interpret the attributes of a study as determinants of the precision of the information signal of that given study. We assume that the two attributes of the research both individually contribute to a more precise signal to the effect size in the policymakers' own municipality. The assumption is very straightforward with respect to sample size: large sample studies should provide

a more precise signal of the effect size. The analogous for developing country location deserves further clarification. In table 1.4 we show that on average policymakers position their municipality with a similar impact size as a developing country. We thus initially consider that information from developing country locations should provide a more precise signal for the policymaker than research findings from developed countries. We revisit this assumption, and discuss a more general case in Section 1.3.5. We expect that β_1 and β_2 are both positive: as the information signal that is being offered to be purchased has greater precision (from a developing country study or large-sample study) the policymakers should have greater WTP for it.

Table 1.5 presents the OLS results of specification 1-1. Column 1 pools the two rounds together, while column 2 and 3 present estimates per round, round 1 and round 2 respectively. Policymakers allocate on average 45 lottery tickets (out of the 100 tickets they are offered) to learn about the effect size of a particular study. By offering them to exchange the experimental currency with a *Lojas Americanas* gift card, we recovered their money value for the experimental currency.[24] Table 1.6 shows that a lottery ticket was exchanged by USD 0.80.[25] In total the money value of the research finding was about 45 lottery tickets and thus USD 36. This shows policymakers were interested in the research finding. Yet, such interest might be orthogonal to what the research is truly offering. We find quite a sensible demand-for-research pattern.

The demand for the findings vary with the attribute of the research. Large sample size studies have a WTP that is 8% larger than the average study. This is consistent with the first prediction where there should be greater demand for studies that offer a more precise signal of the effect size. The relationship is stronger in the second round when studies are offered side-by-side, but not statistically different from the first round (p-value 0.496). We do not find significant differences between the WTP for research information from developing versus developed countries. Is important to acknowledge that throughout the conferences we vary the precise design of the experiment in terms of the number of rounds in which we offered studies for purchase. Thus, in Appendix table A.2 we show that results are similar restricting the sample of participants to those that faced the exact same experimental design (p-value round 1 vs round 2 is equal to 0.786). The results are also robust to alternative definitions of large sample studies in Appendix table A.3.

[24] *Lojas Americanas* is a retail chain that sells a variety of products in physical stores and online all over Brazil.

[25] As can be seen from the table, participants were willing to pay 6.78 lottery tickets more for a gift card of R$ 25 than for a gift card of R$ 5. Thus, the value in R$ of the experimental currency can be calculated as R$ 20 divided by 6.78 lottery tickets = R$ 2.94. Considering an exchange rate of R$ 3.70 per USD, it gives USD 0.80 per lottery ticket.

We also analyze other determinants of WTP. Our design does not experimentally vary determinants other than the research attributes just discussed. Thus, these additional results are associations and should not be interpreted as evidence in favor or against the basic framework. We estimate a generalized version of equation 1-1, precisely:

$$WTP_{ijs} = \beta_0 + \beta_1 Determinant_{ijs} + \varepsilon_{ijs} \tag{1-2}$$

$Determinant_{ijs}$ are 19 different potential determinants of WTP including policymakers' characteristics and municipal characteristics. Standard errors are also clustered at the participant level. Appendix table A.4 presents OLS estimates of specification 1-2. Two characteristics out of the nineteen are strongly associated with WTP. These are: whether the participant declared that his or her municipality implemented a ECD policy before, and whether the participant reported that he or she had already heard about the policy. Both variables are positively associated with WTP. One may argue, however, that past experience with ECD policies should be negatively associated with the willingness to pay (rather than positively associated as in Appendix table A.4). The rationale being that as you have a more precise prior, the benefit of new information should be lower, lowering the WTP. Past experience with ECD policies may be, however, correlated with greater benefits of having access to the policy—for example if they are planning to stop the program in their municipality. Unfortunately, we did not experimentally varied this condition and thus we are limited in what we can say. This seems a fruitful avenue for future research.

Posteriors about Effect Size. After revealing their WTP, the software randomly generates a price for the research information (i.e. the effect size found by the particular study that was offered to be purchased). By design 90% of participants are assigned a "price" zero and receives the research information. Thus, the research information was randomly assigned irrespective of WTP in most cases. We study the causal consequence for the posterior after the policymaker was randomly exposed to a study with a particular effect size. The framework in Appendix A.3 outlines two predictions with respect to the posterior. First, the posterior should be a weighted average with positive weights on the prior as well as on the information signal. Second, the weight on the information signal should be larger the larger is the precision of the signal. We test each of these predictions using the following specifications respectively:

$$Posterior_{ij} = \beta_1 Prior_{ij} + \beta_2 Signal_{ij} + \varepsilon_{ij} \quad (1\text{-}3)$$

And

$$Posterior_{ij} = \beta_1 Prior_{ij} + \beta_2 Signal_{ij} + \beta_3 Signal_{ij} \times Developing_{ij} + \beta_4 Signal_{ij} \times Large_{ij} + \varepsilon_{ij} \quad (1\text{-}4)$$

$Posterior_{ij}$ is the policymakers' (i) belief about the effect size in their own municipality after having access the research findings either in round 1 or 2, indexed by j. Appendix figure A.8 presents the script on how policymakers were asked about the posterior. $Prior_{ij}$ is the previously reported beliefs about the effect size in their own municipality. When $Posterior_{i2}$, concerns round 2, then the $Prior_{ij}$ is the $Posterior_{i1}$. $Signal_{ij}$ is the effect size of the bought study. $Large_{ij}$ and $Developing_{ij}$ are defined as in equation 1-1.

Table 1.7 presents the OLS results of specification 1-3. Column 1 pools the two rounds, while columns 2 and 3 present estimates per round, round 1 and round 2 respectively. Consistent with the framework predictions, the estimand of β_1 and β_2 are both positive and statistically significant. More generally, the updating is quite sophisticated for a few reasons. First, policymakers put larger weight in their prior than in the information signal. This suggests that policymakers are updating based on the research finding but they are not accepting whatever the researchers say. It is a intentional calculated report of their belief. Second, they put greater weight in their prior when it refers to their municipality than when it refers to an alternative location. This is noted by comparing column 4 and column 2. In column 4 the posterior and the prior are both referring to the effect of a ECD policy in an *alternative location* of a random study (instead of their *own municipality* like in columns 1-3). This is also true, although weakly, when we restrict the sample of participants to those that faced the exact same experimental design (Appendix table A.5). Lastly, the weight on the prior increases in the second round after they already had access to one research finding and thus has a more precise prior.[26]

Table 1.8 presents the OLS results of specification 1-4. Again, column 1 pool the two rounds, while columns 2 and 3 present estimates per round. We start from the reduced form association on how WTP correlates with the updating. Columns 1-4 presents the results on whether larger WTP for a study is associated with a larger weight on the signal from that study. An increase

[26]The p-value of the t-test of whether the prior of round 2 is different than the prior of round 1 is equal to 0.000.

in 1 standard deviation of the WTP is associated with weight on the signal that is 10.4% larger. There are however many determinants of WTP, most of which we do not experimentally vary. To have a causal interpretation, we focus on how the research attributes, $Large_{ij}$ and $Developing_{ij}$ impact the weight on the signal. Columns 5-8 shows that the weight on the signal is larger for large sample studies but not for developing country studies. This is consistent with the framework prediction—as the optimal weight on the signal should be larger the more precise is the signal. Remember that $Large_{ij}$ was also the only attribute that were found to be relevant to explain WTP in specification 1-1. Thus it is reassuring that policymakers have a higher value for large-sample studies and also put a higher weight on the signal that comes from large-sample studies.

Caveats and Qualifications Taken together the evidence on the prior, WTP and posterior are all consistent with the main predictions of the simple framework and show that policymakers are quite sophisticated in how they value and process research findings. There are nonetheless two caveats we would like to discuss in greater detail.

Developing/Developed country lack of impact. While policymakers are willing to pay more experimental currencies for large sample and put larger posterior weights on signals from such studies, we do not find a similar pattern with respect to Developing/Developed country. One potential explanation is that Brazil is very diverse. It is possible that some policymakers representing municipalities in the poorest regions of the country, perceive Jamaica and Colombia closer to their reality. While others, in the wealthiest parts of the country perceive to be closer to the US. If that is the case, for the latter set of municipalities, studies from Developed countries represent a more informative signal than studies from developing countries. The evidence we find is mixed. Appendix table A.6 reports the results on whether certain characteristic of the mayor of the municipality, the policymaker, the public administration or the municipality affect the weight given to the signal for studies with a particular attribute. Consistent with the diversity of municipalities attenuating the partial effect of developing country attribute, we find that policymakers in less well off places put larger weight on the signal when the study is from a developing country. Precisely, municipalities that present higher share of the population under the poverty line and higher Gini coefficient. However, this same characteristics do not explain WTP for the developing country attribute of a study (Appendix table A.7), and recall that Gini coefficient is one of the characteristics that is unbalanced in the location attribute.

Effect size vs large sample. The interpretation of the results of specifica-

tion 1-4, which refers to the posterior, may suffer from a caveat embedded in the experimental design. Important to emphasize that this potential limitation does not affect the results on the prior and WTP. We chose to randomize a true study which has certain attributes (e.g. large sample, developing country, effect size).[27] There were four studies to be randomized, each of which completing the 2-by-2 matrix of possibilities regarding the first two attributes (large/small sample size; developing/developed country). The chance of each of these two attributes to appear is orthogonal to each other and equally likely. The problem arises when we think about the third dimension: effect size. Some effect sizes are larger than others and these might be correlated with the first two attributes (large/small sample size; developing/developed country). In fact, large sample size studies on average document a much smaller impact. This is a feature in our 4 studies sample and also more generally documented in the ECD literature (Barnett, 2011). Hence, we propose an additional test to investigate whether policymakers were responsive only to the effect size when updating the beliefs or also to the large/small sample as initially we interpreted.

In six of the fourteen conferences that we participated, we artificially varied the effect size of certain studies. In practice, we reported a true effect size of that study but assessed in a much longer period of time which thus resulted in a smaller effect size. Appendix table A.8 tests whether the larger weight on the signal for large sample size studies is less pronounced in those conferences where we artificially manipulated the effect size. Consistent with our initial interpretation, policymakers do not seem to be updating only based on the effect size, as the impact of the research attributes on the signal weight does not vary significantly between conferences.

1.3.5 Deviations from the simple Bayesian framework

Policymakers' priors, WTP and posteriors were all consistent with a simple bayesian framework. While this suggests that policymakers are quite sophisticated in how they value and process information about research findings, the patterns might as well be consistent with alternative models. In this section we provide direct evidence against other interpretations. We consider a variety of deviations from the simple framework. Politically-motivated updating and policymakers underlying incentives.

[27]The choice to do not randomize attributes directly rather than studies, was primarily a moral one. If we had randomized the attributes instead we would have had to offer fake study results. We did not want to mislead policymakers and we chose to always present true findings of research.

Politically-Motivated Information Processing. Politically-motivated reasoning is thought to be a source of persistent disagreement over facts (Kahan, 2015). There is evidence that providing information on politically-charged topics can actually increase polarization, and that individuals neglect information inconsistent with their group's position. Most of this evidence is from student or Mturk samples with no incentives for truthful elicitation and often in a very artificial environment and artificial set of decisions. Little evidence from policymakers and with incentivized elicitations exists. We randomize messaging (truthfully) revealing strong support for the policy from past left governments (Lula and Dilma's governments from the Labor Party - PT). Precisely 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-K and nursery enrollment in the headcount for federal transfers. Programs for pre-K 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 a similar message is shown right before the WTP. The period in which this intervention took place, throughout 2017, the labor party in Brazil (PT) had passed through several corruption scandals and political losses which affected considerably their social image in the country.[28] As a result, in 8 years the PT lost half of the leadership of municipalities, 550 (in 2008) to 256 (in 2016) mayors (Velasco et al., 2016), and a little less than half of the seats in congress, 88 (in 2010) to 56 (in 2018) (Caesar, 2018). Thus, the objective of the message was to leverage the political tension existent at the time of the intervention to associate a neutral policy to a particular party that was facing very high rejection rates and observe whether this influenced priors, WTP and posteriors. The specifications is:

$$Y_{ij} = \beta_0 + \beta_1 Message_i \times Leftist_i + \beta_2 Leftist_i + \beta_3 Message_i + \varepsilon_{ij} \quad (1\text{-}5)$$

Where Y_{ij} is the policymaker elicited prior or WTP. $Leftist_i$ is a binary variable equal to 1 if the policymaker position himself closer to the left tail in a left-right ideology spectrum, and $Message_i$ is equal to one if policymaker received the partisan message, 0 otherwise. As we explained in

[28]A short list of events that damaged their image includes the President Dilma Rouseff impeachment (Garcia et al., 2016); Lula, the former president and one of the founders of the PT party, was sentenced to 12 years in jail (PR, 2018) and denied the right to be a candidate in the 2018 presidential election (Shalders, 2018); many high profile politicians affiliated with the party were caught and sentenced to jail as part of the Car Wash operation (Venturini, 2018), etc.

the experimental design, the message is assigned at the beginning and remains the same throughout the experiment, and thus it does not vary with the rounds (j).

Table 1.9 presents OLS results of specification 1-5. Among leftists, the message has a positive but statistically insignificant impact on their elicited priors. The effect on WTP is not robust, the point estimate varies substantially and it is statistically significant in one specification only (column 5). We consider alternative definitions of leftist, precisely whether the mayor of the municipality is associated with PT, whether the policymaker is affiliated with PT, and whether the policymaker is affiliated with a political party on the left—Appendix tables A.9 and A.10. The conclusion remained the same. Since the partisan message did not interfere on how policymakers' value and initially perceive the policy, it shouldn't affect how they process new information about the effectiveness of the policy, which we confirm in Appendix tables A.11 and A.12.

Incentives for truthful elicitation One may worry about the policymakers' underlying incentives driving the declared beliefs and valuations. Policymakers may declare beliefs without putting much thought into it, or might try to second-guess the correct answer, and thus report beliefs that feels right even when not truthful. Or even, they may demand the research findings regardless of the informational content, just to look good among the research team or the peers. Our survey design attenuate such concerns by using incentives for correct predictions.

Our design cross-randomized the stakes associated with a correct prediction. In practice it works as follows. Policymakers reported their prior about the effect size of an ECD policy in their own municipality. Then, before they are asked about the effect size guess in other randomly chosen location, for example, Colombia, they were informed about the incentive. If their effect size guess about Colombia was close to what the researchers had found they would get extra 'X' experimental currency. We then randomly vary the size of 'X'. This design accomplishes two objectives. First it makes sure that policymakers have a direct use of the research finding information—if they believe accessing the research findings helps them predict the effect size in other places they will increase their WTP to obtain more experimental currency. Without such incentive policymakers may not demand research findings even if they believe it may help with predictions: primarily because they have no budget in the near future to implement the policy. Second, and perhaps most importantly, the incentives may also nudge policymakers to think more carefully about each

declared belief.[29]

It is important to recognize that incentives may affect only beliefs about effect size for the other random location rather than effect size beliefs regarding their own municipality. This is because we do not know the true effect size on their municipality and cannot reward that. It is therefore reassuring that the weight on the signal relative to the prior, and differential effect by research attribute is observed for their own municipality (not incentivized) as well as for the randomly chosen location (incentivized). This was discussed previously and is found in table 1.8, in columns 6 and 8 respectively. We also test directly for whether the (randomly assigned) stake-levels impacted prior about random location, WTP and posterior and the interactions with the research attributes. Appendix table A.13 shows the results. Overall we find little evidence that the incentives alter policymakers beliefs and valuation.

1.3.6 How heads of government value and process research: summary

The demand-for-research experiments help us establish three main findings. First, policymakers are willing to pay to learn about research on policy effectiveness. They pay more for larger sample studies but not for developing-country studies. Second, they also change their beliefs when confronted with evidence from research: they place substantial weight on the new information. They place more weight on larger-sample studies, but again, not on developing-country studies. There is weak evidence that Brazil diverse set of municipalities, with wealthy and poor areas explains in part the lack of effect by country status. Third, the evidence does not support some deviations from the basic-bayesian framework such as partisan-motivated processing of information. In short, policymakers seem to both value evidence and update their beliefs in a quite sophisticated way. But, does access to research leads to more effective policies adopted?

At the very end of the experiment, policymakers are given the chance to purchase information about implementation challenges. We interpret this as their demand for implementing the policy. Since we experimentally vary research attributes—effect size, developing context and large sample—and is shown to affect posteriors, we use those to instrument the posterior, and thus present evidence of how research drives policy change through the shaping of policymakers beliefs. Appendix table A.14 shows the results.

[29]The downside of the incentive is that it complicates the interpretation of the WTP as a measure of demand for research information out of the experimental setting. We find, however, that the stakes matter little for WTP, and also that the average WTP remain similar even if we remove entirely the prediction exercise from the design.

This is the cleanest evidence on how policy-research findings alone, precisely the informational signal about the effect size that comes out of a study—not the researcher engagement, not the marketing of the graphs—drives policy change. Yet, one may worry about whether this indeed capture policy adoption for a real government policy. The supply-of-research experiment focus on whether the supply of research information affects the adoption of a real policy: tax reminder letters for taxpayers.

1.4 How Heads of Governments' Policy Decisions Respond to Supply of Research Information

The demand-for-research experiment discussed in the previous section showed that local Brazilian policymakers value academic research and update their beliefs in response to new findings. This suggest that supplying mayors and their staff with findings from policy-effectiveness research should influence their policy decisions. We investigate this hypothesis using a nationwide field experiment.

1.4.1 Experimental setting

The supply-of-research 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 incumbent and new mayors who are about to start their four-year term the following January. Each mayor participates in the conference for about two days, and can attend many of the parallel training sessions led by CNM staff. The training sessions cover a variety of public policy areas.

Municipalities in our sample were assigned to treatment and control following a randomization stratified the mayor's education level and her term limit, the average education level among public employees in the municipality, and its population size; Gini coefficient; and region. Both groups were free to attend any of CNM's regular *Novos Gestores* training sessions, and also encouraged—and incentivized through lottery tickets—to participate in our baseline survey. Only mayors in the treatment group were allowed to attend our research information session. We encouraged and incentivized them to do so.[30]

[30] As in the demand-for-research experiment, participants in the supply-of-research experiment received lottery tickets for a raffle of a plane ticket to Boston with four nights of hotel included and a visit to Harvard University. Every mayor that filled the baseline survey received one lottery ticket. Mayors from the treatment group received a second lottery ticket if they attended our information session.

1.4.2 The supply-of-research experiment

Our information session focused on a particular way that existing research suggests municipalities can increase revenues. The session lasted 45 minutes and was led by an experienced presenter. The presenter introduced mayors to the findings of a set of rigorous studies evaluating the effect of a simple-and-cheap-to-implement policy on tax revenues: taxpayer reminder letters.

As discussed in the Introduction, over 90 percent of Brazilian municipalities raise taxes locally—through taxes on real estate, professionals and businesses, and/or public services (such as street lighting, waste collection)—and many mayors see increasing fiscal capacity as a major priority (see Section 1.2). A prominent think tank estimates, for example, that at least 20 percent of taxpayers do not comply with the real estate taxes (De Cesare and Smolka, 2004).

Direct outreach to taxpayers to increase tax compliance via a reminder letter policy like the one we focus on is relatively uncommon in Brazil, but far from unheardof. In the endline survey we conducted for our supply-of-research experiment, 32 percent of mayors in the control group reported that their municipality was sending reminder messages to taxpayers.[31]

As discussed in Sub-section 1.2.3, we chose to focus on a taxpayer reminder letter policy because the impact of the policy has been carefully documented in existing rigorous research, and because the policy is easy and cheap to implement.[32] The information session was designed to be accessible, compelling, nonpartisan, and sufficiently short to sustain the attending mayors' attention. The session consisted of a 30 minutes presentation, plus 15 minutes for questions from the audience.[33] First, the presenter provided a brief description of the taxpayer-reminder policy, including a template of a reminder letter. She then gave the magnitude of the effect size found in the various existing studies. A list of reminder letter characteristics found to be effective in inducing taxpayers to pay their taxes on time—stating the tax payment deadline; the possibility of fines and audits for not paying taxes on time; and that most

[31] Our survey asked whether the municipality was sending reminder messages to taxpayers and the channels used for it: letters, text messages, media advertising (radio, tv, newspapers), flyers and posters, etc.

[32] The findings were based on Coleman (1996); Hasseldine et al. (2007); Del Carpio (2013); Fellner et al. (2013); Castro and Scartascini (2015); Hallsworth et al. (2017).

[33] During the 15 minutes reserved for open discussions with mayors, mayors 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.

people pay their taxes on time—was emphasized. At the end of the session, mayors received a professionally-produced policy brief with the same information content as presented.[34] As previously highlighted, control group mayors had total freedom to attend any *Novos Gestores*' training sessions with the exception of our research information session.

1.4.3 Data collection

Our primary data source on policy outcomes is in-depth phone survey of the municipal administrations that were part of the sample for the supply-of-research experiment. We called all municipalities in the control and the treatment group 15 to 24 months after the treatment. We attempted to reach the mayor and the bureaucrat in charge of implementing tax policy in each municipality.[35]

The survey was supervised by a research assistant, and conducted by a team of nine surveyors who did not have any knowledge of the purpose of the intervention. 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—50 percent of the mayors and 75 percent of the chief tax bureaucrats in the sample.[36] On average, 2.5 phone calls talking directly to the mayor were needed to get an appointment with him/her and complete a survey, and 2.3 for the chief tax bureaucrats.[37] The survey lasted approximately 15 minutes. The most important question asked the respondent if the taxpayer reminder letter policy was used by the municipality since the date of the information session. We also elicited beliefs about the effectiveness of taxpayer reminder letters. Likewise, we asked a similar set of questions about two other policies: financial incentives to taxpayers and usage of e-procurement.[38] We

[34]Appendix figures A.9, A.10, A.11, and A.12 show a print of the policy brief.

[35]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 kindly asked to get the phone number of the person in charge of implementing tax policy.

[36]We were not able to make contact with 10 percent of the sample municipalities. In some cases phone numbers were not working, and in other cases we never found a phone number for the municipality.

[37]On average, many hours of work were needed before we could talk to the mayors and chief tax bureaucrats over the phone, mainly collecting municipalities' phone numbers. Not all 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.

[38]In order to increase compliance with taxes many municipalities offer financial incentives in the form of discounts or lotteries for those taxpayers that pay their taxes on time. We reviewed the web pages of 30 randomly selected municipalities and found that, at least, 50% of the municipalities offered financial incentives to taxpayers between 2014 and 2016.

interpret the latter as a placebo policy of the experiment. 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.[39]

1.4.4 Sample and baseline balance

We define the sample frame for the supply-of-research experiment as all Brazilian municipalities with populations between 5,000 and 100,000 inhabitants for which the mayor was confirmed to attend the relevant CNM meeting—here the *Novos Gestores* convention in 2016—three days before it began. 1,818 municipalities—45 percent of all mayoral administrations in Brazil within the relevant population range—are part of our sample.

The randomization assigned 881 municipalities to the treatment group and 937 municipalities to the control group.[40] Table 1.10 displays summary characteristics of the sample mayors. We see, for example, that almost 90 percent of the mayors are men; around 60 percent have at least a bachelor degree; and approximately 16 percent are in their second and last term in office.[41] The table also shows that the treatment and control groups are balanced on mayor's characteristics. Age is the only mayor's characteristic that displays a statistically significant (but small) difference in means between the two groups. Control and treatment group mayors are on average 46.76 and 48.08 years old respectively.

Table 1.10 also displays summary characteristics of the municipalities the mayors in the sample run. For example, 6 percent of total municipal revenues come from local taxes in both the treatment and control groups. The treatment and control groups are balanced on municipal characteristics, such as the number of inhabitants; the share of the population that has at least a bachelor degree; the share of municipal public administration employees that has at least a bachelor degree; the poverty rate; the Gini coefficient; and average monthly income per capita and region. The joint hypothesis test reported in the last column of the table fails to reject that the average characteristics of the

[39]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.

[40]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.

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

treatment and control group mayors and municipalities are jointly statistically equal.

As noted above, our main outcome variables were collected through a voluntary phone survey implemented from February to November 2018. Attrition from the *Novos Gestores* conference to the endline survey was low: we interviewed 49.3 percent of the mayors in the treatment group in the endline phone survey, and 51.6 of the mayors in the control group (and 75.8 versus 75.0 percent of chief tax bureacrats). Table 1.11 shows that the correlation between attrition and treatment status is small and statistically indistinguishable from zero. This holds both for mayors, chief tax bureaucrats, and when we pool all policymakers in one regression (column 1). As shown in table 1.12, at endline survey respondents from the treatment and control groups are balanced in the same variables as at baseline. In sum, our randomization was successful at producing statistically similar treatment and control groups at baseline, and the balance remains among the set of policymakers and municipalities that participated in the endline survey.

Invitations to attend the session were sent in an official email from CNM two days before the start of the convention. This was the first time mayors heard about our information session. In addition, in the conference opening day, we sent an email and SMS invitations to the treatment group. 37.9 percent of the mayors in the treatment group chose to attend our session.[42] In addition to the opportunity costs of attending—in the *Novos Gestores* setting, these were arguably sizeable due to multiple sessions running parallel to ours and meetings mayors may have had to forego to attend—this moderately sized proportion may be due in part to more context-specific constraints.[43]

The characteristics of the mayors in the treatment group that chose to attend the research information session and their municipalities are important. Tables 1.13 and 1.14 present results from regressing a dummy for attending on respectively the mayor's and municipal characteristics using a linear probability model.[44] Older mayors—those above the median age of 47—are seven percentage points less likely to attend than younger mayors, while mayors with a bachelor's degree are 15 percentage points more likely to attend than those without one. We do not find any evidence that the mayor's term limit,

[42]Only 6 out of 917 municipalities in the control group attended our session.

[43]First, through the communication platforms we used to send the invites, we realized that many mayors did not have updated contact information stored in the CNM system. Second, CNM gave an official participation certificate to mayors that attended a minimum number of *Novos Gestores* sessions. Our session was the only that did not count towards receiving the certificate.

[44]The difference in the number of observations between the columns focusing on mayor's and municipal characteristics is explained by the fact that three municipalities did not report accounting information to the National Treasury during 2010-2015.

or inequality, income per capita, or the relative importance of local tax revenues in the municipality influence the decision to spend an hour at a research information session.

Overall, the results presented in this section reinforce the take-aways from the demand-for-research experiments—namely that revealed interest in research information is quite high among Brazilian mayors, and not very unevenly distributed across mayors and municipalities of different types.

1.4.5 Results

As is standard in experiments with imperfect compliance, we show both intent-to-treat (ITT) and treatment-on-the-treated (ToT) results, where we instrument receiving research information with being assigned to treatment in the ToT approach.[45] Since our primary interest is in the impact of research information on policy adoption, we present ITT results in the Appendix and focus here on the ToT estimates. We thus run:

$$Y_i = \omega + \beta \widehat{I}_i + \epsilon_i \tag{1-6}$$

Where Y_i represents an outcome of interest in municipality i, and β captures the intervention impact. Standard errors are clustered at the municipality level throughout.

Beliefs. We start by studying policymakers' beliefs about the effectiveness of taxpayer reminder letters. We asked mayors and the bureaucrats in charge of the tax division their beliefs regarding the effectiveness if the policy were to be implemented in their own municipality. Considering that at baseline we were not able to get the priors of a number enough of participants we cannot analyze the belief updating of being exposed to the research information session.[46] Instead, we compare policymakers' belief statement against what was informed in the information session. Mayors were informed that the

[45]We already have shown that our instrument—the assignment to treatment—appears to satisfy the exogeneity condition in that the instrument does not correlate with observable characteristics of the treatment and control groups. Insofar as assignment to treatment predicts actual participation in the research information session, the randomized assignment can thus be used as an instrument for participation. To confirm this, we run $I_i = \alpha + \gamma T_i + \mu_i$, where I_i is a dummy for the mayor i attending the information session, T_i is a dummy for the mayor being assigned to the treatment group, and μ_i is an error term. As shown in Appendix table A.15, assignment to treatment strongly predicts participation in the information session, increasing the likelihood of attendance by 37.3 percent.

[46]The baseline survey was completed by 38.0% of treatment mayors and 29.6% of control mayors. On the other hand, key bureaucrats of the local tax area did not participate in the *Novos Gestores* convention in Brasília.

effect sizes found in the existing research studies were 10%, 12% and 20%, depending on the variants of the policy (i.e. tax payment deadline, risk of audits if not paying taxes on time, and social norms regarding paying taxes). Therefore in table 1.15 we present results from specification 1-6, where Y_i is the absolute difference between the stated belief of individual i and each of the informed effect sizes (panels A, B and C). We interpret this difference as a measure of long-run accuracy or recollection about the research information presented. Besides presenting results for reminder letters, in columns 4-6 we show estimates for the financial incentives policy (i.e. discounts or lotteries for taxpayers that pay taxes on time).[47] In columns 1 and 4 we pool all policymakers in one regression, whereas in the rest of the columns we present estimates for mayors and finance staff separately. We find that providing mayors with information about effect sizes increases the degree of accuracy in regard to the true effect size of reminder letters, 15 to 24 months after the intervention, by about 20 percent relative to the control group (panels A and B). This holds both for mayors, tax bureaucrats, and when we pool all decisionmakers in one equation. Is interesting to note that we do not find any effects on financial incentives, a policy about which research information was not provided.

After requesting their beliefs about effect sizes we asked how confident they were with their answers using a 5 point likert-scale from "not at all confident" to "very confident". To examine this question, in panel D we standardized our measure of confidence to have mean 0 and standard deviation 1. As can be seen, the information session did not affect, at least in the long-run, the reported level of confidence. ITT estimates of panels A, B, C and D are presented in Appendix table A.16.

Policy adoption. The most important question of the phone survey asked policymakers if the taxpayer reminder policy was adopted by the municipality at any time since the date of the information session. Our survey design allows us to unravel not only whether the policy was adopted but also whether the informed components of the policy were adapted to local contexts. In particular, we have information about the type of message sent—tax payment due date, risk of audits, social norm, the channel used to send the reminders—letters, text messages, media advertising, street flyers, and the timing of the reminders—before or after first payment deadline. Thus, we first study the causal impacts of the information session on adoption of taxpayer reminders (i.e. considering any variants, channels and timing of the policy) and then we analyze the impact on the exact components of the policy

[47]Is important to highlight that although during the information session mayors asked questions about the effectiveness of financial incentives, in all cases we stated that there was not enough research evidence to be conclusive about their concerns.

informed to mayors during the information session (i.e. variants—tax payment due date, risk of audits, social norm, channel—letters, timing—before first payment deadline).

Table 1.16 presents the results on taxpayers reminders (see ITT in Appendix table A.17), where the outcome variable is a dummy which takes the value of 1 if the policymaker says the policy was adopted in municipality and 0 otherwise. Again, to increase statistical power we show estimates pooling all heads of government in one regression, and also separating mayors and tax bureaucrats. When pooling policymakers in column 1, we find that supplying mayors with research information about taxpayers reminder letters increases the probability that the municipality adopt the policy by 10 percentage points, or 32 percent relative to the control group mean. We also find similar positive magnitudes for both mayors and finance staff (columns 3 and 5 respectively), although estimates are not significant. As mentioned in the Introduction, the phone survey introduced respondents with a placebo variant message of the policy.[48] Despite not finding evidence that the placebo variant has differential effects between treatment and control municipalities (Appendix table A.18), in even columns we present estimates dropping from the analysis those observations that answered affirmatively to the placebo information component. Results are robust to this alternative sample.

Appendix table A.19 reports estimates on taxpayers reminders but adding to equation 1-6 a set of characteristics of both the mayor and the municipality as controls (ITT estimates are shown in Appendix table A.20). Across all three sets of respondents the estimated coefficients are stable and become more precise.[49] Depending on the sample used, we now see significant effects of the information session on policy adoption individually for mayors and for the key bureaucrats of the municipal tax division.

We next investigate whether the information session have a differential effect on unusual or innovative components of the informed policy. For that, we disentangle the type of message sent, the channel used and the timing of

[48]The placebo variant was: "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988".

[49]In Appendix tables A.21 and A.22 we investigate whether mayor's and municipal characteristics have any differential effects on policy adoption. We interact each characteristic with participation in the information session. Among mayor's characteristics we consider gender, age, education, term limits, and political party. Among municipalities' characteristics we evaluate population size, average education level among adults, average education level among public employees, poverty level, Gini coefficient, average income per capita, the relative importance of local tax revenues in 2010-2015, and region. We expressed all continuous variables as indicators of above/below the median of the sample distribution. We do not find any suggestive evidence that any of these characteristics of the mayor and municipalities increase or decrease the probability of policy adoption after the information session.

the reminders, according to the information provided during the experimental session. We define the dependent variable as a dummy equal to 1 if the respondent says the component 'X' of the policy was adopted in municipality and 0 otherwise, independently of whether the rest of the components were implemented or not. Table 1.17 presents the results of our preferred specification (i.e. pooling all policymakers in one regression and excluding placebo observations). Is particularly interesting to see that we find huge adoption effects on the two most unusual information components of the tax reminders policy (as indicated by the control group mean): including a reminder message highlighting the social norm of paying taxes on time, and using a hard copy letter to communicate the reminder (ITT estimates in Appendix table A.23). The social norm component result is robust to the mayors' sample but not to the bureaucrats' one, whereas the letter component becomes insignificant when we split the sample by policymaker (Appendix tables A.24 and A.25).

Overall, these findings provide novel evidence that the policymakers that have access to academic research translate research findings into policy adoption, underscoring the importance of information transmission.[50] Still, one may worry about whether this indeed identifies policy adoption or is driven by an experimenter demand effect. We conduct two additional analysis to provide further evidence about the robustness of the results. The phone survey asked adoption questions about two other local policies that were not part of the information session: financial incentives to taxpayers and usage of e-procurement in municipalities' public purchases. As the tax reminders policy, financial incentives are also designed to increase tax revenues. Several questions were asked about the effectiveness of financial incentives even though we did not present any research information during our session. We investigate this policy with two purposes. First, to understand whether the exposure to the information session moved tax policies decisions from financial incentives to tax reminders. And second, to have a measure of the experimenter demand effect with a policy that was not the theme of the information session but that was indirectly raised during it, and that has the same goals than tax reminders. We complement this analysis using the usage of e-procurement by the municipality as a strict placebo policy that is under the scope of decisions of the same bureaucrats that answered the tax reminders policy adoption

[50]Whether municipalities successfully increase tax revenues is beyond the scope of this paper. We plan to study this question in subsequent years when tax revenues from 2017-2019 cycles get disclosed from administrative sources. Municipalities may have additional frictions affecting the quality of their implementation, and we plan to use heterogeneity of state capacity, as well as variation in the group of municipalities in the mayors' network that got treated, to answer how such frictions matter for the success and persistence of effective policies in local governments.

questions. Appendix tables A.26 and A.27 show that we do not find any adoption effects on these policies neither pooling all policymakers in the same regression nor separating by heads of governments.[51]

1.5 Conclusion

Today's organization of research activities assumes that policymakers update their beliefs in response to information about new research findings, and change their policies accordingly. But do they? In this paper we leverage a unique collaboration with the National Confederation of Brazilian Municipalities to provide causal evidence on the research-to-policy transmission process.

To understand what types of research findings persuade policymakers and potentially policy take-up, we carried out a series of survey experiments with 900 municipal officials—mayors, vice-mayors, municipal secretaries and council members. The experiments were designed to assess whether local policymakers demand research information, and how being informed about the findings from rigorous studies with varying attributes affects municipal officials' beliefs. We find that municipal officials are willing to pay about USD 36 to learn research findings, and update their beliefs based on the findings provided. As we randomly assigned policy-research attributes to participants, we study how these causally mediate demand for research findings and belief updating. Willingness-to-pay and the degree of belief updating both respond more strongly when the respondent is presented with a large sample size study. Other attributes—like similarity of the location studied to the municipality in question and the degree of partisan support for the policy—appear to matter less.

We then use a large-scale field experiment with 1,818 Brazilian municipalities to investigate whether the provision of policy-effectiveness research findings influences policy decisions. A randomly selected treatment group of 881 mayors was invited to attend a 45 minutes-long research information session. At the treatment session, an experienced presenter informed mayors about a set of rigorous RCTs showing positive effects of a simple-and-cheap-to-implement taxpayer reminder letter policy on tax revenues, and where more information could be found. 15 to 24 months later, we called mayors and key bureaucrats of municipalities' tax divisions to assess policy adoption. We find that providing mayors with information about research findings increase the probability that their municipalities adopt the policy by 32 percent relative to the control group

[51]Results are also robust to the inclusion of controls (tables not reported).

mean, and that the effect of the information provided is larger on adoption of unusual variants of the tax reminders policy.

Our findings suggest that policymakers are interested and are able to act on (new) research information, highlighting the relevance of the research-to-policy transmission process. This implies that program evaluation research can to a considerable extent help political leaders improve their constituents' lives.

Tables

Demand-for-Research Experiment

Table 1.1: Study characteristics

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

Notes: Studies used and its attributes. Jamaica (Grantham-McGregor et al., 1991; Walker et al., 2005), Colombia (Attanasio et al., 2014), Michigan (Barnett, 2011), USA (Puma et al., 2010)

Table 1.2: Summary statistics - Sample

VARIABLES	Mean	SD
Male (Mayor)	87.82	(32.73)
Age (Mayor)	48.04	(10.71)
College or more (Mayor)	60.27	(48.97)
2nd Term (Mayor)	20.09	(40.10)
Leftist Political Party (Mayor)	37.29	(48.39)
Population (2016 - Thousands)	24.59	(55.62)
College Population (2010)	5.14	(2.76)
Public Adm College (2016)	33.34	(13.96)
Poverty (2010)	24.92	(18.14)
Gini (2010)	49.85	(6.44)
Kids in School (0-3, 2010)	19.81	(11.51)
Kids in School (4-5, 2010)	78.34	(15.87)
Big South	53.58	(49.91)
Per Capita Income (2010)	480.69	(237.69)

Notes: Mean and standard deviation of sample participants (at the municipality-level) of the demand-for-research experiment. Male-Leftist political party, are characteristics of the mayor that runs the municipality. College population indicates the share of adults with college degree. Public administration college indicates the share of municipal public employees with college degree. Poverty refers to municipalities' poverty rate. Gini refers to the Gini coefficient of municipality. Kids in school (0-3) indicates the share of children 0-3 years old that attend pre-K education. Kids in school (4-5) indicates the share of children 4-5 years old that attend pre-K education. Big south indicates the share of municipalities from the south, southeast and midwest regions and 0 are north and northeast regions. Per capita income indicates municipality monthly income per capita.

Table 1.3: Study characteristics - Balance

	(1)	(2)
VARIABLES	Developing	Large
Male (Mayor)	-1.3211	-4.0462***
Age (Mayor)	-0.0997	-0.4731
College (Mayor)	2.7315	0.0545
2nd Term (Mayor)	0.3243	-0.1866
Leftist Party (Mayor)	-1.0846	1.7034
Mayor	-0.2295	-1.4094
Prof Politician	0.7786	-0.4208
Leftist Scale	-2.4781	-1.9016
Implemented ECD	0.3214	-3.1046
Heard ECD	-0.7741	-0.0720
Kids in School (0-3)	-1.0415**	0.0754
Kids in School (4-5)	-0.4169	0.1541
Population	1.4364	1.2248
College Population	-7.6688	1.7868
Public Adm College	-0.9649*	-0.8442
Poverty	0.4999	0.4229
Gini	0.4802*	0.4444
Big South	-0.8158	-1.1354
Income Per Capita	-0.0280	-0.0177
Observations	1,371	1,371
Round	1 and 2	1 and 2
Clusters	766	766

Notes: OLS results. Each cell reports the estimated coefficient of a regression of each characteristic on a dummy which is equal to one for Jamaica and Colombia and zero otherwise (Developing), and on a dummy which is equal to one for for Colombia and US and zero otherwise (Large). Robust standard errors are clustered at the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Priors about different locations - Summary statistics

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

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

Table 1.5: Willingness to pay - Study characteristics

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

Notes: OLS results. The dependent variable is willingness to pay, which is elicited in two different rounds. Developing is a dummy which is equal to one for Jamaica and Colombia and zero otherwise. Large is a dummy which is equal to one for Colombia and US and zero otherwise. Mean LHS is the mean WTP on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.6: Value of experimental currency - WTP for gift cards

WTP for	N	Average	SE	95% CI	
25 Reais Gift Card	133	36.80	3.19	30.50	43.10
5 Reais Gift Card	162	30.01	2.67	24.74	35.29
Difference	295	6.78	4.13	-1.33	14.90

Notes: Differences in means between the willingness to pay for *Lojas Americanas'* gift cards of R$ 25 and R$ 5. 1 USD could buy between 3 and 4 R$ during the conferences. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7: Belief updating - Weight placed on study result

	(1)	(2)	(3)	(4)
VARIABLES	Posterior	Posterior	Posterior	Posterior
Prior	0.6850***	0.5906***	0.8066***	0.5512***
	(0.0221)	(0.0295)	(0.0248)	(0.0302)
Signal	0.3190***	0.3729***	0.2476***	0.4166***
	(0.0199)	(0.0261)	(0.0241)	(0.0301)
Observations	1,188	702	486	544
Round	1 and 2	1	2	1
Context	Municipality	Municipality	Municipality	Random Study
Clusters	702	702	486	544
Mean LHS	0.442	0.429	0.461	0.430

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. Context is either the respondent's own municipality (columns 1, 2 and 3) or one of the four possible studies (column 4). Mean LHS is the average posterior belief of the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.8: Belief updating - Weight placed on WTP and study characteristics

VARIABLES	(1) Posterior	(2) Posterior	(3) Posterior	(4) Posterior	(5) Posterior	(6) Posterior	(7) Posterior	(8) Posterior
Prior	0.6091***	0.5059***	0.7423***	0.5986***	0.6834***	0.5901***	0.8042***	0.6518***
	(0.0261)	(0.0344)	(0.0320)	(0.0334)	(0.0221)	(0.0296)	(0.0249)	(0.0281)
Signal	0.3385***	0.4030***	0.2575***	0.3776***	0.2746***	0.3213***	0.2171***	0.3403***
	(0.0225)	(0.0303)	(0.0292)	(0.0328)	(0.0249)	(0.0331)	(0.0299)	(0.0390)
Signal*Developing	0.0145	-0.0010	0.0321	0.0082				
	(0.0226)	(0.0317)	(0.0307)	(0.0353)				
Signal*Large	0.2631***	0.2935***	0.2182***	0.1864***				
	(0.0386)	(0.0551)	(0.0602)	(0.0623)				
Signal*WTP					0.0009**	0.0010**	0.0007	0.0005
					(0.0004)	(0.0005)	(0.0005)	(0.0006)
Observations	1,188	702	486	544	1,188	702	486	544
Round	1 and 2	1	2	1	1 and 2	1	2	1
Context	Municipality	Municipality	Municipality	Random Study	Municipality	Municipality	Municipality	Random Study
Clusters	702	702	486	544	702	702	486	544
Mean LHS	0.442	0.429	0.461	0.442	0.442	0.429	0.461	0.442

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. WTP is the willingness-to-pay for research information. 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. Context is either the respondent's own municipality (columns 1, 2, 3, 5, 6 and 7) or one of the four possible studies (columns 4 and 8). Mean LHS is the average posterior belief of the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9: Effect of partisan message on priors and WTP

VARIABLES	(1) Prior	(2) Prior	(3) WTP	(4) WTP	(5) WTP
Message	-0.0097	0.0373	-1.7860	-1.5547	-1.9143
	(0.0223)	(0.0320)	(4.1797)	(4.2907)	(5.3723)
Leftist	-0.0500	-0.0814**	-6.9875	3.4735	-14.2820*
	(0.0321)	(0.0354)	(6.1908)	(7.0247)	(7.2419)
Message*Leftist	0.0628	0.0416	10.6305	-1.5245	19.3606*
	(0.0479)	(0.0564)	(8.6526)	(9.5921)	(10.1863)
Observations	475	360	881	341	540
Context	Municipality	Random Study	-	-	-
Round	-	-	1 and 2	1	2
Clusters	475	180	341	341	180
Mean LHS	0.376	0.393	49.25	53.52	46.56

Notes: OLS results. In columns 1 and 2, the dependent variable is respondents' priors. In columns 3, 4 and 5, the dependent variable is willingness to pay for studies. 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 for self-identifying leftist (0-4) on a 0-10 scale. Mean LHS is the average of the left-hand side variable of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Supply-of-Research Experiment

Table 1.10: Summary statistics and balance - Full sample

Variables	Control	Treatment	Difference	p-value
Male (Mayor)	88.26	89.67	1.41	0.34
Age (Mayor)	46.76	48.08	1.32***	0.01
College or more (Mayor)	57.74	56.98	-0.76	0.74
2nd Term (Mayor)	15.69	17.25	1.56	0.37
Leftist Political Party (Mayor)	32.98	35.07	2.10	0.35
Population (2016 - Thousands)	20.86	20.80	-0.06	0.94
College Population (2010)	5.17	5.02	-0.15	0.25
Public Adm College (2016)	32.60	33.44	0.84	0.24
Poverty (2010)	26.41	26.14	-0.27	0.76
Gini (2010)	50.33	50.14	-0.19	0.54
Local Tax Revenues (2010-15)	6.06	6.15	0.09	0.68
Big South	51.01	50.40	-0.62	0.79
Per Capita Income (2010)	457.64	461.06	3.42	0.75
Joint F-test				0.18

Notes: Sample means by experimental group and differences in means between groups. Male-Leftist political party, are characteristics of the mayor that runs the municipality. College population indicates the share of adults with college degrees. Public administration college indicates the share of municipal public employees with college degrees. Poverty refers to municipalities' poverty rate. Gini refers to the Gini coefficient of municipality. Local tax revenues (2010-2015) indicates the average share of municipal tax revenues on total municipal revenues from 2010 to 2015. Big south indicates the share of municipalities from the south, southeast and mid-west regions; and 0 are north and northeast regions. Per capita income indicates municipality monthly income per capita. p-value of mean-comparison t-tests between groups for full sample, and the joint significance F-test p-value. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.11: Probability of answering phone survey

VARIABLES	(1) Surveyed	(2) Surveyed	(3) Surveyed
Treatment Assignment	-0.0074	-0.0228	0.0080
	(0.0177)	(0.0235)	(0.0202)
Observations	3,636	1,818	1,818
Respondent	All	Mayor	Finance Staff
Clusters (Municipalities)	1818	1818	1818
Mean Control	0.633	0.517	0.750

Notes: OLS estimation results. The dependent variable is a dummy which takes the value of 1 if mayor (finance staff) was interviewed. Treatment assignment is a dummy which takes the value of 1 if the municipality's mayor was assigned to the treatment group. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.12: Summary statistics and balance - Survey respondents

Variables	Control	Treatment	Difference	p-value
Male (Mayor)	90.01	89.88	-0.14	0.93
Age (Mayor)	47.08	48.69	1.61***	0.00
College or more (Mayor)	57.66	58.39	0.73	0.78
2nd Term (Mayor)	15.18	16.09	0.91	0.63
Leftist Political Party (Mayor)	32.76	34.12	1.36	0.58
Population (Thousands - 2016)	20.23	20.28	0.06	0.95
College Population (2010)	5.47	5.33	-0.14	0.31
Public Adm College (2016)	33.51	33.50	-0.01	0.99
Poverty (2010)	23.05	23.15	0.11	0.91
Gini (2010)	49.37	49.54	0.17	0.61
Local Tax Revenues (2010-15)	6.40	6.48	0.08	0.75
Big South	59.92	57.56	-2.36	0.36
Per Capita Income (2010)	489.23	492.01	2.78	0.81
Joint F-test				0.17

Notes: Sample mean by experimental group and differences in means between groups, among phone survey respondents. Male-Leftist political party, are characteristics of the mayor that runs the municipality. College population indicates the share of adults with college degrees. Public administration college indicates the share of municipal public employees with college degree. Poverty refers to municipalities' poverty rate. Gini refers to the Gini coefficient of municipality. Local tax revenues (2010-2015) indicates the average share of municipal tax revenues on total municipal revenues from 2010 to 2015. Big south indicates the share of municipalities from the south, southeast and mid-west regions; and 0 are north and northeast regions. Per capita income indicates municipality monthly income per capita. p-value of mean-comparison t-tests between groups for full sample, and the joint significance F-test p-value. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.13: Individual predictors of session participation

VARIABLES	(1) Information Session	(2) Information Session
Male (Mayor)	0.0168	-0.0013
	(0.0546)	(0.0559)
Age (Mayor)	-0.0713**	-0.0776**
	(0.0327)	(0.0335)
College or more (Mayor)	0.1551***	0.1500***
	(0.0327)	(0.0333)
2nd Term (Mayor)	-0.0051	-0.0005
	(0.0441)	(0.0449)
Political Party Leftist (Mayor)	0.0327	0.0402
	(0.0345)	(0.0350)
Constant	0.3041***	0.3532***
	(0.0619)	(0.1124)
Observations	881	878
Municipal Characteristics	No	Yes
R-Squared	0.0335	0.0416

Notes: Linear probability results. Response variable is information session participation and takes the value of 1 for mayors that attended the information session and 0 otherwise. The individual characteristics included in the model are: Male (1/0); Age above-below median (1/0); College or more (1/0); 2nd Term (1/0) and Political party leftist (1/0, mayors belonging to a center-leftist party according to historical political platforms). The municipal 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); Monthly income per capita above-below median (1/0); Local tax revenues share 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). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.14: Municipal predictors of session participation

VARIABLES	(1) Information Session	(2) Information Session
Population (2016)	-0.0079	-0.0164
	(0.0343)	(0.0339)
College Population (2010)	0.0634	0.0421
	(0.0466)	(0.0458)
Public Adm College (2016)	-0.0345	-0.0372
	(0.0339)	(0.0335)
Poverty (2010)	-0.1015	-0.0739
	(0.0903)	(0.0928)
Gini (2010)	0.0449	0.0413
	(0.0382)	(0.0379)
Per Capita Income (2010)	-0.0762	-0.0642
	(0.0839)	(0.0855)
Local Tax Revenues (2010-2015)	-0.0245	-0.0119
	(0.0459)	(0.0451)
Big South	0.0258	0.0597
	(0.0662)	(0.0661)
Constant	0.4343***	0.3532***
	(0.0937)	(0.1124)
Observations	878	878
Individual Characteristics	No	Yes
R-Squared	0.0084	0.0416

Notes: Linear probability results. Response variable is information session participation and takes the value of 1 for mayors that attended the information session and 0 otherwise. The municipal 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); Monthly income per capita above-below median (1/0); Local tax revenues share 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). The individual characteristics included in the model are: Male (1/0); Age above-below median (1/0); College or more (1/0); 2nd Term (1/0) and Political party leftist (1/0, mayors belonging to a center-leftist party according to historical political platforms). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.15: Beliefs and confidence

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	| Belief-10% |	| Belief-10% |	| Belief-10% |	| Belief-10% |	| Belief-10% |	| Belief-10% |
Information Session	-1.5474**	-1.6763*	-1.4551*	0.1817	-0.6224	0.6672
	(0.6336)	(0.9244)	(0.8038)	(0.6436)	(0.9558)	(0.8168)
Mean Control	6.775	6.824	6.742	7.172	6.857	7.382
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	| Belief-12% |	| Belief-12% |	| Belief-12% |	| Belief-12% |	| Belief-12% |	| Belief-12% |
Information Session	-1.3267**	-1.2743*	-1.3767**	0.3159	-0.2013	0.6173
	(0.5218)	(0.7371)	(0.6856)	(0.5350)	(0.7847)	(0.6892)
Mean Control	6.968	6.859	7.041	7.006	6.719	7.198
Panel C	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	| Belief-20% |	| Belief-20% |	| Belief-20% |	| Belief-20% |	| Belief-20% |	| Belief-20% |
Information Session	-0.6244	-0.1925	-1.0134	0.7382	0.9761	0.5771
	(0.6956)	(0.9917)	(0.9320)	(0.7440)	(1.0589)	(0.9611)
Mean Control	9.122	8.389	9.616	7.904	7.860	7.933
Panel D	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Confidence	Confidence	Confidence	Confidence	Confidence	Confidence
Information Session	0.0916	0.1202	0.0906	0.0080	-0.2897	0.2191
	(0.1085)	(0.1722)	(0.1385)	(0.1135)	(0.1766)	(0.1429)
Mean Control	-0.0171	0.109	-0.102	-0.00291	0.126	-0.0894
Observations	2,186	860	1,326	2,156	845	1,311
Respondent	All	Mayor	Finance Staff	All	Mayor	Finance Staff
Policy	Reminder Letters	Reminder Letters	Reminder Letters	Financial Incentives	Financial Incentives	Financial Incentives
Clusters (Municipalities)	1440	860	1326	1432	845	1311

Notes: 2SLS estimation results. In panels A, B and C, the dependent variable is the absolute difference between self-reported beliefs about effect sizes of policy on local tax revenues, and the informed effect size of the reminder letters policy during the information session. In panel D, the dependent variable is self-reported confidence level about beliefs in a likert-scale of 1 to 5 (standardized to mean 0 and standard deviation 1). Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminder letters. This last variable is instrumented with treatment assignment. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.16: Policy adoption - Reminder letters

VARIABLES	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted	(6) Adopted
Information Session	0.1031*	0.0934*	0.1125	0.1347	0.1028	0.0733
	(0.0531)	(0.0551)	(0.0791)	(0.0870)	(0.0656)	(0.0667)
Observations	2,271	2,055	913	785	1,358	1,270
Respondent	All	All	Mayor	Mayor	Finance Staff	Finance Staff
Placebo Included	Yes	No	Yes	No	Yes	No
Clusters (Municipalities)	1465	1413	913	785	1358	1270
Mean Control	0.317	0.298	0.367	0.342	0.283	0.270

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality, and 0 otherwise. Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.17: Policy adoption - Reminder letters information components

VARIABLES	(1) On Time	(2) Audit	(3) Social Norm	(4) Before Due	(5) Letter
Information Session	0.0899	0.0735	0.1110***	0.0643	0.0715*
	(0.0548)	(0.0484)	(0.0372)	(0.0533)	(0.0427)
Observations	2,055	2,055	2,055	2,055	2,055
Respondent	All	All	All	All	All
Placebo Included	No	No	No	No	No
Clusters (Municipalities)	1413	1413	1413	1413	1413
Mean Control	0.294	0.198	0.0944	0.275	0.142

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the information component of the policy was adopted in municipality, and 0 otherwise. On Time refers to a reminder message highlighting the tax payment deadline. Audit refers to a reminder message highlighting the risks of audits for not paying taxes on time. Social Norm refers to a reminder message highlighting the social norm of paying taxes. Before due refers to sending the reminder message before taxes' due date. Letter refers to sending the reminder message using a hard copy letter. Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

2
Revenue Shocks and Fiscal Capacity: Evidence from Brazil

2.1
Introduction

The capacity to tax is one of the key drivers of the development process. Yet, the ability to collect taxes varies substantially across countries. Today's developed nations collect larger shares of taxes relative to GDP (the tax take) compared to low-income countries. Part of this variation can be explained by political incentives and the institutions required for a well functioning fiscal system (see Besley and Persson, 2013 for a review of stylized facts). These institutions have been developed and improved in high-income countries throughout the development process. For example, modern tax administrations use computer based technologies to keep track of the tax base and rely on third party reported information such as from firms or banks. Furthermore, they generally have a set of mechanisms to assure compliance and to monitor tax payments. Those institutions guarantee efficient tax collection and allow countries to extract a relatively large proportion of GDP as tax revenues. For example, the OECD average of taxes relative to GDP is 34.3%, while it is only 22.8% for Latin American countries.[1] This pattern has been documented in cross-country studies, but empirical micro evidence about the mechanisms which trigger investment in fiscal capacity which enables countries to collect more tax revenues is still scarce.

Early contributions explain investment in fiscal capacity by the need to extract further revenues during wartime (Tilly, 1985). More recent studies focus on other factors that affect the incentives for governments to invest in fiscal capacity such as political turnover, the cohesiveness of society and structural changes. Persson and Svensson (1989) argue that political instability can lead incumbent politicians to undervalue the future benefits of investing in fiscal capacity. At the same time, in unstable societies, politicians may have incentives not to invest in fiscal institutions because they might be used by rival groups which might take over the government in the future (Alesina and Perotti, 1999). Other contributions argue that the type of taxes (such as import

[1] See OECD/ECLAC/CIAT/IDB (2017) for an overview.

or export duties or income and consumption taxes) that are suitable for a specific country change with the process of economic development (Kleven et al., 2009; Gordon and Li, 2009). Gillitzer (2017) motivates the role of negative income shocks which can induce a government to invest in fiscal capacity to keep revenues stable. If existing tax bases shrink due to economic contractions, governments extend tax bases to other sources of income such as sales taxes.

In many developing countries, however, negative income shocks do not only occur because of negative shocks to domestic tax bases, but also because of aid reductions from donor countries. Besley and Persson (2013) highlight the role of non-tax revenues for investment in fiscal capacity to increase actual revenues from taxes or other revenue sources under the governments discretion. They show a negative correlation between the tax take and development aid received. However, they acknowledge that the direction of causality remains unclear: transfers, such as development aid or intergovernmental grants, are endogenous to fiscal capacity building.

This paper contributes to this debate. More specifically, we exploit the specific federal setting in Brazil to get a causal estimate of this relationship in an emerging economy. We combine the literature on fiscal capacity with the literature analyzing public finances in federal systems. Brazilian municipalities receive a large proportion of their revenues from a formula grant assigned by the central government. The specific design of the allocation formula allows us to investigate the adjustment policies enacted by local governments after a positive or negative revenue shock which exogenously hits the jurisdiction. These shocks happen when census outcomes are used to update population figures, which determine the level of grants. The amount received changes discontinuously at various population thresholds. Our identification follows Serrato and Wingender (2016) and uses the update of the population registered by the census as a source of exogenous variation on federal grants. Whether or not a municipality gets reshuffled around the threshold is exogenous as the municipality has no information by how much previous population figures will be corrected.

While different authors have already estimated the effects of intergovernmental transfers in developed countries (Knight, 2002; Gordon, 2004; Dahlberg et al., 2008; Lundqvist et al., 2014; Cascio et al., 2013 and Litschig and Morrison, 2013), to the best of our knowledge little has been done in the context of emerging economies. It is important to distinguish between those two groups of countries. Fiscal institutions in developed countries, relative to those in low-income countries, are already further advanced. In this case, the only margin

at which a reaction of local governments is possible is changing tax rates or local expenditures. In middle and low-income countries, however, institutions and infrastructure related to tax collection are less developed. For example, all European countries charging property taxes usually use a computer based cadastre which covers all (legal) properties. Therefore, the tax administration is already advanced to a well-functioning level and further investment into fiscal capacity would not change much at the margin. This is different for developing and emerging economies. In 2006, before the shock we analyze occurred, 47% of Brazilian municipalities still had not implemented an IT based system for all three main local taxes. Around 16% did not have a digital cadastre which is essential to administer efficiently one of the most important local taxes. Around 10% implemented such a register until 2015, which is one way to improve fiscal capacity.[2] Another dimension that clearly reflects the difference in the degree of development of the tax collection structure between emerging and developed countries is the size of the tax administration. According to data extracted from the OECD's Tax Administration Comparative Series 2017, Brazil has 1.1 federal tax employees per 10,000 inhabitants while this number is 5.7 for OECD's countries.[3] Our data shows that around half of Brazilian municipalities do not employ specialized workers related to tax collection and auditing, while the average is of about 2 tax workers per municipality.

Hence, instead of changing tax rates themselves, there is still room to improve the tax base, tax compliance, and the tax administration by investing in fiscal capacity. This is reflected in the share of taxes collected by municipalities. Relative to GDP, the average municipality in our sample collects 9.7% of taxes, but there is huge variation in this figure ranging from almost zero up to more than 50%.

This paper contributes to the body of research on what mechanisms trigger investment in fiscal capacity. In particular, our findings add causal evidence to the literature studying the role of non-tax revenues (see Besley and Persson, 2013). Moreover, despite we are unable to rule out changes in tax rates due to data limitations, our empirical strategy allows us to estimate asymmetric budgetary reactions to positive and negative non-tax revenues shocks and its relation to fiscal capacity investment.

We begin by establishing the relationship between population updates

[2]The Inter American Development Bank, for example, defines one of their program objectives for Brazil as "addressing subnational government fiscal management challenges by promoting the reduction of institutional disparities between the country's tax administrations; cooperation and integration among tax administrations [...] and improvement of tax education and citizenship programs." See IADB (2015).

[3]This database contains data provided by federal tax administrations in response to a tax administration survey implemented in 2016.

and the grant allocation. We find a stable impact of population updates on the grants received by municipalities after the shock but no trends prior to the census. Thereafter, we exploit this mechanism to estimate the causal effect of non-tax revenue shocks on budgetary adjustments. For positive shocks, the increase of revenues translates in an almost identical increase of expenditures. Most interestingly, the adjustment is very different for negative shocks. On average, the exogenous reduction in financial resources causes only a small reduction of expenditures. We find that part of the shock is absorbed by an increase in tax revenues and demonstrate that this effect is related to the level of education of the mayor. Municipalities administered by mayors with low levels of human capital do not invest in fiscal capacity and cut expenditures as a reaction to the shortfall of transfer revenues. On the other hand, local jurisdictions ruled by mayors with a high level of human capital are able to compensate part of the loss. The mechanism behind the increase of tax revenues for the group of municipalities with high-educated mayors is hiring tax auditors and other tax related workers. Overall, our results suggest that the quality of public administration leaders is key for development policies and must be taken into account, for instance, when donor countries decide to increase or decrease aid.

The rest of the paper is organized as follows. Section 2.2 discusses the institutional background and provides some stylized facts. Section 2.3 presents our data and explains our identification strategy. Results are discussed in Section 2.4, and Section 2.5 discusses the mechanisms behind them. We conclude in Section 2.6.

2.2 Institutional details

Brazil is one of the most decentralized countries in the world. It has 5,570 municipalities, which are managed by an elected mayor and an elected city council, divided into 26 states. Brazilian local governments are in charge of providing a significant portion of services and public goods related to education, infrastructure and health. Municipalities' main sources of revenues are intergovernmental transfers from the states and the federal government, representing on average 60% of total local revenues, while local taxes have a minor role in municipal budgets, representing on average less than 15% of total local revenues. The most important budgetary item of Brazilian municipalities is a constitutional federal transfer called *Fundo de Participação dos Municípios* (FPM), which represents on average 22% of total municipal revenues. We exploit the variation in this transfer in our empirical setting.

The FPM is a federal formula transfer that is financed with a fraction of the total revenues of two federal taxes: the income tax and the industrialized products tax.[4] Each year FPM funds are allocated to municipalities according to a predetermined mechanism based on local population estimates and the amount of resources allocated to the state in which the municipality is located. First, the FPM allocation mechanism establishes a fixed share (unchanged since 1991) of the total FPM funds to be assigned to each Brazilian state depending on states' total population and income per capita. Second, 18 population brackets are defined with an associated coefficient that varies non-linearly between 0.6 and 4, with smaller population brackets corresponding to lower coefficients. Then, each municipality is annually assigned to a population bracket depending on its number of inhabitants. This number is defined by the federal agency *Instituto Brasileiro de Geografia e Estatística* (IBGE) using population counting. However, considering that Brazilian censuses are conducted every 10 years, in the inter-censuses period, the IBGE constructs local population estimates taking into account past censuses statistics about birth, mortality and immigration rates. Once the population estimates are produced, the IBGE sends the estimates to the *Tribunal de Contas da União* (TCU), which is the federal agency in charge of determining the population bracket and, therefore, the associated coefficient that each municipality will have. As a general rule the TCU announces the FPM coefficients for all municipalities by November of year *t-1*, municipalities vote their budget by December of year *t-1*, and the FPM funds are transferred to municipalities along the year *t*. Table 2.1 describes the population brackets, the coefficients associated and the percentage variation of the associated coefficients between consecutive brackets.

Combining the local population estimates with the share of total funds that each state has, the allocation mechanism can be defined as:

$$FPM_{i,t}^{k} = \frac{FPM_{k,t} \times \lambda_{i,t}}{\sum_{i \epsilon k} \lambda_{i,t}}$$

In this formula, $FPM_{i,t}^{k}$ is the amount of FPM transfers received by municipality i in year t in state k; $FPM_{k,t}$ is the total amount of resources allocated to state k in year t; and $\lambda_{i,t}$ is the FPM coefficient of municipality i. The formula above highlights an interesting characteristic of the allocation mechanism. Municipalities in the same population bracket receive the exact same amount of FPM transfers, independently of the precise number of inhabitants if they belong to the same state.

[4]The fraction varied greatly through the years. From 10% in 1965 to 24.5% nowadays (STN, 2018).

Since 1992, a national law establishes that the IBGE has to conduct a population counting in the inter-census period in order to update the population estimates more frequently. This has become necessary as large fluctuations which were not predicted by the forecasts became quite usual. These population censuses are conducted every 5 years but, for organizational and/or financial reasons, can be administered at any point in the mid-decade years.[5] Considering that the number of inhabitants that the TCU uses to define the population bracket of each municipality is updated according to the population census as well, the timing of its implementation can have huge impacts on municipalities' finances. Due to the last population counting in the inter-census period held in 2007 (the census we exploit for identification), 403 municipalities were switched to a higher population bracket while 443 municipalities changed to a lower bracket. For comparison, in non-census years these numbers are approximately 170 (positive change) and 18 (negative change). These bracket changes implied an increase (decrease) in FPM transfers per capita in 2008. The increase (decrease) was on average 40.8% (6.2%) for municipalities that crossed to a higher (lower) population bracket, while municipalities that remained in the same bracket increase their FPM transfers per capita by 13.9%.[6]

The 2007 population counting was implemented by the IBGE in all municipalities with less than 170,000 inhabitants plus 21 municipalities above that threshold, comprising in total 97% of Brazilian municipalities. All households within those municipalities were surveyed having as a reference date March 31 to April 1 of 2007. The cut in 170,000 inhabitants was decided considering that municipalities above that number are distant from the lower-bound of the last FPM population bracket and, therefore, were not going to be affected by a new population figure. In those remaining municipalities the IBGE constructed population estimates.

2.3 Identification and data

[5]For instance, the population counting planned for 2016 was not held due to budgetary restraints, and in the 90's was implemented in 1996.

[6]In September of 2007 a Constitutional Amendment was approved which allocated to the FPM total resources a further 1% of the total revenues from the income tax and the industrialized products tax. The amendment came into effect in 2008, what explains in part the increase in FPM transfers per capita experimented by municipalities that remained in the same population bracket.

2.3.1 Model specification

A common identification approach in similar settings is to exploit the bracket cut-offs of the allocation formula (see Corbi et al., 2014; Brollo et al., 2013 for examples in the Brazilian context). Given that the transfer is a non-linear function of population, variation in local population figures which shift a municipality across a threshold can be used for identification if population were exogenous. There is, however, growing evidence that local population responds to the incentives generated by the formula transfer and that the distribution of population might not be smooth at the bracket cut-offs.[7] Furthermore, a regression discontinuity design (RDD) with fixed effects identifies the effect from those moving across the threshold, but would estimate the average effect between those moving to the right and those to the left. While the focus on those moving across the threshold is in line with our idea to investigate local jurisdictions which experience a budgetary shock (rather than comparing a difference in levels), we aim to identify positive and negative shocks separately as the adjustment to them might be substantially different from each other.

For this reason, our identification strategy follows the idea of Serrato and Wingender (2016) and uses the outcome of the 2007 census which is used to update the 2008 local population figures as exogenous variation. We identify budgetary reactions to the wind- or shortfall of formula transfer generated by the update of local population figures. Treatment T is assigned to municipalities which crossed a threshold after the update of local population due to the population counting in 2007, and therefore received a different level of the transfer in 2008. This is exogenous even in the case that the local administration was aware of any kind of manipulation as long as they can not anticipate that the magnitude of the correction is sufficient to shift the municipality across a threshold (we discuss this assumption in Section 2.3.3). Note that this setting allows us to define two different treatment groups, one for those which cross a threshold upwards and receive more transfers, and another for jurisdictions which have been adjusted downwards and receive less transfers. This is an important feature which allows us to estimate potential asymmetric reactions to positive and negative shocks, which is not feasible in a standard RDD setting. Municipalities which never crossed a threshold after the

[7] Litschig (2012) provides evidence for deliberate manipulation of population estimates in Brazil. Eggers et al. (2015) provide a general discussion of using population thresholds for identification and provide evidence for manipulated figures in 4 European countries. Foremny et al. (2017) show that Spanish municipalities manipulate their population figures. By comparing census and local register data, they show that the main mechanism is the incorrect treatment of immigrants.

previous unanticipated local population update are used as the control group. Considering that the results from the population census of 2000 were used to update population figures in 2002, control municipalities are those which never crossed a threshold since that year.

We implement a difference-in-difference event-study design to illustrate the treatment effect over time (2005-2012):

$$ln(Y)_{i,t} = T_i \left[\sum_{p=-2}^{-1} \pi_y \mathbf{1}(t - t^* = p) + \sum_{p=1}^{5} \beta_y \mathbf{1}(t - t^* = p) \right] + \omega_{i,t} + \gamma_t + \delta_i + \mu_{i,t} \tag{2-1}$$

Where $\mathbf{1}(t - t^* = p)$ are indicator variables relating to the time since the census was conducted in $t^* = 2007$. As such, π_y show the evolution of various budgetary outcomes Y prior to the shock (2005-2007) and the β_y show the evolution after the shock from 2008 to 2012. Coefficients are normalized to the omitted year 2007, which is the last one before the shock became effective. $\omega_{i,t}$ is a set of dummy variables that controls for municipalities that shifted positively or negatively in the pre-treatment years.[8] Year fixed effects γ_t control for national developments that could affect outcome variables, such as federal tax policies and the business cycle. Municipal fixed effects δ_i account for time-invariant factors that determine political and economic conditions at the local level. This specification allows us to observe any direct adjustments to local budgets and to compare trends before and after treatment occurs. We provide results from this reduced form event study model for all outcomes. We complement all results with the estimation of an average treatment effect in a standard difference-in-difference setting, i.e. the average effect over the years 2008-2012.

In addition to the reduced form estimates we estimate the elasticity of a budgetary outcome Y with respect to the transfer. This measure can be estimated by a simple log-log specification in the form of $\ln(Y)_{i,t} = \epsilon \ln(fpm)_{i,t} + \mu_{i,t}$. To deal with the potential endogeneity of transfers, we apply an instrumental variables approach and use our treatment variable as

[8] We include dummy variables of whether a municipality jumped positively or negatively in the pre-treatment period (2005-2007), interacted with dummy variables of whether the municipality jumped positively or negatively in 2002-2004 (where 2002 was the last population update based on population census before 2008). The dummy variables are constructed as *dummyposyear* and *dummynegyear*, one for each year 2005-2007, and is equal to 1 from the year of the jump. For instance, if municipality jumped positively in 2005 and negatively in 2007: *dummyneg*2005, *dummypos*2006, *dummyneg*2006 and *dummypos*2007 are 0 for the entire period 2005-2012; *dummypos*2005 is 1 from 2005 onwards (0 before) and *dummyneg*2007 is 1 from 2007 onwards (0 before). This allows us to control for jumps across population thresholds before the treatment took place.

described above as an instrument in the first stage regression:

$$\ln(fpm)_{i,t} = \alpha\, T_{i,t} + \omega_{i,t} + \gamma_t + \delta_i + \mu_{i,t} \tag{2-2}$$

The second stage uses the difference-in-difference first stage to estimate the impact of transfer-cuts or windfalls on budgetary outcomes Y:

$$\ln(Y)_{i,t} = \epsilon \ln(\widehat{fpm})_{i,t} + \omega_{i,t} + \gamma_t + \delta_i + \varepsilon_{i,t} \tag{2-3}$$

Given the log-log specification, ϵ can directly be interpreted as the elasticity of budgetary outcomes with respect to transfers. We complement all results with an aggregate elasticity obtained by this approach in Appendix B.4, and show annual coefficients for comparison.

2.3.2 Data and sample

All municipalities which crossed a population threshold due to the population counting of 2007 are assigned to treatment. We define two treatments according to the sign of the transfer shock. Those municipalities that were reshuffled positively (crossed to a higher population bracket in 2008) constitute the positive shock treatment group, while those municipalities that were reshuffled negatively (crossed to a lower population bracket in 2008) constitute the negative shock treatment group. We hold the set of municipalities fixed for the entire period (2005-2012) independently of whether they crossed a threshold in the pre-treatment years.[9] At the same time, we keep all municipalities at treatment in the post period, even though some of them might have shifted backwards. We follow this intention to treat approach because investment in fiscal capacity after the shock is still going to pay off, even though the effect of an increase or decrease of grants vanishes. Municipalities which never crossed a threshold between 2003-2012 (since the previous unanticipated population update) are used as the control group.

We exclude three groups of municipalities from the analysis: i) state capitals, considering that are subject to a different formula of the FPM transfer; ii) municipalities below 3,972 inhabitants and above 179,060 inhabitants, considering that within this population range are the municipalities that ever

[9]In Appendix B.5.2 we use a different approach that allows treatment municipalities to enter the study in a dynamic fashion. We define an alternative sample in which treatment municipalities enter the study in the pre-treatment year after they have crossed any threshold for the last time. Results are generally robust to this alternative sample.

crossed a threshold; and iii) municipalities with missing values in the main regressor of interest and outcome variables.[10]

2.3.2.1 Budgetary data and population figures

Our dataset combines data from various sources for the years 2005-2012. This period allows us to control for pre-trends 3 years before treatment and observe outcomes for 5 years after treatment. Population estimates that determine transfers are taken from the IBGE and TCU. FPM transfers received by each municipality are retrieved from the National Treasury. Data on municipal public finances are available from the *Controladoria Geral da Uniãa* (CGU) and from *Finanças do Brasil* database (FINBRA). FINBRA is a dataset containing self-reported information about local budgets compiled by the National Treasury. It contains yearly accounting records from all Brazilian municipalities and includes disaggregated data on revenues and expenditures.

Table 2.2 reports statistics for municipal population and public finances, grouped by population brackets. Almost 90% of Brazilian municipalities are in the first eight population brackets, which include cities with less than 50,000 inhabitants. This group is highly dependent on the resources coming from other levels of government. The federal transfer FPM is clearly the most important source of local revenues, accounting on average for more than 35%, follow by state transfers and other federal transfers, representing on average 24% and 13%, respectively. Unlike large municipalities (9-18 brackets), in which tax revenues represent on average up to 20% of local revenues, in the big majority of Brazilian municipalities local tax revenues only represent a small fraction of total revenues (10% or less).

Table 2.3 presents the summary statistics for the budgetary variables of the sample that we use in the main analysis. Note that expenditures and revenues are on average very similar as municipalities do not have access to borrowing. Furthermore, the break-down of revenues highlights the importance of the FPM formula-transfer, which is almost a quarter of total revenues. While taxes represent a much smaller share of total revenues, we observe substantial variation of this variable. The standard deviation is of about three times the mean. This indicates that the degree to which municipalities rely on own tax revenues varies substantially across them. A similar pattern, but at lower magnitudes, can be observed for discretionary grants from the federal

[10]In this way 32% of Brazilian municipalities are excluded from the analysis (0.5% are state capitals; 19% are municipalities outside the aforementioned population range and 13% are municipalities with missing values).

government. General descriptive statistics for both mayors' and municipalities' characteristics are presented in Appendix B.1.

To explore potential effects of the FPM shock on municipalities' tax related operational structure we use mainly two datasets: the *Pesquisa de Informações Básicas Municipais* (MUNIC) and the *Relação Anual de Informações Sociais* (RAIS). MUNIC is produced by the IBGE and contains information on the structure, technology and operation of municipal public institutions. Despite the richness of the data, one issue for our analysis is that the entire set of variables are not available annually. On the other hand, RAIS is an yearly administrative dataset compiled by the Ministry of Labor that includes labor market contractual information and it covers roughly all formally employed workers from both the private sector and public sector. We aggregate employment information at the municipal level considering only local government employees.

2.3.2.2 Discussion of the FPM data

Despite the clear rules in the allocation of the FPM funds and the exogeneity of the population updates to the municipalities, there were cases of (temporary) mis-assignment of funds during the period under analysis. Through court disputes some municipalities were able to maintain FPM coefficients which were not consistent with their population.[11] On the other hand, throughout the 1990s some municipalities were created by the division of existing ones, and to prevent negative impacts on municipalities' finances the federal government passed a law establishing a transition period in the FPM coefficients of the affected municipalities. Clearly, this temporarily mis-assignments of funds were not totally random. To overcome this potential issue we use in the instrumental variables analysis the FPM transfers that each municipality in each state should have received according to its number of inhabitants. We called this variable "theoretical FPM transfers". Theoretical or law-implied FPM transfers have also been used by Brollo et al. (2013);

[11]For instance, the municipality of Barcelos, from the state of Amazonas, filed a judicial complaint in 2008 after its population got reduced by more than 9,000 inhabitants from 2007 to 2008, according to official statistics. This population decline implied a reduction of the FPM coefficient from 1.6 to 1.4. Nevertheless, through several court disputes at different judicial levels, the municipality was able to maintain its 2007 FPM coefficient up to February of 2018. Different was the case of Ribeirão do Pinhal, from the state of Paraná. After a reduction of 82 inhabitants its FPM coefficient was reduced from 1.0 to 0.8 in 2010. The municipality presented a formal judicial complaint but was only able to maintain the coefficient of 1.0 for 9 months. Is important to mention, however, that successful judicial complaints, from municipalities standpoint, are not the rule. The TCU in its website list approximately 20 cases per year in which FPM coefficients are temporally changed by judicial decisions.

Corbi et al. (2014); Gadenne (2017) in their empirical strategies to exploit the allocation mechanism of federal transfers to Brazilian municipal governments.[12]

In figure 2.1 we plot actual and theoretical FPM transfers against population over the period 2005-2012, respectively. Vertical red lines represent the bracket cut-offs of the FPM allocation formula. Purple dots represent FPM transfers averaged over population bins of 500 inhabitants, and in blue lines we depict the smoothed mean of transfers for each population bracket. Note that both figures exhibit clear jumps in each population threshold, with visible variability within brackets due to the different shares of FPM funds received by each state and because FPM funds have grown over time.[13] Observe, however, that jumps are sharper and variability lower in figure (b), which displays theoretical FPM transfers.

2.3.3 Identification validity

The validity of our identification strategy depends on whether the population update was indeed exogenous to municipalities. In other words, the update should not be anticipated by the local administration. Even if we would be able to detect manipulation, our identification would still be valid as long as the manipulation occurs randomly and is not systematically assigned to specific municipalities. The identification would be invalid if time-invariant characteristics of certain municipalities drive measurement errors in population figures (which have to be corrected by the census) and at the same time might be determinants of budgetary outcomes (Serrato and Wingender, 2016). For example, certain local characteristics could systematically attract individuals, which yields larger measurement errors as long as population projections do not properly account for internal migration. If those individuals are high skilled workers, the tax base might increase and drive revenues upwards. If they are low skilled, expenditures for social assistance might increase instead. As a first test, we follow Serrato and Wingender (2016). If those time-invariant features were to drive the census shock, then the population updates of two consecutive shocks should be serially correlated. Figure 2.2 shows a scatter plot of the shock in 2008 versus the previous one in 2002. The flat slope confirms that there is

[12] In table B.2 we show that there exists a strong link between actual and theoretical FPM transfers—although not perfect—as allocation rules are not fully enforced as explained in the text.

[13] Brazilian GDP per capita grew 3% annually over the period 2005-2012, and in 2008 total FPM resources increased through a federal amendment. As explained before, FPM funds are financed with a fraction of two federal taxes: income tax and the industrialized products tax; and the FPM allocation mechanism establishes a share of funds to be assigned to each Brazilian state depending on states' total population and income per capita.

no serial correlation between the two shocks, and that the measurement error is the source of variation which we exploit for identification.

To provide further evidence for the validity of our identification we run a linear probability model with the treatment status as an outcome on a large set of observables $\mathbf{Z}$. We use the same sample as in our main analysis and differentiate between the two types of treatment. Given the cross-sectional nature of this exercise, we include population bracket by state fixed effects ($\lambda_i \times \sigma_s$) and cluster standard errors at the same level.[14] Results from this estimation, $T_i = \beta \mathbf{Z_i} + \lambda_i \times \sigma_s + \varepsilon_{i,t}$, are shown in table 2.4.

We show correlates with three different blocks of variables for the positive and negative shock, respectively. The first model for both types of shocks (columns 1 and 6) only includes state by bracket fixed effects. Models (2)-(4) and (6)-(8) introduce one of the blocks of variables per estimation and models (5) and (10) include all variables simultaneously. The first set of variables are related to the local population and are expected to be significant as they mechanically determine to some extent the treatment status. We find that a larger pre-census population decreased the probability of having a positive shock, a positive population growth between 2003 and 2006 increases the probability of having a negative shock (which might correct some artificial growth or previous manipulation), and that municipalities closer to a threshold are more likely to move upwards to a higher transfer category. All this variables indicate that the measurement error of the population projections correlate with the shock. While this is expected, more important for our purpose is that the second block of variables which includes political determinants does not show strong significant correlations. Model (2) shows that aligned mayors at the state level are to some extend more likely to expect a positive shock. The effect, however, is unstable across specifications and disappears completely when we run the model including only this variable. Apart from that, we do not find strong evidence that political alignment (with both, the federal or state government), the education or gender of the mayor, and the party affiliation are correlated with the positive or the negative shock. This indicates that the 2007 census indeed was used to correct previous errors in population figures unconditional on any political motivation. The last set of indicators captures other characteristics of the local population. While for this group of variables it is less clear how they could affect any manipulation, we find very few significant variables. Given that this large set of observables does not show strong correlations with treatment status, it is also unlikely

[14]Note that we cannot cluster at the state or bracket level because of small numbers of clusters. However, results are generally robust to the way of treating the standard errors.

that any unobservable would. Furthermore, the explanatory power of the political variables and other characteristics is limited as the R^2 remains almost unchanged compared to the baseline with fixed effects only (columns 1 and 6).

2.4 Results

2.4.1 Budgetary reactions

To discuss the results from equation 2-1 we show the estimated coefficients graphically. Each figure shows the two point estimates before the census in 2005 and 2006 as well as five estimates for the post-period from 2008 to 2012 (relative to 2007).

Figure 2.3 presents the estimates for the theoretical FPM formula transfer itself and confirms the relevance of the population update. The mechanism works as expected for both, the positive shock in the top panel and the negative shock in the bottom panel. In the case of the positive shock, the transfers jump precisely at the time of the shock and remain stable at the higher level during treatment, while pre-trends are zero. For the negative shock in the bottom panel we observe a sharp downward shift in 2008. However, the effect reverts to some extend in subsequent years. This is driven by the design of our sample: we keep municipalities at treatment even though they might shift back to a higher FPM coefficient after the negative shock, and this offsets the initial negative shock in some cases. These results also confirm the relevance of the census shock as an instrument, as formula transfers react precisely when local budgets were hit in 2008. Appendix figure B.1 shows that the mechanism also works as expected for actual FPM transfers. Nevertheless, we see slight trends before the shock took place for both the positive and negative shock. This illustrates the importance of using theoretical FPM transfers in our IV approach as allocation rules are not always fully enforced.

As a next step we analyze how the shocks translates into budgetary outcomes. Figure 2.4 shows results for total revenues (light red) and total expenditures (dark red) after a positive shock in panel (a) and after a negative shock in (b). Table 2.5 summarizes the point estimates. In the case of the positive shock, the additional amount of transfers translates immediately into additional revenues. Note that point estimates are lower compared to the transfer itself since revenues from the formula transfer are only a fraction of total revenues. The IV—elasticity—estimate (see table B.8 in the Appendix) shows that a 10% increase in formula transfers increases total revenues by

4.7%. The figure reveals that this increase in revenues is accompanied by an almost equal increase in expenditures (a 10% increase in transfers increases expenditures by 3.9%) and both effects are persistent over time. This implies that additional transfers are spent and not used, for example, for tax reductions suggesting the existence of a flypaper effect (Inman, 2008).

Panel (b) shows results for the same categories after a negative shock. We find that total revenues (and equally expenditures) drop sharply after a negative shock. The co-movement of both series confirms that Brazilian municipalities face a balanced budget constraint, and any shock to revenues yields necessarily a cut in expenditures. The respective elasticities confirm that a 10% decrease of transfers reduces total revenues on average over the 5 years by 1.8% and spending by 1.4% (columns 3 and 4 of Appendix table B.8). It is worth noting that the budget recovers relatively fast a few periods after the shock, which also explains the lower average elasticity of a negative shock compared to the one obtained after a positive shock. This indicates that, after initial budget cuts, some of the losses are compensated by increases in other types of revenues to compensate for the shortfall of transfers.

This effect is confirmed in figure 2.5 (see table 2.6 for the corresponding estimates). Taxes in municipalities hit by a positive shock increase relative to the control group, in line with a multiplier effect which might increase the tax base (Corbi et al., 2014).[15] Most interestingly, a similar effect occurs for the negative shock. Even though smaller in magnitude and less precise, tax revenues of jurisdictions hit by a negative shock tend to increase. Furthermore, note that this increase occurs even though part of the tax base might vanish due to multiplier effects.[16] This effect partially explains why total revenues as shown in 2.4(b) recover after some periods. Comparing both graphs reveal that total revenues (which include tax revenues) recover in the periods when local tax revenues increase. This confirms that local jurisdictions use tax policies to counteract the shortfall of transfer revenues.[17]

In Appendix B.5 we show that results are in general robust to local population controls (table B.13) and allowing treatment municipalities to enter the analysis in a dynamic fashion (table B.15).

[15]Corbi et al. (2014) provide evidence for a multiplier effect but they do not find an effect on tax revenues, which might be driven by the fact that their identifying variation relies on positive and negative shocks simultaneously.

[16]Controlling for local population increases the average effect over the entire post-treatment period, but might be biased due to endogeneity. Results available on Appendix table B.13

[17]This result is in line with the zero findings for taxes presented in Corbi et al. (2014). In their setting, identification is based on the difference between jurisdictions which shift to the left and to the right. If taxes increase to some extend independent of the direction, a RDD will result in a zero estimate.

2.4.2 Heterogeneous effects: Education of the mayor

The previous section discussed the average response across jurisdictions. The theoretical literature on fiscal capacity emphasizes the role that politics, social structures, and the quality of institutions and politicians in charge, have on fiscal capacity building (see the arguments provided in Besley and Persson, 2013). For this purpose, we wish to analyze if forces related to the structure of political institutions, the cohesiveness of society, and the human capital of the public administration play a role in the implemented adjustment process. In Brazil, the mayor's office proposes the annual budget to the legislative branch and, after its approval, has complete autonomy over the execution. Considering this policymaking authority that the mayor has, we begin the analysis focusing on the education of the mayor that runs the municipality during the shock as a proxy of the human capital of the public administration.

We split our sample in two groups. The first one includes municipalities with mayors which completed primary or secondary education (low education, blue shaded in the figures), and the second one mayors which attended college or university (high education, red shaded in the figures).[18] Figure 2.6 shows the total marginal effect for the evolution of revenues and expenditures for these two groups from an interacted model. Panel 2.6(a) and (b) confirm the previous results for the positive shock. In this case, no significant difference exists between the high- and low-education jurisdictions. The effects are significantly different from each other whenever the point estimate of one group is not supported by the confidence interval of the other group (and vice verse). Panel 2.6(c) and (d) instead show that the adjustment process after a negative shock is significantly different in both environments. Total revenues in municipalities with highly educated mayors recover much faster compared to those with low educated mayors. Expenditures follow a similar pattern, indicating that cuts have been much more severe in the group of low-educated mayors (see coefficients in Appendix table B.3). On the other hand, these results suggest that it is predominately the group of high-educated mayors which invests in fiscal capacity instead of cutting expenditures. Table 2.7 and Figure 2.7 provide evidence for this. Tax revenues increase for the group of highly educated mayors and are significantly different from those in municipalities of low-educated mayors for most of the periods. The elasticity for municipalities administered by high-educated mayors is 0.54 (see IV estimate $Shock\,X\,After\,+\,Shock\,X\,After\,X\,High\,Education$ in Appendix table B.10

[18] 56.88% of municipalities are run by highly-educated mayors in the positive shock sample, while this number is 57.66% in the negative shock sample.

column 3). Evaluated at the mean of these municipalities, a 1% decrease in formula transfers triggers an increase of tax revenues of 0.54% or R$ 48,995.18 in 2012 Brazilian Reais (approx. US$ 15,375.86). This implies roughly R$ 1 per inhabitant. Again, results are robust to population controls and to the alternative sample (see Appendix tables B.14 and B.16 respectively).

While it can be argued that the revenue shock in formula transfers was exogenous to municipalities, the human capital of the mayor in charge of the local government might not be randomly assigned. Thus, our measure of mayor education could be capturing other characteristics of the municipality, or the mayor itself, that lead to a differential effect of the FPM shock on local tax revenues. For instance, the education of the mayor could serve as a proxy of the education level of the municipality. If more educated citizens comply more often with their tax obligations compared to less educated citizens, then the positive effect on tax revenues that we found after a negative shock could be driven by municipalities with different education levels. Another possibility is income inequality. More equal societies may be willing to be taxed more to redistribute through the provision of important public services. Alternatively, in unequal municipalities, the median voter is poorer than the average, which could be reflected in a higher demand for redistributive policies and therefore, greater incentives for tax collection. Another potential confound is mayoral term limits. First-term mayors may have greater incentives to invest in fiscal institutions than term-limited mayors because they could capitalize on the benefits of the investment in an eventual second term.[19]

To test for these potential confounds we follow Ferraz and Finan (2008) and in columns 2 and 4 of table 2.7 we include a series of interactions terms where we allow the shock to vary with several characteristics of the municipality and the mayor: first term in office, political party alignment with the federal government (Labor Party - PT), Gini index, share of adults with college degree, literacy rates, per capita income, share of urban population and existence of local radio stations. We expressed all continuous variables as indicators of above-below the median of the distribution of sample municipalities. As can be seen from the table, our estimates remain significant and the magnitudes are almost identical relative to the estimation without interactions, in particular for the negative shock.

Appendix tables B.4 (positive shock) and B.5 (negative shock) present the results for each interaction separately. We do not find any significant differences on the impact of the negative shock on taxes by term-limits, federal political alignment, literacy rates, per capita income, adults' education,

[19]Brazilian mayors serve a four-year term and can hold office up to two consecutive terms.

and radio. We do find, however, significant and positive differential effects in more unequal municipalities. We interpret this result as a decrease of rent-seeking by the richest strata of the municipalities with its local administration (Mahlmeister et al., 2018).

Another source of revenues that municipalities have available to finance the provision of public services are discretionary federal grants. For most municipalities this type of federal transfers represents a very small share of their budgets (from 0% up to 18%—on average 3.4%). Nevertheless, this is a budgetary item that could be used by the central government as a form of compensation to those municipalities affected by the transfer shock. Therefore we analyze whether the shock affects discretionary federal grants considering not only the level of education of the mayor, but also if the political party alignment between the two levels of government matters for the allocation of these resources.

Results are presented in Appendix tables B.6 and B.7, and figure B.2. We do not find any significant effects in federal discretionary grants neither for the full sample of municipalities nor interacting the shock with the human capital of the mayor or the political alignment between the federal and municipal governments.

To sum up, our results provide evidence that negative revenue shocks provide incentives to invest in fiscal capacity, but only local governments with highly educated mayors do so. In the next section we explore potential mechanisms that municipalities could have used to increase local tax revenues.

2.5 Mechanisms

To shed light on the mechanisms driving the increase of tax revenues we analyze different channels through which a municipality can increase its fiscal capacity. We start by analyzing the local tax administration and turn to investment into tax related infrastructure afterwards.

2.5.1 Tax administration and tax employees

We use detailed labor market data which we observe at the individual level to examine contractual information of two employment categories directly related with the local tax administration: tax technicians and tax auditors.[20]

[20]Tax Technicians and Tax Auditors are described by the CBO2002 (*Classificacão Brasileira de Ocupacões)* of the Brazilian Ministry of Labor and Employment as: "They supervise compliance with tax legislation; constitute the tax credit through launching; control the collection and promote the collection of taxes, applying penalties; analyze and make

We observe the employer for each contract, which allows us to aggregate the information at the municipal level since the municipality itself appears as the contractor. We generate various indicators from this information.

We first examine how the number of tax technicians and/or tax auditors varies (intensive margin) after the shock, and how municipal government wage bill on tax workers responds to the exogenous change on federal transfers. We define tax workers' wage bill as total salaries paid in year t by the municipal government on tax auditors and tax technicians. We express these variables in log scale. Again, we split our sample according to the level of education of the mayor and present estimates with and without interacting the shock with other local characteristics.

Figure 2.8 shows the evolution of the number of tax workers and the wage bill on those employees from the interacted model with the human capital of the mayor (see point estimates in columns 1-4 in tables 2.8 and 2.9). Panels (a) and (b) show that there is a null effect on the two dimensions of tax workers for the positive shock, and that no significant differences exist between the high- and low-education jurisdictions. Most interestingly, the results change when we compare municipalities with low and high educated mayors facing a negative shock in panel (c) and (d). We find positive and significant effects on the intensive margin in high-education contexts precisely in the years in which we found that local tax revenues significantly increase (2009, 2010 and 2012). The coefficients of the IV estimates in Appendix table B.12 implies that a 10 percentage point decrease in FPM transfers increases in about 2 percentage points ($Shock\,X\,Year\,+\,Shock\,X\,Year\,X\,High\,Education$), on average, the total number of tax technicians and/or tax auditors in a municipality. Yet, this increment does not seem to be translated into the wage bill of the tax workers—despite the statistical differences regarding the low-educated jurisdictions, what suggests that highly educated mayors adjusted downwards the salaries of the area. Results are robust to the inclusion of other interactions as the even columns show.

Considering that a little less than 50% of the municipalities in our sample have declared to employ tax technicians and/or tax auditors between 2005 and 2012, we also examine the extensive margin of the tax administration. To observe whether a municipality has specialized tax workers in its local

decisions on administrative and tax proceedings; control the transaction of goods and services; assist and guide taxpayers, and also plan, coordinate and direct tax administration offices. For the exercise of the functions of Public and State Tax Auditors, a higher education course is required. For Tax Technician, middle level education is required. Access to the functions takes place through differentiated public exams, for auditors and technicians, according to specific legislation of the states and municipalities." ISCO-88 equivalent classification: Tax Collector; Inspector Taxation; Excise Officer; Tax Examiner.

administration we construct a dummy variable equal to 1 if municipality i employs at least one tax auditor and/or one tax technician in year t, and run equation 2-1 which can be interpreted in this context as a linear probability model in the event-study design. Columns 5-6 of tables 2.8 and 2.9 present the results. Although the results indicate that the average municipality that faced a negative shock with a highly educated mayor does not increase its local tax revenues by employing or not tax workers, we do find significant and positive differences in regard to low-educated mayors. An average municipality where the mayor has at least completed high school education has a higher probability (11 p.p.) of having a specialized tax worker in 2009 and 2010 than a municipality where the mayor has at most a high school diploma.

Summing up, the results shown so far are consistent with what was discussed in the previous section (see table 2.7). Municipalities affected negatively by the population update, and with high levels of human capital in the local government, invest in fiscal capacity, at least in part, through the recruitment of specialized tax workers.

2.5.2 Tax infrastructure

Another potential mechanism is to invest in infrastructure which improves tax collection. We collected data from a publication of the Brazilian Statistical Institute (IBGE) which surveys annually all municipalities. The waves 2005, 2006, 2009 and 2012 include specific questions about tax collection. From this information we code dummy variables about the existence of a real estate cadastre, a register of land and house prices, and a register of local service providers. These administrative tools are acknowledge as relevant for municipal tax collection in Brazil (Afonso et al., 2013). The real estate cadastre is the instrument through which the municipal administration registers the attributes, whether physical or of location, of the real estate located in its jurisdiction, and identifies the respective owners. In the register of land and houses prices, the tax administration estimates the market value of all properties of a municipality based on an individual valuation model of these properties. Self-employed professionals and legal entities that in their social contract have as purpose some activity that corresponds to the provision of services, must register as a local service provider to pay taxes on those activities.

Since the information is not available on an annual base, we collapse our data into one pre-shock and one post-shock period. In other words, we set the outcome variables equal to zero in the pre-shock (post-shock) period if the waves between 2005 and 2006 (2009 and 2012) indicate that the respective

register was not available and equal to one otherwise. Table 2.10 presents the results.

We do not find any clear evidence for fiscal capacity building by investing into infrastructure. One interpretation is that the positive shock makes municipalities less likely to invest in infrastructure which reduces costs, but we do not find evidence in either direction. After a negative shock, however, an average municipality where the mayor has at most completed high school decreases its probability of having both a real estate cadastre and a property price register in about 3.0 p.p. and 9.0 respectively. Jurisdictions administered by highly educated mayors seems to attenuate this negative effect, especially for the property price register, but decreases even more the probability of having a provider register ($Shock + Shock\,X\,High\,Education$ is significantly different to zero).

2.6 Conclusion

This paper provides novel evidence on the adjustment process of local budgets after fiscal shocks in a developing country context. The empirical exercise exploited the design of the allocation formula of the most important source of revenues that Brazilian municipalities have—a constitutional grant assigned by the central government. Importantly, our empirical strategy allows us to investigate budgetary reactions to both windfalls and shortfalls of federal formula grants.

We find evidence indicating a flypaper effect after a positive fiscal shock. Municipalities translate revenue windfalls in an almost identical increase in local expenditures, instead of reducing the tax burden on local voters. The adjustment is very dissimilar after a negative fiscal shock. In line with theory (see Besley and Persson, 2013), we find that a shortfall of non-tax revenues causes significant investment in fiscal capacity to absorb part of the revenue loss. The most striking result is that this effect depends crucially on the level of education of the mayor. We find that this effect only prevails for municipalities which are administered by a highly educated decisionmaker. Municipalities with low-education mayors, instead, cut expenditures to the same extent as the revenue shock hit the budget. Our results are also informative about the mechanism through which investment in fiscal capacity occurs. We are able to show that municipalities hire tax related workers to compensate the revenue loss. However, due to data limitations, we are unable to rule out that part of the increase in tax revenues is driven by an increase in tax rates.

These results are important from a policy perspective. Any shortfall of

fiscal resources, such as grants or aid programs, might have serious consequences for the local population if jurisdictions cut important expenditures. If public goods are consumed to a larger extent by the poor part of the population, these events might also cause negative equity effects. However, we also show that governments can dampen these effects if they have sufficient knowledge to implement policies which increase fiscal capacity and therefore tax revenues. Providing this information to jurisdictions which are administered by low skilled officials can help to mitigate the budgetary impact and to incentivize investment in fiscal capacity in local governments.

Tables

Table 2.1: FPM coefficients

Bracket	Population	Coefficient	% Variation
1	0-10,188	0.6	-
2	10,189-13,584	0.8	33.3
3	13,585-16,980	1	25.0
4	16,981-23,772	1.2	20.0
5	23,773-30,564	1.4	16.7
6	30,565-37,356	1.6	14.3
7	37,357-44,148	1.8	12.5
8	44,149-50,940	2	11.1
9	50,941-61,128	2.2	10.0
10	61,129-71,316	2.4	9.1
11	71,317-81,504	2.6	8.3
12	81,505-91,692	2.8	7.7
13	91,693-101,880	3	7.1
14	101,881-115,464	3.2	6.7
15	115,465-129,048	3.4	6.2
16	129,049-142,632	3.6	5.9
17	142,633-156,216	3.8	5.6
18	156,217-	4	5.3

Notes: Information retrieved from *Tribunal de Contas da União* (TCU).

Table 2.2: Population and budget items

		Population			Revenues (% of Total)			
Bracket	Share	Mean	Mean Growth	SD Growth	FPM	Local Taxes	State Trans.	Fed. Trans.
1	0.47	5,255	0.01	0.07	0.45	0.05	0.24	0.11
2-4	0.29	15,574	0.01	0.06	0.34	0.07	0.23	0.14
5-8	0.14	33,363	0.01	0.05	0.26	0.10	0.24	0.15
9-13	0.06	70,746	0.01	0.04	0.19	0.13	0.26	0.15
14-18	0.04	239,689	0.01	0.03	0.10	0.20	0.29	0.14
1-18	1.00	26,021	0.01	0.06	0.22	0.13	0.26	0.14

Notes: The table covers the population of Brazilian municipalities (does not include state capitals) and includes 44,300 observations of 5,538 municipalities over the period 2005-2012. The table reports by population bracket: proportion of municipalities, municipal population mean, municipal population growth mean and standard deviation, and main municipal sources of revenues as a share of total revenues. Main sources of municipal revenue comprise i) FPM transfers; ii) local taxes which include ISS (service tax), ITBI (tax on the transaction of properties), IPTU (property tax), IRRF (income tax retained by municipalities) and public services fees; iii) state transfers and iv) federal transfers net of FPM transfers.

Table 2.3: Summary statistics - Budgetary outcomes

Variables	Mean	SD	Min	Max
Expenditures	28,670.35	42,296.09	284.29	1,539,031.00
Revenues	29,322.74	43,604.32	755.06	1,442,916.00
- FPM transfers	7,024.90	4,676.10	1,532.21	62,964.89
- Taxes	2,757.93	9,087.84	0.00	315,664.30
- Fed. discretionary grants	1,009.08	1,596.50	0.00	40,052.43

Notes: The table covers the sample of municipalities used in the main analysis and includes 23,600 observations of 2,950 municipalities over the period 2005-2012. All values are in thousands of Brazilian Reais, expressed in 2012 constant prices.

Table 2.4: LPM - Determinants of the instrument

	Positive Shock					Negative Shock				
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log population 2006		0.638***			0.635***		-0.415***			-0.382***
		(0.071)			(0.067)		(0.059)			(0.056)
Population growth 2003-2006		-3.000***			-3.132***		3.993***			3.643***
		(0.503)			(0.508)		(0.954)			(0.908)
Distance threshold in 2007 (inhabitants)		-0.000**			-0.000*		0.000***			0.000***
		(0.000)			(0.000)		(0.000)			(0.000)
Population density 2006 (inhabitants/area)		-0.000			-0.000		0.000**			0.000
		(0.000)			(0.000)		(0.000)			(0.000)
Urban population 2000 (%)		-0.031			-0.029		-0.071**			-0.023
		(0.036)			(0.045)		(0.031)			(0.029)
Mayor gender (dummy)			0.044**		0.021			-0.022		-0.013
			(0.021)		(0.020)			(0.024)		(0.023)
Mayor age (years)			-0.001		-0.001*			-0.000		0.001
			(0.001)		(0.001)			(0.001)		(0.001)
Mayor education (dummy)			0.007		-0.006			-0.024*		-0.008
			(0.013)		(0.012)			(0.013)		(0.013)
Mayor first term (dummy)			0.009		0.001			-0.002		0.006
			(0.015)		(0.013)			(0.013)		(0.013)
Public administration education 2005 (dummy)			-0.012		-0.013			0.016		0.018
			(0.014)		(0.015)			(0.012)		(0.013)
Alignment state (dummy)			0.018		0.033**			0.019		0.010
			(0.018)		(0.017)			(0.019)		(0.018)
Alignment federal (dummy)			-0.005		0.005			-0.004		-0.008
			(0.022)		(0.021)			(0.025)		(0.022)
Party PMDB (dummy)			0.003		0.004			0.006		0.003
			(0.020)		(0.019)			(0.017)		(0.016)
Party PSDB (dummy)			-0.012		-0.013			0.012		0.006
			(0.023)		(0.022)			(0.020)		(0.019)
Party PP (dummy)			0.033		0.035			-0.009		-0.012
			(0.028)		(0.026)			(0.022)		(0.022)
Party PFL (dummy)			0.006		0.016			-0.003		-0.024
			(0.022)		(0.019)			(0.018)		(0.019)
Party PTB (dummy)			0.008		0.017			0.023		0.003
			(0.026)		(0.025)			(0.023)		(0.021)
Adults college degree 2000 (%)				-0.431	-0.824				-1.067*	-0.402
				(0.541)	(0.596)				(0.640)	(0.672)
Adults literate 2000 (%)				0.027	0.128				0.062	-0.002
				(0.156)	(0.147)				(0.106)	(0.106)
Life expectancy at birth 2000 (years)				0.000	0.002				-0.002	-0.001
				(0.005)	(0.004)				(0.004)	(0.004)
Gini 2000 (index)				0.343***	0.132				-0.228	-0.150
				(0.111)	(0.103)				(0.138)	(0.123)
Log income per-capita 2000 (R$)				0.025	0.027				-0.007	-0.035
				(0.037)	(0.037)				(0.031)	(0.035)
Radio station 2005 (dummy)				0.041	0.023				-0.135***	-0.096***
				(0.031)	(0.029)				(0.026)	(0.023)
Government website 2006 (dummy)				-0.020	-0.021				0.005	0.010
				(0.015)	(0.013)				(0.014)	(0.014)
Criminal court 2006 (dummy)				0.026	-0.009				-0.012	0.012
				(0.025)	(0.024)				(0.021)	(0.020)
Special civil court 2004 (dummy)				0.018	-0.027				-0.053**	-0.040*
				(0.020)	(0.021)				(0.023)	(0.021)
Municipal guard 2006 (dummy)				0.008	-0.005				-0.016	-0.012
				(0.034)	(0.029)				(0.029)	(0.026)
Zoning law 2005 (dummy)				0.024	0.015				-0.032*	-0.023
				(0.018)	(0.018)				(0.019)	(0.017)
Public library 2005 (dummy)				-0.008	-0.014				0.000	0.012
				(0.018)	(0.020)				(0.015)	(0.015)
Local bus 2005 (dummy)				0.029	0.016				-0.019	-0.013
				(0.019)	(0.017)				(0.021)	(0.019)
Touristic area 2005 (dummy)				0.028	0.021				0.001	0.005
				(0.018)	(0.018)				(0.019)	(0.017)
Economic incentives 2006 (dummy)				0.021	0.018				0.012	0.007
				(0.017)	(0.016)				(0.016)	(0.015)
Industrial district 2006 (dummy)				-0.013	-0.023				-0.035*	-0.028*
				(0.020)	(0.018)				(0.019)	(0.017)
Education council 2006 (dummy)				0.010	-0.005				0.007	0.001
				(0.015)	(0.014)				(0.015)	(0.014)
Civil defense council 2006 (dummy)				-0.009	-0.001				-0.005	-0.005
				(0.017)	(0.018)				(0.016)	(0.015)
Log distance to state capital (kms)				0.019	0.013				-0.012	-0.003
				(0.014)	(0.013)				(0.010)	(0.010)
Observations	2,588	2,587	2,588	2,583	2,583	2,632	2,631	2,632	2,627	2,627
R-squared	0.205	0.333	0.208	0.218	0.342	0.449	0.522	0.452	0.476	0.535

Notes: The table covers the sample of municipalities used in the main analysis. All models include threshold by state fixed effects. The political and mayor characteristics were constructed using data from Brazil's electoral commission (*Tribunal Superior Eleitoral*). The socioeconomic characteristics were constructed using data from IBGE and TCU. (*log population in 2006*) is the log of the municipal estimated population in 2006; *population growth 2003-2006* is the annual mean growth rate of municipal estimated population between 2003 and 2006; *distance to threshold in 2007 (inhabitants)* is the distance to the closest threshold in number of inhabitants; *population density in 2006 (inhabitants/area)* is municipal estimated population divided by municipal area; *urban population in 2000 (%)* is the share of 2000 census population that live in urban areas; *mayor education* is a dummy variable equal to 1 if mayor has attended college or university; *public administration education 2005* is a dummy variable equal to 1 if the share of local government employees with college degree is above the median of the distribution of municipalities; *alignment state* is a dummy variable equal to 1 if mayor belongs to same political party than state governor; *alignment federal* is a dummy variable equal to 1 if mayor belongs to any of the political parties of the formal federal government coalition (PT, PRB, PCdoB); *party* is a dummy variable equal to 1 if mayor belongs to party, where party: PMDB, PSDB, PP, PFL, PTB are major political parties in Brazil and accounts for approximately 65% of the mayors in 2005-2008; *adults college degree 2000 (%)* is the share of 2000 census population above 25 years with college degree; *adults literate 2000 (%)* is the share of 2000 census population above 25 years that is literate; *life expectancy at birth 2000 (years)* is the life expectancy at birth measured with 2000 census; *radio station 2005* is a dummy variable equal to 1 if municipality has a radio station in 2005; *government website in 2006* is a dummy variable equal to 1 if the municipal government has its own website in 2006; *criminal court 2006* is a dummy variable equal to 1 if municipality has a criminal court in 2006; *special civil court 2004* is a dummy variable equal to 1 if municipality has a special civil court in 2004; *municipal guard 2006* is a dummy variable equal to 1 if the municipality has a local police in 2006; *zoning law 2005* is a dummy variable equal to 1 if municipality has a zoning law in 2005; *public library 2005* is a dummy variable equal to 1 if the municipality has a public library in 2005; *local bus 2005* is a dummy variable equal to 1 if the municipality has a public bus in 2005; *touristic area 2005* is a dummy variable equal to 1 if municipality was declared as a tourist area in 2005; *economic incentives 2006* is a dummy variable equal to 1 if municipality provides economic incentives to business in 2006; *industrial district 2006* is a dummy variable equal to 1 if municipality has an industrial district in 2006; *education council 2006* is a dummy variable equal to 1 if municipality has an education council in 2006; *civil defense council 2006* is a dummy variable equal to 1 if municipality has a civil defense council in 2006. Robust standard errors, clustered at the state-threshold level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Reduced form - Revenues and expenditures

	Positive Shock		Negative Shock	
Outcome Variables	ln(*expenditures*) (1)	ln(*revenues*) (2)	ln(*expenditures*) (3)	ln(*revenues*) (4)
Shock X 2008	0.029	0.064***	-0.049***	-0.043***
	(0.022)	(0.014)	(0.011)	(0.011)
Shock X 2009	0.082***	0.092***	-0.035***	-0.019*
	(0.014)	(0.014)	(0.010)	(0.011)
Shock X 2010	0.090***	0.092***	-0.023**	-0.012
	(0.015)	(0.014)	(0.011)	(0.011)
Shock X 2011	0.088***	0.095***	-0.017	-0.004
	(0.016)	(0.016)	(0.011)	(0.011)
Shock X 2012	0.089***	0.101***	-0.008	0.007
	(0.016)	(0.016)	(0.012)	(0.012)
Shock X After	0.084***	0.103***	-0.020***	-0.016**
	(0.009)	(0.009)	(0.008)	(0.007)
Observations	20,704	20,704	21,056	21,056
Municipalities	2,588	2,588	2,632	2,632

Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. In odd columns the dependent variable is the log of municipal expenditures, while in even columns is the log of total municipal revenues. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Reduced form - Tax collection

	Positive Shock	Negative Shock
Outcome	ln(*taxes*)	ln(*taxes*)
Variables	(1)	(2)
Shock X 2008	0.000	-0.015
	(0.050)	(0.024)
Shock X 2009	0.028	0.034
	(0.032)	(0.025)
Shock X 2010	0.048	0.067**
	(0.030)	(0.028)
Shock X 2011	0.071**	0.035
	(0.034)	(0.031)
Shock X 2012	0.074**	0.058*
	(0.036)	(0.031)
Shock X After	0.057**	0.019
	(0.023)	(0.020)
Observations	20,704	21,056
Municipalities	2,588	2,632

Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. The dependent variable is the log of municipal tax revenues. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Reduced form - Tax collection: Mayor education heterogeneity

	Positive Shock		Negative Shock	
Outcome	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)
Variables	(1)	(2)	(3)	(4)
Shock X 2008	-0.014	-0.014	-0.061*	-0.060*
	(0.106)	(0.101)	(0.035)	(0.035)
Shock X 2009	0.016	0.016	-0.048	-0.043
	(0.048)	(0.047)	(0.035)	(0.036)
Shock X 2010	0.064*	0.062	0.024	0.028
	(0.037)	(0.038)	(0.038)	(0.040)
Shock X 2011	0.065	0.066	-0.035	-0.042
	(0.041)	(0.042)	(0.043)	(0.044)
Shock X 2012	0.079*	0.074*	-0.026	-0.045
	(0.043)	(0.044)	(0.047)	(0.047)
Shock X 2008 X High Education	0.026	0.029	0.075	0.060
	(0.113)	(0.105)	(0.047)	(0.048)
Shock X 2009 X High Education	0.021	0.038	0.133***	0.141***
	(0.064)	(0.064)	(0.049)	(0.053)
Shock X 2010 X High Education	-0.028	-0.022	0.070	0.072
	(0.057)	(0.059)	(0.054)	(0.063)
Shock X 2011 X High Education	0.010	0.021	0.115*	0.113*
	(0.066)	(0.068)	(0.060)	(0.065)
Shock X 2012 X High Education	-0.008	0.009	0.136**	0.145**
	(0.069)	(0.071)	(0.063)	(0.067)
Shock X After	0.042	0.037	-0.031	-0.033
	(0.038)	(0.038)	(0.028)	(0.029)
Shock X After X High Education	0.026	0.036	0.081**	0.067
	(0.047)	(0.046)	(0.039)	(0.042)
Other Interactions	No	Yes	No	Yes
Observations	20,704	20,704	21,056	21,056
Municipalities	2,588	2,588	2,632	2,632

Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. The dependent variable is the log of municipal tax revenues. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. Other interactions include: first term in office, political party alignment with the federal government (Labor Party - PT), Gini index, share of adults with college degree, literacy rates, per capita income, share of urban population and existence of local radio stations. We expressed all continuous variables included in the interactions as indicators of above/below (1/0) the median of the distribution of sample municipalities. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Reduced form - Tax workers: Mayor education heterogeneity

	Positive Shock					
Outcome	Intensive Margin		Wage Bill		Extensive Margin	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Shock X 2008	0.002	0.016	-0.238	-0.157	-0.030	-0.024
	(0.029)	(0.029)	(0.193)	(0.199)	(0.020)	(0.021)
Shock X 2009	-0.033	-0.026	-0.326	-0.287	-0.031	-0.028
	(0.044)	(0.044)	(0.307)	(0.312)	(0.032)	(0.032)
Shock X 2010	-0.072	-0.063	-0.576	-0.519	-0.056	-0.050
	(0.048)	(0.048)	(0.357)	(0.364)	(0.037)	(0.038)
Shock X 2011	-0.054	-0.044	-0.320	-0.282	-0.035	-0.030
	(0.048)	(0.048)	(0.361)	(0.363)	(0.037)	(0.038)
Shock X 2012	-0.010	-0.007	-0.003	0.012	0.001	0.004
	(0.056)	(0.056)	(0.373)	(0.377)	(0.038)	(0.038)
Shock X 2008 X High Education	-0.026	-0.023	0.102	0.166	0.013	0.017
	(0.041)	(0.039)	(0.285)	(0.274)	(0.029)	(0.029)
Shock X 2009 X High Education	0.043	0.029	0.438	0.384	0.030	0.023
	(0.060)	(0.061)	(0.427)	(0.433)	(0.044)	(0.044)
Shock X 2010 X High Education	0.068	0.064	0.663	0.659	0.053	0.051
	(0.064)	(0.066)	(0.480)	(0.488)	(0.049)	(0.050)
Shock X 2011 X High Education	0.080	0.074	0.515	0.560	0.040	0.042
	(0.070)	(0.069)	(0.501)	(0.505)	(0.051)	(0.051)
Shock X 2012 X High Education	0.098	0.101	0.639	0.688	0.046	0.050
	(0.081)	(0.078)	(0.531)	(0.530)	(0.053)	(0.053)
Shock X After	-0.010	-0.002	-0.171	-0.106	-0.021	-0.013
	(0.036)	(0.036)	(0.242)	(0.243)	(0.025)	(0.025)
Shock X After X High Education	-0.016	-0.010	0.149	0.191	0.014	0.017
	(0.053)	(0.054)	(0.373)	(0.381)	(0.037)	(0.038)
Other Interactions	No	Yes	No	Yes	No	Yes
Observations	20,704	20,704	20,704	20,704	20,704	20,704
Municipalities	2,588	2,588	2,588	2,588	2,588	2,588

Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. In columns 1 and 2, the dependent variable is the log of the total number of tax auditors and/or tax technicians employed by municipality i in year t. In columns 3 and 4, the dependent variable is the log of the total salaries paid to tax auditors and/or tax technicians by municipality i in year t. In columns 5 and 6, the dependent variable is a dummy variable equal to 1 if municipality i employs at least one tax auditor and/or one tax technician in year t. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. Other interactions include: first term in office, political party alignment with the federal government (Labor Party - PT), Gini index, share of adults with college degree, literacy rates, per capita income, share of urban population and existence of local radio stations. We expressed all continuous variables included in the interactions as indicators of above/below (1/0) the median of the distribution of sample municipalities. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.9: Reduced form - Tax workers: Mayor education heterogeneity

	Negative Shock					
Outcome	Intensive Margin		Wage Bill		Extensive Margin	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Shock X 2008	-0.005	0.009	-0.299	-0.205	-0.038	-0.030
	(0.035)	(0.036)	(0.238)	(0.254)	(0.024)	(0.025)
Shock X 2009	-0.049	-0.038	-0.769**	-0.691*	-0.083**	-0.076**
	(0.053)	(0.056)	(0.337)	(0.359)	(0.032)	(0.034)
Shock X 2010	-0.086	-0.074	-0.912**	-0.795**	-0.093***	-0.080**
	(0.062)	(0.064)	(0.376)	(0.393)	(0.036)	(0.038)
Shock X 2011	-0.034	-0.026	-0.554	-0.470	-0.050	-0.039
	(0.058)	(0.060)	(0.378)	(0.402)	(0.037)	(0.040)
Shock X 2012	0.018	0.016	-0.147	-0.104	-0.020	-0.013
	(0.064)	(0.066)	(0.451)	(0.474)	(0.045)	(0.047)
Shock X 2008 X High Education	0.030	0.043	0.315	0.396	0.038	0.047
	(0.048)	(0.050)	(0.299)	(0.317)	(0.029)	(0.031)
Shock X 2009 X High Education	0.151**	0.184**	1.135***	1.312***	0.112***	0.129***
	(0.071)	(0.081)	(0.425)	(0.460)	(0.040)	(0.044)
Shock X 2010 X High Education	0.200**	0.213**	1.267***	1.202**	0.113**	0.108**
	(0.085)	(0.090)	(0.486)	(0.495)	(0.045)	(0.045)
Shock X 2011 X High Education	0.139*	0.132	0.767	0.740	0.055	0.051
	(0.081)	(0.094)	(0.488)	(0.546)	(0.046)	(0.052)
Shock X 2012 X High Education	0.119	0.111	0.526	0.511	0.035	0.028
	(0.089)	(0.099)	(0.561)	(0.607)	(0.054)	(0.058)
Shock X After	0.002	0.015	-0.224	-0.119	-0.027	-0.015
	(0.047)	(0.049)	(0.310)	(0.329)	(0.030)	(0.032)
Shock X after X High Education	0.087	0.097	0.425	0.448	0.034	0.036
	(0.065)	(0.070)	(0.389)	(0.402)	(0.037)	(0.038)
Other Interactions	No	Yes	No	Yes	No	Yes
Observations	21,056	21,056	21,056	21,056	21,056	21,056
Municipalities	2,632	2,632	2,632	2,632	2,632	2,632

Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. In columns 1 and 2, the dependent variable is the log of the total number of tax auditors and/or tax technicians employed by municipality i in year t. In columns 3 and 4, the dependent variable is the log of the total salaries paid to tax auditors and/or tax technicians by municipality i in year t. In columns 5 and 6, the dependent variable is a dummy variable equal to 1 if municipality i employs at least one tax auditor and/or one tax technician in year t. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. Other interactions include: first term in office, political party alignment with the federal government (Labor Party - PT), Gini index, share of adults with college degree, literacy rates, per capita income, share of urban population and existence of local radio stations. We expressed all continuous variables included in the interactions as indicators of above/below (1/0) the median of the distribution of sample municipalities. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

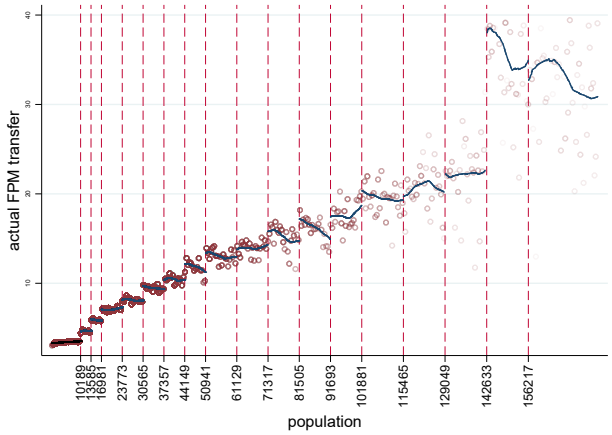
Table 2.10: Reduced form - Tax Infrastructure: Mayor education heterogeneity

Panel A	Positive Shock					
Outcome	Real Estate Cadastre		Property Price Register		Provider Register	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Shock	-0.024	-0.022	0.023	0.013	0.015	0.003
	(0.024)	(0.024)	(0.042)	(0.042)	(0.032)	(0.032)
Shock X High Education	0.033	0.033	-0.022	-0.015	-0.021	-0.023
	(0.026)	(0.026)	(0.052)	(0.053)	(0.040)	(0.040)
Other Interactions	No	Yes	No	Yes	No	Yes
Observations	5,174	5,174	5,174	5,174	5,174	5,174
Municipalities	2,587	2,587	2,587	2,587	2,587	2,587
Panel B	Negative Shock					
Outcome	Real Estate Cadastre		Property Price Register		Provider Register	
Variables	(7)	(8)	(9)	(10)	(11)	(12)
Shock	-0.031*	-0.034*	-0.088**	-0.098**	-0.001	-0.014
	(0.017)	(0.018)	(0.037)	(0.038)	(0.031)	(0.032)
Shock X High Education	0.029	0.024	0.092**	0.122**	-0.036	-0.016
	(0.020)	(0.021)	(0.044)	(0.048)	(0.037)	(0.041)
Other Interactions	No	Yes	No	Yes	No	Yes
Observations	5,262	5,262	5,262	5,262	5,262	5,262
Municipalities	2,631	2,631	2,631	2,631	2,631	2,631

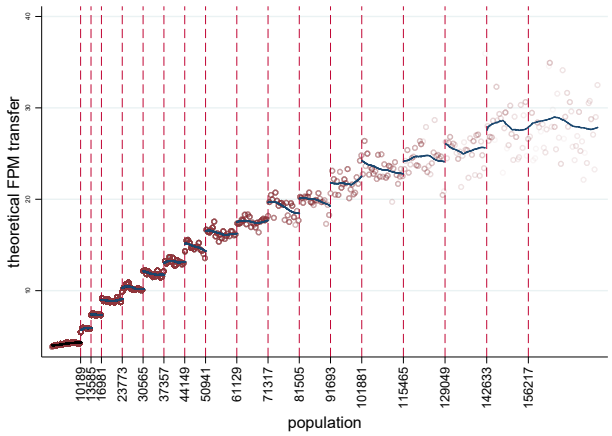
Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. In columns 1, 2, 7 and 8, the dependent variable is a dummy variable equal to 1 if municipality has a real estate cadastre. In columns 3, 4, 9 and 10, the dependent variable is a dummy variable qual to 1 if municipality has a register of land and house prices. In columns 5, 6, 11 and 12, the dependent variable is a dummy variable equal to 1 f municipality has a register of local service providers. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. Other interactions include: first term in office, political party alignment with the federal government (Labor Party - PT), Gini index, share of adults with college degree, literacy rates, per capita income, share of urban population and existence of local radio stations. We expressed all continuous variables included in the interactions as indicators of above/below (1/0) the median of the distribution of sample municipalities. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figures

Figure 2.1: Population brackets and FPM transfers



(a) actual FPM transfers



(b) theoretical FPM transfers

Notes: 2005-2012 scatter plot of actual (panel a) and theoretical (panel b) FPM transfers averaged over 500-inhabitants bins and running-mean smoothing between thresholds (blue lines).

Figure 2.2: Correlation between different shocks

Notes: The figure shows the scatter plot and linear prediction between the census shock in t (2008) and the previous census shock in 2002 (t-6) after controlling for state-bracket fixed effects.

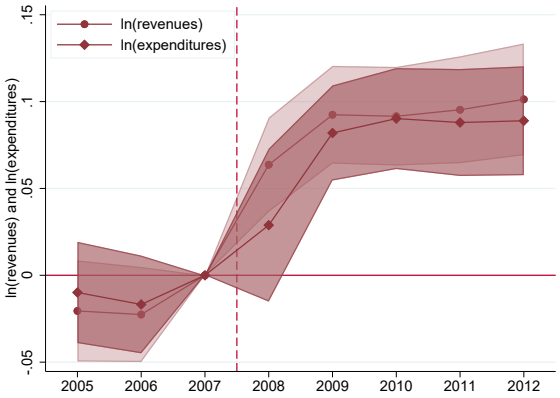
Figure 2.3: Formula transfer (theoretical FPM)

(a) positive shock

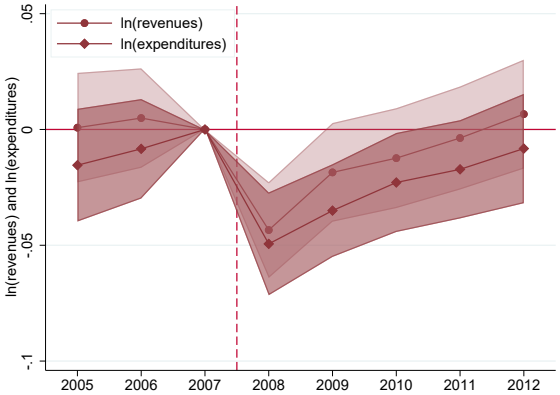
(b) negative shock

Notes: The figure shows the results from equation 2-1 for theoretical FPM transfers. The top panel (a) shows effects for the positive shock and the bottom panel (b) shows effects for the negative shock. 95% confidence intervals indicated around the point estimates.

Figure 2.4: Revenues and expenditures



(a) positive shock



(b) negative shock

Notes: The figure shows the results from equation 2-1 for revenues (light red) and expenditures (dark red) after a positive shock in the top panel (a) and a negative shock in the bottom panel (b). 95% confidence intervals indicated around the point estimates. Table 2.5 presents the corresponding estimates.

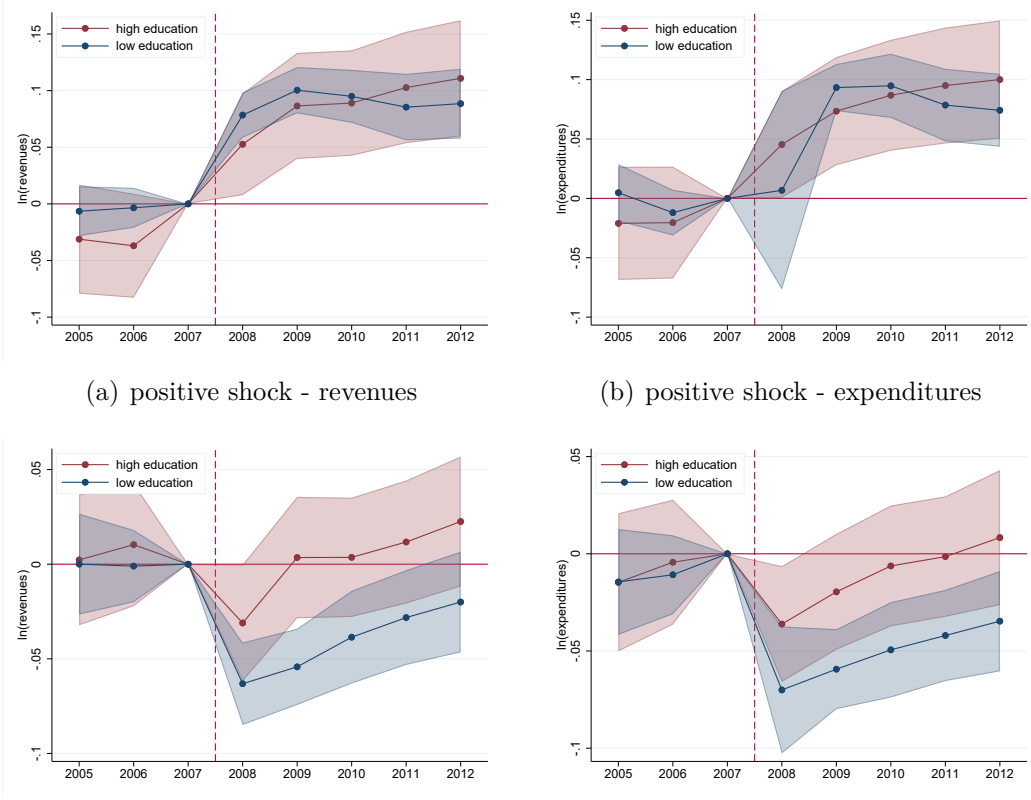
Figure 2.5: Tax collection

(a) positive shock

(b) negative shock

Notes: The figure shows the results from equation 2-1 for municipal tax revenues after a positive shock in the top panel (a) and a negative shock in the bottom panel (b). 95% confidence intervals indicated around the point estimates. Table 2.6 presents the corresponding estimates.

Figure 2.6: Revenues and expenditures: Mayor education heterogeneity



(a) positive shock - revenues

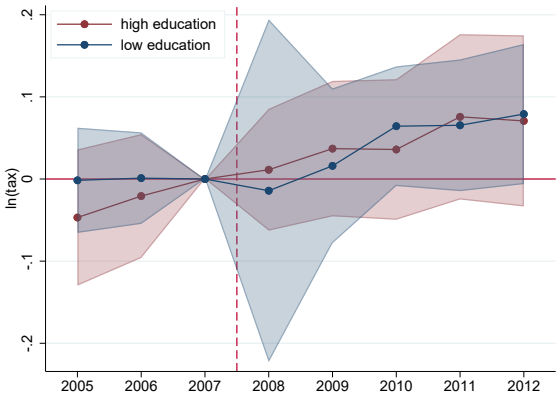
(b) positive shock - expenditures

(c) negative shock - revenues

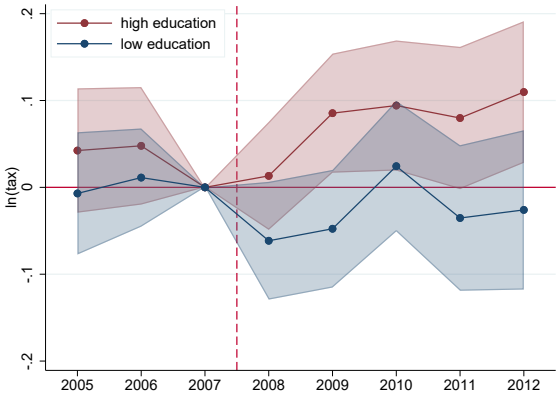
(d) negative shock - expenditures

Notes: The figure shows the results from equation 2-1 for revenues and expenditures, interacting the shock with an indicator of the human capital of the mayor. Red (blue) illustrates municipalities where the elected mayor in 2008 has at least (at most) high school education. The top panel shows effects for the positive shock and the bottom panel shows effects for the negative shock. 95% confidence intervals indicated around the point estimates. Table B.3 presents the corresponding estimates.

Figure 2.7: Tax collection: Mayor education heterogeneity



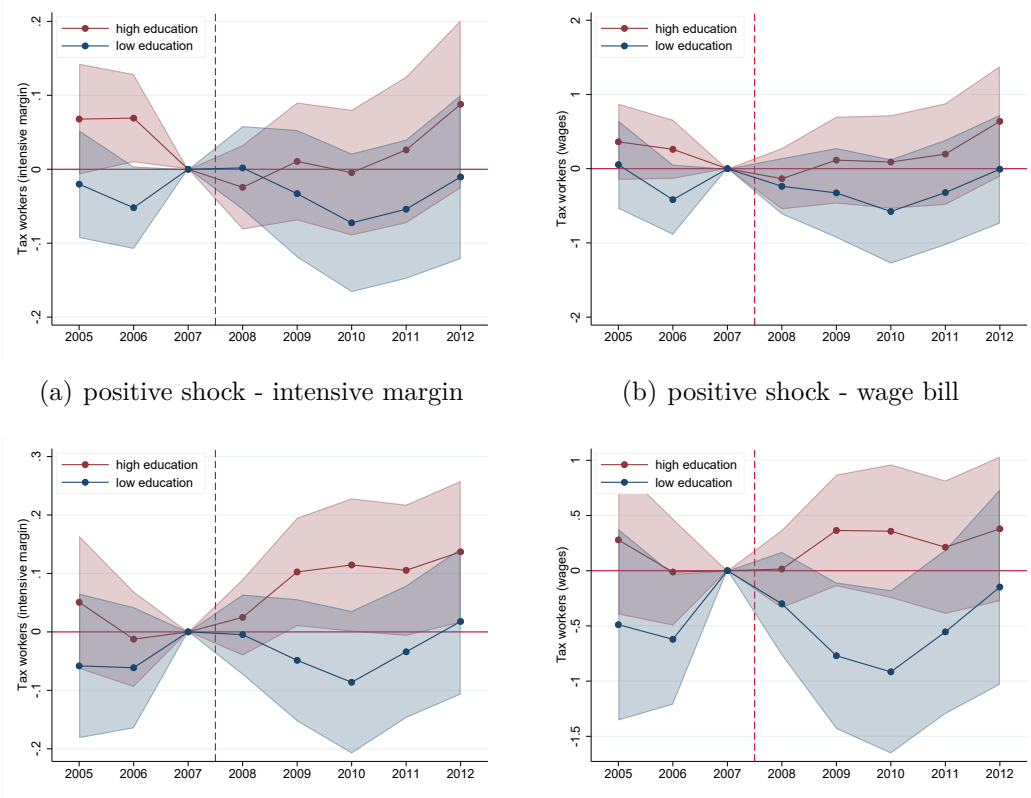
(a) positive shock



(b) negative shock

Notes: The figure shows the results from equation 2-1 for municipal tax revenues, interacting the shock with an indicator of the human capital of the mayor. Red (blue) illustrates municipalities where the elected mayor in 2008 has at least (at most) high school education. The top panel (a) shows the effect after a positive shock and the bottom panel (b) shows the effect after a negative shock. 95% confidence intervals indicated around the point estimates. Table 2.7 presents the corresponding estimates.

Figure 2.8: Tax workers: Mayor education heterogeneity



(a) positive shock - intensive margin

(b) positive shock - wage bill

(c) negative shock - intensive margin

(d) negative shock - wage bill

Notes: The figure shows the results from equation 2-1 for tax workers' intensive margin and wage bill, interacting the shock with an indicator of the human capital of the mayor. Red (blue) illustrates municipalities where the elected mayor in 2008 has at least (at most) high school education. The top panel shows effects for the positive shock and the bottom panel shows effects for the negative shock. 95% confidence intervals indicated around the point estimates. Tables 2.8 and 2.9 present the corresponding estimates.

3
Knowledge-Based Hierarchies and the Organization of the State: Evidence from Brazil

3.1
Introduction

The effective functioning of public sector institutions is an important factor for economic development (Besley and Persson, 2010). The literature on state effectiveness has emphasized the importance of effective public service delivery and its connection with leaders' identities (Chattopadhyay and Duflo, 2004; Jones and Olken, 2005; Besley et al., 2011; Martinez-Bravo, 2014, 2017), with incentives to front-line staff (Duflo et al., 2012; Ashraf et al., 2014), and, recent contributions, with bureaucrats' performance (Nath, 2015; Best et al., 2017; Khan et al., 2018). Yet, there is little evidence connecting the organization structure in which bureaucrats work and public sector capacity.

Motivated by the recent theoretical literature on firm organization (Garicano, 2000; Garicano and Rossi-Hansberg, 2006; Caliendo and Rossi-Hansberg, 2012), an important body of empirical work has emerged showing the importance of organization structures on firms' performance (Tåg, 2013; Caliendo et al., 2015b,a; Friedrich et al., 2015; Tåg et al., 2016). This literature adopt the concept of a *layer* of employees. A layer is comprised by a group of employees with similar characteristics summarized in their knowledge. According to the theory, a firm organizes knowledge following a pyramid design in which lower layers are bigger and have less knowledgeable employees than higher layers, forming, consequently, a knowledge-based hierarchy. As a result, in a knowledge-based hierarchy the knowledge to solve the easiest or more common problems is concentrated in the lowest layer of the firm, whereas the harder or more infrequent obstacles are handled in the highest layers. The firm problem is then to choose how many layers to have in the organizational structure and, in each layer, the number of working hours and the level of knowledge of its employees. Using employer-employee firm data the empirical literature reports two important findings. First, most firms are hierarchical in their layers both in terms of number of hours hired and level of knowledge at each layer. Second, firms' productivity is associated with the way that firms organize their knowl-

edge (see Caliendo et al., 2015a). An immediate question that arises from the aforementioned findings is whether these organizational actions and decisions are important also for the organization and effectiveness of public institutions Rasul and Rogger (2017) provide one of the first descriptive evidence on this matter. In particular, using data from the Nigerian Civil Service they show that management practices associated with bureaucrats' autonomy correlate to the quantity and quality of public project completion rates, suggesting that organizational structures and practices are also important for public service delivery. Borrowing the insights from the recent literature on firm organization, our study aims to bring new evidence on the importance of public sector bureaucratic organizational structures and state effectiveness.

We apply the knowledge-based hierarchies framework to study the internal organization of Brazilian local governments. We use employer-employee data from the universe of Brazilian municipal governments over 2003-2012 to first demonstrate that municipal bureaucracies are organized as a hierarchical triangular structure. We measure municipalities' hierarchies based on workers' occupations, and show that lower layers of municipal employees are larger in the number of working hours employed and earn lower wages than higher layers. We then present suggestive evidence that municipal employees categorization into layers of knowledge is a useful economic classification of municipal bureaucracies. Despite our empirical strategy is not designed to estimate the causal effects of bureaucratic organizational structures on local government outcomes, we present descriptive evidence that suggests that the organization of municipal bureaucracies in layers of knowledge can be importantly related to an indicator of government capacity. In particular, we propose to use the obtainment of discretionary federal grants as a proxy of capacity. Since the access to discretionary federal grants is not granted by any law, we believe that obtaining this type of funds can be used as a dimension of the capacity of Brazilian municipalities. We find that a bureaucratic reorganization that adds a layer of knowledge correlates positively with the monetary amount of discretionary federal grants that a municipality receives.

Another important element of the structure of an organization is the span of control of its principals (Garicano, 2000). The classical models of hierarchies and organizational structures (Williamson, 1967; Calvo and Wellisz, 1978; Qian, 1994; Aghion and Tirole, 1997) pose the economic trade-offs of the breadth of principals' span of control. According to these, a wide span of control increases the monitoring cost of each agent by the principal but could spur the agent's effort associated with a diminution in oversight, the so-called *initiative effect*. Aghion and Tirole (1997) consider that the increase

of autonomy to a subordinate (i.e. increased span of control) incentivizes the agent to acquire relevant information and to participate more actively in the organization's activities. To complement our analysis of municipal knowledge-based hierarchies, we study whether bureaucrats' autonomy affects the correlation between layers of knowledge and our measure of government capacity. We find that this correlation is larger, the larger the autonomy of subordinates.

The remainder of the paper is structured as follows. Section 3.2 discusses the theory of knowledge-based hierarchies. Section 3.3 presents the data and introduces the empirical methodology to define layers of knowledge. Section 3.4 describes municipalities' internal organization in terms of hierarchies. Section 3.5 shows our descriptive evidence correlating municipal organizational structures and government capacity. Section 3.6 concludes.

3.2 Theory description

We briefly characterize in this section the theory of knowledge-based hierarchies initially proposed by Garicano (2000), and further developed by Garicano and Rossi-Hansberg (2006) and Caliendo and Rossi-Hansberg (2012).[1] The starting point of the theory is to acknowledge that production requires physical inputs (e.g. labor) and knowledge about how to combine them to obtain a good or provide a service. Under certain circumstances workers do not need to possess all the essential knowledge to produce. If communication is accessible, for instance, they may acquire the minimum necessary knowledge for their tasks, and when challenged with a problem they cannot solve, they may ask another worker of the organization. If that is the case, the organization has to decide who must learn a certain matter and to whom each worker must consult when faced with an unknown obstacle. In a knowledge-based hierarchy the knowledge to solve the easiest or more common obstacles is concentrated in the lowest layer of the organization. When this layer confronts a problem that cannot solve, they hand over the challenge to the layer immediately above. Problems move up through the hierarchical ladder until someone is able to solve them. In such an organizational structure, therefore, the knowledge about the harder or more infrequent obstacles is concentrated in the higher layers of the hierarchy. However, despite being endowed with more knowledge the higher layers cannot run the entire production process by themselves, because they are time constrained and needs the inputs of the lower layers to focus on the

[1]For a detailed discussion of the model and proofs of the results, we referred the reader to the cited papers.

obstacles that only the higher layers can solve.

The organization problem is then to choose how many layers to have in the organizational structure and, in each layer, the number of working hours and the level of knowledge of its employees. The theory considers wages as an indicator of the knowledge level of the workers, as wages summarize the marketable characteristics of the employees. Consequently, an employee will obtain a higher salary if is more knowledgeable about the production process than another. As organizations grow they need to hire more workers and/or add more layers of knowledge because additional, or unusual, problems will arise. When organizations add a layer, preexisting layers do not need to possess the level of knowledge that had before the reorganization, considering that its workers can now solve those harder problems by asking to the recently added layer of knowledge. Since wages are established based on workers' level of knowledge, average wages at each preexisting layer should decrease when layers are added to the organization structure. Thus, as organizations grow by adding layers they hire more but less knowledgeable workers at all preexisting layers. The reverse also holds when organizations drop layers.

To summarize, the theory has the following implications: i) organizations are hierarchical. Lower layers have more workers and lower average wages than higher layers; ii) adding (dropping) layers is associated with increases (decreases) in the number of workers in each layer but with decreases (increases) in the average wage in each preexisting layer.

Several papers went to the data guided by the implications of the theory to study the internal organization of firms. Caliendo et al. (2015b) show that the empirical patterns match the theoretical predictions using data on French manufacturing firms, whereas Tåg (2013) and Caliendo et al. (2015a) do the same for Swedish and Portuguese firms, respectively. The latter also study the endogenous response of firm productivity to a reorganization in the number of layers and finds that such a reorganization is positively associated with firm productivity. Others have also used the hierarchical framework to study wage inequality among Danish firms (Friedrich et al., 2015), and entrepreneurship in Sweden (Tåg et al., 2016). On the other hand, in the Brazilian context, Cruz et al. (2018) study the impact of a capacity building program on firms' hierarchical organization and how this interacts with firms' export performance.

We follow a similar path than the aforementioned empirical studies but to analyze the organizational structure of an important Brazilian public institution: local governments.

3.3 Data

Our data on the hierarchical structure of municipalities is constructed using an employer-employee dataset from the Brazilian Ministry of Labor called *Relação Anual de Informações Sociais* (RAIS). Each year the Brazilian Ministry of Labor collects contractual information of all formal employers and its respective employees, including wages, hours of work, education, age, and gender. Furthermore, RAIS also includes the occupational classification of each worker following a Brazilian version of the International Standard Classification of Occupations (ISCO-88). The Brazilian Classification of Occupations (CBO2002) assigns workers to one of ten main occupational categories, aggregated by the level of skills required and similarity in the performed activities.[2] We collect data for all municipalities for which RAIS is available for the period 2003-2012.[3] However, we exclude state capitals from the analysis due to a lack of variation of the employed occupational categories in these local governments.

The second dataset that we use contains information on discretionary federal grants and was drawn from *Portal da Transparência*. The dataset includes for each discretionary grant transferred to the municipalities by the federal government the following information: value in Brazilian Reais (R$), period of validity of the agreement, purpose of the agreement, and the granting ministry. We use this information to construct a proxy of bureaucratic performance to study how the organization of the municipal bureaucracy in layers of knowledge correlates with an indicator of state capacity. To get access to these discretionary resources municipalities have to elaborate and present a work plan which is analyzed in terms of its expected results, feasibility, technical qualifications and managerial capacity to execute the object of the agreement.[4] On that account, we believe that obtaining resources from discretionary federal grants is a reasonable proxy of a dimension of local government capacity.

[2]The CBO is an occupational classification and not an educational, training or degree classification, since individuals of similar training can practice different occupations. For instance, an engineer working as a financial analyst at a bank will be classified as a financial analyst rather than as an engineer. A physician who works as a director of a hospital will be classified as a director of a hospital and not as a physician.

[3]In 2002 there was a major revision of occupational codes that resulted in the substitution of the Brazilian Classification of Occupations 1994 (CBO94) for the CBO2002, which came into force in 2003.

[4]For a description of discretionary federal grants in Brazil see this document prepared by the *Tribunal de Contas da União* in 2016.

3.3.1 Occupational structure

Following the empirical methodology developed by Caliendo et al. (2015b) we use the CBO2002 codes to define the number of knowledge-based layers in each municipality. The objective is to group a set of workers with similar levels of knowledge and authority into the same layer. That is, we would like to separate employees according to the number of layers of subordinates that they have below them, and not based on the functional characteristics of the tasks they perform. For that, we first assign a rank to each employee in each municipality, and then we define the number of layers considering the number of distinct ranks occupied in the municipality. Table 3.1 shows the original CBO2002 categories, with its official designated skill level, and our rank classification. The highest rank (*Senior Government Officials*) includes executive secretaries and senior members of the executive branch. The second rank (*Directors and Managers*) consists of department directors and division managers. The next rank (*Professionals and Technicians*) consists of workers classified as professionals of science and the arts, and technicians of intermediate level. The last rank (*Clerks, Service Workers and Production Workers*) comprises white-collar non-supervisory positions.[5]

To understand whether the proposed rank classification is adequate to capture a form of distance consistent with hierarchies, in table 3.2 we present percentiles of the distribution of contracts—total number of labor contracts signed by a municipality in a given year, hours—total number of hours hired by a municipality in a given year, and wages—hourly wage expressed in 2012 reais—in the different ranks of workers. The distributions are clearly ranked. Municipalities tend to sign fewer contracts, hire less number of hours and pay higher wages in higher ranks. Contracts and hours increase, while wages decrease as we move to lower ranks. This is true at all percentiles, although the between rank differences are smaller at the lower end of the distribution. Since distributions are distinctly ranked, we translate the number of different ranks in a municipality into layers of knowledge. A municipality reporting in a given year R distinct occupational ranks will be defined as having $L = R$ layers of knowledge (as long as there is at least one hour of work employed in an occupational rank, a layer exists). Therefore a municipality with occupational ranks 1 and 3, for instance, will have 2 knowledge-based layers, corresponding

[5]Considering our interest in studying the bureaucratic structure of organizations, we do not include in our rank classification occupations associated with neither education provision (e.g. teachers, professors, school directors) nor with health provision (e.g. physicians, nurses, hospital directors). Nevertheless, results are generally robust to the inclusion of these occupation categories.

to some non-skilled white-collar workers and to some managers.[6]

Table 3.3 (Appendix table C.1) presents the number of municipalities by year, as well as the average (median) number of inhabitants, contracts, hours, hourly wage and layers, for the ten years period of our data. There are clear trends over time. Municipal bureaucracies seem to become larger through the years as the total number of contracts and hired hours increase, yet, hourly wages fluctuate around R$ 7. On the other hand, municipalities employ a positive number of hours, on average, in approximately 3.2 layers out of a potential maximum of 4 layers (recall that we classified workers using 4 distinct ranks). Since we are interested in studying how municipalities change as they drop or add layers of knowledge, we need that municipalities do not tend to hire workers in all layers. Despite the average number of layers not being so far from its maximum, during 2003-2012, on average, 27% (19%) of municipalities changed its number of layers in the year in which a first-term (second-term) mayor took office, while 15% did it in the rest of the years. Hence, there are opportunities in our data for municipalities to reorganize in terms of the number of layers of its structures.

We have shown in this section that our occupational ranks are clearly ranked. However, much more is needed to demonstrate that this classification of employees into layers is a useful economic classification of municipal bureaucracies. We aim to show this relevancy in the following sections.

3.4 Municipalities' hierarchies

So far we have classified employees in ranks and have defined the number of layers of knowledge considering the number of distinct ranks, but we have not established which layers municipalities actually include in their organizational structure. We study this in table 3.4. As can be seen, municipalities tend to have consecutively ordered layers starting from layer 1. We define a municipality as having consecutively ordered layers if it has 1 (2) [3] {4} layers and reports an occupation in rank 1 (1 and 2) [1, 2 and 3] {1, 2, 3 and 4}.[7] About 77% of municipalities that have only one layer indeed have employees in rank 1.[8] Moreover, the vast majority of municipalities with two and three layers also organize their bureaucratic structure in an ordered fashion, representing

[6]Would be possible to use fewer or more ranks. However, the hierarchical structures that result generally fail to induce clearly ordered distributions of contracts, hours and/or wages.

[7]By definition all municipalities with four layers have consecutively ordered layers of knowldege.

[8]We assume that mayors are the main supervisor in one layer municipalities.

86% and 94% of those municipalities respectively. Overall, 94% of our sample municipalities have consecutively ordered layers.

The empirical evidence of knowledge-based hierarchies of production firms suggests that hierarchies are pyramidal since lower layers employ more hours of work and pay lower hourly wages. Table 3.5 shows that this is also true for municipal bureaucracies. Each panel of the table presents the mean and median values for contracts, hours and hourly wages, across layers as a function of the number of layers in the municipality. That is, for a municipality that has $L = 3$ layers, for instance, the table reports the mean (and median) for layers 1, 2 and 3. Is important to highlight that in this table layers' numbers do not necessarily mirror the occupational ranks of the employees. Namely, a municipality with one technician and one clerk will have two layers and consist of workers in ranks 2 and 1. This municipality will appear in the same cell as a municipality consisting of one government official and one division manager (ranks 4 and 3). Our definition of layers seems to capture adequately the hierarchical structure of Brazilian municipalities. On average, higher layers consist of a small number of individuals supervising larger groups of workers in lower layers and earning higher hourly wages, which, according to the theory, can be understood as a proxy of larger knowledge in management. Observe also that municipalities with more layers tend to be larger in terms of contracts and hours. However, the difference between four and three layers municipalities is not so big (422 versus 395 contracts), what suggests that these municipalities are making different organizational decisions.

Panels A, B and C of table 3.6, display the fraction of municipalities that satisfy a hierarchy in contracts, hours and wages, respectively. A municipality satisfies a hierarchy in contracts or hours between layer l and $l + 1$ in a given year, if the number of total annual contracts or hours in layer l is at least as large as the number of total annual contracts or hours in layer $l + 1$. On the other hand, a municipality satisfies a hierarchy in wages between layer l and $l + 1$ in a given year, if the average hourly wage in layer l is at most as large as the average hourly wage in layer $l + 1$. The majority of municipalities satisfy a hierarchical ranking of layers. For instance, in a municipality of four layers, the highest of the four has fewer workers than the third layer in 76% of the municipality-year observations and has larger hourly wages 76% of the times. Despite certain imprecisions, such as with the hierarchies of wages between layers 2 and 3, we believe that the evidence shown so far suggests that is reasonable to think in the representative municipality as having a hierarchical triangular structure.

3.4.1 Layers transitions

As previously discussed, even though on average municipalities have a number of layers that is close to its potential maximum of four, a relatively large proportion also add or drop layers. Hence, is important to determine how often they do so and the patterns of this reorganization. We begin the analysis by presenting a transition matrix across layers. Table 3.7 shows that between 12% and 30% of municipalities in a given year add or drop layers, and consequently restructure its bureaucracy. Note also that the vast majority of these municipalities add or drop only one layer. Moreover, with the exception of municipalities with one layer, we see very few reorganizations that add or drop more than one level of knowledge. These results underline two important features of Brazilian local bureaucracies. Large expansions or contractions are unusual, and when municipalities decide to expand or contract, they perform so in a systematic way.

Reorganizations are characterized by different municipality-level adjustments. We now analyze how municipal bureaucracies change when they add or drop layers of knowledge. The theory of knowledge-based hierarchies of production firms implies that a reorganization that adds layers of knowledge leads to increases in the number of hours hired in each layer but to a decrease in the average wage in each preexisting layer. The rationale is that by adding a new layer of knowledge, the firm can economize in the knowledge (i.e. wages) that is infrequently used by the bottom ranks in the hierarchy. That is, they hire more, but less knowledgeable workers at all preexisting layers, because now the new top manager can solve those infrequent challenges. All these conjectures reverse when the reorganization leads to a reduction in the number of layers.

We study how contracts, hours and wages changes depending on whether the municipality adds, does not change or drops layers. As shown in table 3.3 most of these municipality-level variables present some trends over time. To account for this, we detrend all variables using aggregate trends. Namely, we detrend a variable by removing the yearly mean across all layers and municipalities. If i refers to a specific municipality and t to year, $\tilde{x}_{it} = x_{it}/\bar{x}_t$ denotes the detrended variable, and $\bar{x}_t$ is the average of the variable across all municipalities and layers in year t. We then estimate the following equation:

$$d\,ln\,\tilde{x}_{it} = \alpha + \gamma_i + \epsilon_{it} \tag{3-1}$$

Where $d\,ln\,\tilde{x}_{it} = ln\,\tilde{x}_{it} - ln\,\tilde{x}_{it-1}$ denotes a yearly time difference for two consecutive years, ϵ_{it} is a disturbance term, and α is our regressor of interest.

We include municipality fixed effects γ_i to account for time-invariant factors at the municipal level.

Table 3.8 shows for all municipalities, the ones that add layers, the ones that do not change and the ones that drop layers, the average log changes in total contracts, total hours and hourly wages. As can be seen in the second column of the table, adding layers of knowledge is positively related with the change in total number of labor contracts signed by a municipality and with the change in total number of hours hired by a municipality. Moreover, changes in either detrended contracts or detrended hours seem to be of the same magnitude, but with opposite sign, for municipalities that drop layers (column four). Interestingly, we do not see any effects on average wages in those municipalities that initiate a process of reorganization of its bureaucratic structure. As already pointed out by Caliendo et al. (2015b) with French firms, these results are inconsistent with different theories of firm dynamics in which expansions (contractions) always lead to an increase (decrease) of the wages of all employees, but are not inconsistent with the knowledge-based theory. Nevertheless, the dynamics of wages in firms could be very different than the dynamics in the public sector due to distinct rigidities between sectors (e.g. labor unions, civil service examinations, public administration career schemes). At the same time, the results above suggests that this inconsistency is true on average, but they do not imply that this occur layer by layer.

To elucidate the aforementioned we perform a similar analysis but focusing on layer-level outcomes for municipalities that transition from L to L' layers of knowledge. We first present in tables 3.9 and 3.10 estimates of a regression of the log change in the number of contracts and hours, respectively, in layer l in two consecutive years on a constant, including municipality fixed effects. The tables show the number of layers before (column 1) and after the reorganization took place (column 2), as well as the specific layer for which the average log change is estimated (column 3). As before we present estimates with and without detrending ($d\,ln\,\tilde{x}^l_{it}$ and $d\,ln\,x^l_{it}$). Note again that according to the theory of knowledge-based hierarchies, adding layers should lead to an increase in employment in all preexisting layers, while dropping layers should produce the opposite reaction. These implications are also verified to some extent for most of the transitions of Brazilian municipal governments. The sign of the average change is most of the times positive and significant for municipalities that increase the number of layers, and negative and significant for those municipalities that drop one or more layers, with one exception highlighted in color red (at layer 3 for those municipalities that transition from 4 to 3 layers).

The theory of knowledge-based hierarchies also predicts that firms that add layers concentrate workers' knowledge on the top layers but reduce knowledge in all preexisting layers of the hierarchy. We present the results of the average log change in hourly wages when a municipality reorganize in table 3.11. Although not strongly as with contracts and hours, the estimates endorse the theoretical predictions. Municipalities that drop layers tend to increase wages in all preexisting layers as can be seen in most of the transitions described in the table. On the other hand, the sign of the average change for municipalities that add layers seems to go in the direction of the theory, yet the coefficients are not always significant. Several factors related to the hiring process in the public sector could explain this lack of downwards flexibility in average hourly wages. For instance, labor market regulations, a centralized wage-setting at the sector level, a binding minimum wage, among other, could implicitly prevent wage adjustments for new hires by fixing wages at the level of current workers of the organization. Still, all things considered, our findings for Brazilian municipalities reproduces layer by layer the main implications of the theory of Garicano (2000), Garicano and Rossi-Hansberg (2006), Caliendo and Rossi-Hansberg (2012), and its empirical applications in the private sector.

All the descriptive statistics and results shown so far are robust to excluding one layer municipalities, as well as to restricting the sample to municipalities within the 99th percentile of the annual change of wages and contracts. We now move to the next section to study whether a reorganization of the municipal bureaucracy that adds or drops layers correlates with an indicator of government capacity.

3.5 Hierarchies and government capacity

Brazilian municipalities are highly dependent on intergovernmental transfers from the states and federal governments. More than 60% of total local revenues are constitutional transfers and discretionary transfers from these two levels of government, while own sources of revenues, such as local taxes, represents on average less than 15% of total local revenues. Although discretionary federal grants account for a small share of total federal transfers, its importance for the financial well-being of local governments it is not negligible. For the period under analysis (2003-2012) it represents on average 4% of local revenues, and in some municipalities this number increases up to 18%. Unlike other transfers received by the municipality, discretionary federal grants are not formula based. This means that the receipt of these funds depends on the

effort and ability of local governments to solicit them.

When applying for discretionary federal grants, municipalities have to put together a competitive work plan detailing technical feasibility of the project, cost, stages and deadlines of execution (TCU, 2016). The document should be based on preliminary technical studies to ensure reliable inputs, considering that federal agencies base their approval and supervision on the information provided in the work plan. Deficiencies and irregularities in the proposition phase can lead to the rejection of the grant agreement. The most common causes of rejection reported by federal agencies are goals poorly described, budget overestimation or underestimation, and insufficient information about the needs of the funds. Furthermore, the local administration should proof the municipality is neither in a breach-of-contracts nor that has irregularities with other federal agencies (Brollo, 2011).

Typically the areas in which the resources are allocated are education, health, sanitation, road construction and maintanance, water supply, housing, and development of local economies. Hence, highly visible projects that can render electoral support could be financed through these grants (Ferraz and Finan, 2011). Table 3.12 provides descriptive information by granting ministry. On average, almost half of all municipalities gain access to at least one grant per year aimed to finance projects in the health area, and to expand populations' access to housing, sanitation and transportation (Ministry of Cities), while more than 85% of municipalities get access to the discretionary resources of any ministry.[9][10] If we compare the median number of grants provided per year by each ministry with the proportion of municipalities that received at least one grant, is clear that some municipalities are succesfully applying for more than one grant per ministry. This could reflect an accumulation of expertise or competency in ministry-specific grant application. Columns 4 and 6 highlights the scale of the grants in absolute terms and prorated per the median duration (column 5) of the grant agreement, respectively. We observe that two of the least common granting ministries (National Integration and Science and Technology) finance the largest scale projects.[11] The median annual grant value of these two ministries practically doubles the median value of a grant aggregating across all ministries. It is because grants are relatively

[9]In total we collected information from 28 ministries although we only report in the table statistics from those that funded more than 20% of our sample municipalities during the period under analysis. The empirical analysis that follows includes information of all 28 ministries.

[10]If we consider the entire period 2003-2012, almost all municipalities were granted resources from the ministries of Health (97%), Cities (93%) and Education (92%).

[11]The Ministry of National Integration is responsible for establishing strategies for the integration of regional economies, and for formulating and conducting the national irrigation policy, among other competencies.

large-scale that partly explains why not so many municipalities get access to the fundings from the Ministries of National Integration, and Science and Technology.

Since discretionary federal grants provide valuable resources to finance the public services that municipalities consider better suited for its local context, and that the access is not granted by any law, we believe that obtaining this type of funds can be used as a valid proxy of an indicator of local government capacity.

3.5.1 Number of layers and grants

We now present our empirical strategy to measure how the organization of the municipal bureaucracy in layers of knowledge correlates with discretionary federal grants. The empirical specification has as its unit of observation municipality i in year t. We run pooled and within (municipality) regression models, where the outcome variable Y_{it} is either the prorated log value of the grant into the years of its duration or the number of grants received by a municipality.[12] Our main regressor of interest is the number of knowledge layers of municipality i in year t. Throughout, we include year fixed effects (δ_t), state fixed effects (π_s), year-state fixed effects ($\delta_t \times \pi_s$), and second-order population polynomials ($f(pop)$). We cluster standard errors at the municipality level.

$$Y_{it} = \alpha + \beta\, Number\, of\, Layers_{it} + \delta_t + \pi_s + \delta_t \times \pi_s + f(pop_{i,t}) + \mu_{it} \quad (3\text{-}2)$$

Table 3.13 shows our main results on how the hierarchical structure of municipal bureaucracies correlates with an indicator of government capacity. In columns (-a) we consider all municipalities in our sample, assuming that one layer municipalities are supervised by the mayor, while in columns (-b) we restrict our sample to municipalities with two or more layers. We find that the number of layers is significantly correlated with both the value of the grants (odd columns) and the number of grants (even columns) received by a municipality. The log value of the total grants received in year t increases by between 4% and 11% when municipalities add a layer, whereas the number of grant-agreements signed increases by about 5% to 13%. Note how the estimated effect sizes are larger and significantly higher when we exclude one layer municipalities. Even assuming that in these municipalities mayors are the

[12] In the appendix we present results imputing the total value of the grant to the year in which the agreement was signed, instead of prorating the value across the years. Estimates are generally robust to this alternative measure of the outcome variable.

head of the hierarchy, it is reasonable to speculate that there was misreporting in the occupational classification of municipal employees. Hence, we regard the estimates in columns (-b) as our preferred ones.

In the Appendix we present a series of robustness checks to assess the stability of our main findings using an alternative sample, different model specifications, and imputing the total value of the grant to the year in which the grant agreement was subscribed. We first restrict our attention to municipalities within the 99th percentile of the annual change on hourly wages and contracts. Table C.2 shows that the partial correlation of interest (β) remains similar in terms of point estimates and significancy relative to our main specifications of table 3.13. This is also true when we include in the regression the lag value of the dependent variable (table C.3)—although, as expected, past values have a larger effect on the current value of the grants and the number of total grants than the number of hierarchical layers; when we control by the total number of bureaucrats in the organizational structure (table C.4)—instead of controlling by municipal population; or when we consider political (party) alignment between the federal and municipal governments (table C.5) to control for political party favoritism in the allocation of resources. We next document the robustness of our baseline results using an alternative measure of discretionary federal grants. Instead of prorating the value of the grant across the years of the agreement, we impute the total value to the year of its approval. Table (C.6) presents the results. Again, columns (-a) consider all municipalities in our sample whereas columns (-b) restrict the sample to municipalities with two or more layers. Moreover, we also present estimates restricting the sample to municipalities within the 99th percentile of the annual change of wages and contracts in the columns headed by $ln\,(grants\,II)$. As can be seen, although weaker for the within estimates, the number of layers also correlates positively with this alternative measure of discretionary federal grants.

We further examine the partial correlation of municipalities' hierarchies with our proxy of capacity disaggregating the grants by ministry. Tables C.7 and C.8 in the Appendix present within estimates of the log value of the prorated grants and the number of grants, respectively. As before, in columns (-a) we study all municipalities in our sample while in columns (-b) we restrict our attention to municipalities with two or more layers. First, notice that the number of knowledge layers positively correlates with the value and number of grants of almost all ministries, as the point estimates are positive in most cases. Second, and more importantly, our main results seem to be driven by the access to the grants of the Ministry of Cities and the Ministry of National Integration. Ministries which, a priori, finance technically complex projects

such as distribution networks of gas, sewage and water, and projects related to the economic development of municipalities. We also find in specific cases significant and positive correlations with the ministries of Education, Social Development and Tourism.

Taken together, this descriptive evidence depicts a very consistent picture that suggests that the organization of the municipal bureaucracy in layers of knowledge can be importantly related to an indicator of government capacity.

Span of control. Another relevant measure of the structure of an organization is the span of control of principals (i.e. the number of agents under principals' supervision). Standard models of delegation suggest that the devolution of decision-making powers to agents (i.e. increase principals' span of control) allow the organization to leverage the *initiative effect* of their agents associated with a reduction in oversight (Aghion and Tirole, 1997). To complement our analysis of municipal knowledge-based hierarchies, we analyze whether bureaucrats' autonomy affects the correlation between layers of knowledge and discretarionary federal grants. For that, we compute the span of control as the number of agents per each principal in the organization (Caliendo and Rossi-Hansberg, 2012). For a municipality with $L = l$ layers, we count the number of employees at layer $l - 1$—agents—and at layer l—principals, and compute the span of control as the ratio of the number of employees $l - 1$ to l. We compute the span of control of the highest layer, instead of averaging, for instance, the span of control across all adjacent layers as done by Tåg et al. (2016), to avoid extreme variations in our measure of bureaucrats' autonomy. In equation 3-2, therefore, we interact the number of knowledge layers of the municipality with a measure of the span of control of its highest-rank principals. We express the span of control as an indicator of above-below the yearly median distribution of sample municipalities.

Table 3.14 presents the results. The positive and significant coefficients of the interaction term indicates that the correlation of the number of layers with discretionary federal grants is larger, the larger the span of control of the highest layer. Appendix table C.9 shows the robustness of the result to the sample of municipalities within the 99th percentile of the annual change of wages and contracts. Although these are partial correlations, the finding suggests that bureaucrats' autonomy could be a relevant dimension of local governments' capacity. A result that goes along the same lines as the findings of Rasul and Rogger (2017). Using a management survey of the Nigerian Civil Service they find that management practices related to bureaucrats' autonomy are positively correlated with bureaucratic output, whereas practices related to monitoring of bureaucrats are negatively associated.

3.6 Conclusion

We provided, as far as we know, the first empirical application of the knowledge-based hierarchies theory in a public sector context. We did so using a detailed dataset of labor occupations within Brazilian municipal governments over the period 2003-2012. Our analysis has identified clear patterns in the data and robust correlations that match the theoretical predictions: i) Brazilian municipalities have a hierarchical bureaucratic structure in which top layers are made up of a smaller number of bureaucrats earning higher wages than bottom layers; ii) bureaucratic reorganizations that add (drop) layers of knowledge increase (decrease) the number of workers in each layer but decrease (increase) the average wage in each preexisting layer.

Besides providing the first anatomy of the internal organization of Brazilian municipal bureaucracies, we attempted to bring new evidence to the literature of state effectiveness. In particular, we showed how the organization of the municipality in layers of knowledge correlates with an indicator of government capacity. Municipalities that reorganize its bureaucratic structures by adding a new layer of bureaucracy increase the value of the total discretionary grants received from the federal government by about 4% to 11%. Our findings highlight the potential effects that organizational actions and decisions can have on public sector capacity.

Tables

Table 3.1: Occupational classification, skill level and rank

Occupation Classification	Skill Level	CBO2002	Rank
Senior Government Officials	NA	1112-	4
Directors and Managers	NA	1114-, 1210-, 122-, 123-, 131-, 141-, 142-	3
Professionals	4	200-	2
Technicians	3	300-	2
Clerks	2	400-	1
Service Workers	2	500-	1
Production Workers	2	700-	1
Agricultural and Fishery Workers,	2	600-	Omitted
Plant and machine operators and assemblers	2	800-	Omitted
Elementary Workers	2	900-	Omitted
Armed Forces	NA	000-	Omitted

Notes: Skill level refers to the level of competency according to CBO2002. Rank indicates our classification based on employee's knowledge and authority. In each occupation classification we do not include occupations associated with neither education provision nor health provision. The occupation classification Production Workers only includes car drivers and urban bus drivers.

Table 3.2: Distribution of contracts, hours and wages by rank

	Rank			
Variables	4	3	2	1
Contracts (year)				
mean	9	57	64	287
sd	33	271	154	498
p10	1	2	5	39
p25	2	6	12	74
p50	5	14	25	148
p75	9	38	57	308
p90	16	99	131	623
Hours hired (year)				
mean	17,380	102,961	119,366	545,851
sd	64,109	473,902	295,177	926,828
p10	2,054	3,852	8,765	72,229
p25	2,260	10,272	21,571	142,473
p50	8,218	26,022	47,251	283,798
p75	16,435	69,336	107,171	588,586
p90	29,190	183,800	246,562	1,200,000
Wage (hour)				
mean	17.62	13.54	11.08	5.77
sd	17.40	15.42	10.01	4.15
p10	5.97	5.33	4.65	3.58
p25	9.25	7.42	6.19	4.13
p50	13.89	10.72	9.30	5.05
p75	20.82	15.70	13.65	6.38
p90	31.28	22.87	18.53	8.23

Notes: The table reports for each rank category, mean, standard deviation and percentiles of the selected variable across all municipalities and years in the data. One observation in rank r is the average of the selected variable in a given municipality-year from RAIS, conditional on the municipality reporting an occupation in rank r. Contracts is the number of annual contracts from RAIS. Hours is the number of total annual hours from RAIS. Wage is the hourly wage from RAIS in 2012 reais.

Table 3.3: Data description by year - Mean

		Mean				
Year	Munic-Year	Population	Contracts	Hours	Wage	N Layers
2003	5,281	25,471	292.40	551,104	7.66	3.00
2004	5,313	25,962	301.26	568,863	7.37	3.03
2005	5,381	26,085	326.71	618,555	6.95	3.12
2006	5,424	26,205	350.29	665,979	7.21	3.16
2007	5,426	25,737	372.53	706,879	7.30	3.20
2008	5,413	26,638	422.41	799,367	7.13	3.21
2009	5,427	26,888	447.99	846,718	7.21	3.26
2010	5,457	26,705	460.62	872,206	7.08	3.27
2011	5,455	26,889	478.42	901,507	6.75	3.29
2012	5,456	27,031	469.42	881,593	6.76	3.28
Total	5,532	26,383	395.50	747,488	7.13	3.19

Notes: The table reports for each year, the number of municipalities in the data set and the corresponding average across all municipalities for selected variables. Contracts is the average number of annual contracts from RAIS. Hours is the average number of total annual hours from RAIS. Wage is the average hourly wage from RAIS in 2012 reais. N Layers is the average number of layers across municipalities in each year.

Table 3.4: Percentage of municipalities that have consecutively ordered layers

	1 Layers	2 Layers	3 Layers	4 Layers	All Munic
Ordered Layers	77.38	86.15	94.73	100.00	93.96

Notes: The table reports the fraction of municipalities with consecutively ordered layers conditioning on the number of layers in the municipality (first four columns) and overall (fifth column). A municipality of 1 (2) [3] {4} layers has consecutively ordered layers if reports an occupation in rank 1 (1 and 2) [1, 2 and 3] {1, 2, 3 and 4}.

Table 3.5: Distribution of contracts, hours and wages by number of layers

Panel A	Mean Contracts (year)				Median Contracts (year)			
N Layers	Layer 1	Layer 2	Layer 3	Layer 4	Layer 1	Layer 2	Layer 3	Layer 4
2	227	57	-	-	117	12	-	-
3	283	67	46	-	145	27	13	-
4	305	68	40	8	168	28	13	5
Panel B	Mean Hours (year)				Median Hours (year)			
N Layers	Layer 1	Layer 2	Layer 3	Layer 4	Layer 1	Layer 2	Layer 3	Layer 4
2	421,872	104,486	-	-	217,510	21,571	-	-
3	542,751	124,970	83,974	-	278,097	50,470	24,653	-
4	578,694	125,679	73,506	15,553	324,338	52,400	24,653	8,218
Panel C	Mean Wage (hour)				Median Wage (hour)			
N Layers	Layer 1	Layer 2	Layer 3	Layer 4	Layer 1	Layer 2	Layer 3	Layer 4
2	5.92	10.91	-	-	4.89	8.13	-	-
3	5.80	11.35	14.41	-	5.13	9.66	11.53	-
4	5.49	10.49	11.99	17.92	4.89	8.86	9.51	14.14

Notes: Panel A reports the average (and median) number of total annual contracts by number of layers in each layer. Panel B reports the average (and median) number of total annual hours by number of layers in each layer. Panel C reports the average (and median) hourly wage by number of layers in each layer. The layer numbers do not necessarily correspond to the rank numbers used in previous tables. For example, a municipality with one government official and one clerk will have two layers and consist of workers in ranks 4 and 1. This municipality will appear in the same cell as a municipality consisting of one manager and one professional (ranks 3 and 2).

Table 3.6: Percentage of municipalities that satisfy a hierarchy in contracts, hours and wages

Panel A	Contracts (year)		
N Layers	Layer 1 > Layer 2	Layer 2 > Layer 3	Layer 3 > Layer 4
2	91.53	-	-
3	94.41	71.23	-
4	95.80	73.37	79.22
Panel B	Hours (year)		
N Layers	Layer 1 > Layer 2	Layer 2 > Layer 3	Layer 3 > Layer 4
2	91.76	-	-
3	94.66	70.59	-
4	95.90	72.99	77.87
Panel C	Wage (Hour)		
N Layers	Layer 1 < Layer 2	Layer 2 < Layer 3	Layer 3 < Layer 4
2	83.67	-	-
3	93.59	66.26	-
4	94.11	57.81	76.02

Notes: Panel A (B) reports the fraction of municipalities that satisfy a hierarchy in contracts (hours) between layer l and $l + 1$. A municipality satisfies a hierarchy in contracts (hours) between layer l and $l + 1$ in a given year if the number of total annual contracts (hours) in layer l is at least as large as the number of total annual contracts (hours) in layer $l + 1$. Panel C reports the fraction of municipalities that satisfy a hierarchy in wages between layer l and $l + 1$. A municipality satisfies a hierarchy in wages between layer l and $l + 1$ in a given year if the average hourly wage in layer l is at most as large as the average hourly wage in layer $l + 1$.

Table 3.7: Distribution of layers at $t+1$ conditional on layers at t

	N Layers at *t+1*				
N Layers at t	1	2	3	4	Total
1	**69.14**	15.00	12.43	3.42	100
2	8.00	**71.46**	17.55	2.99	100
3	1.29	5.57	**87.64**	5.51	100
4	0.91	2.67	19.03	**77.40**	100

Notes: The table reports the distribution of the number of layers at time *t+1*, grouping municipalities according to the number of layers at time *t*. Among municipalities with L layers ($L = 1, 2, 3, 4$) in any year from 2003 to 2011, the columns reports the fraction of municipalities that have L layers ($L = 1, 2, 3, 4$) the following year (from 2004 to 2012). The elements in the table sum to 100% by row.

Table 3.8: Changes in municipality-level outcomes

		Number of Layers		
Variables	All	Increase	No Change	Decrease
d ln (total contracts)	0.057***	0.224***	0.052***	-0.127***
	(0.002)	(0.023)	(0.002)	(0.034)
- Detrended	0.004*	0.165***	-0.000	-0.180***
	(0.002)	(0.023)	(0.002)	(0.034)
d ln (total hours)	0.059***	0.237***	0.052***	-0.120***
	(0.003)	(0.013)	(0.002)	(0.035)
- Detrended	0.006**	0.178***	0.000	-0.174***
	(0.002)	(0.024)	(0.002)	(0.035)
d ln (total wages)	-0.007***	-0.006	-0.008***	-0.002
	(0.001)	(0.008)	(0.001)	(0.010)
- Detrended	0.006***	0.009	0.005***	0.012
	(0.001)	(0.008)	(0.001)	(0.010)
% of Municipalities	100.00	10.46	82.44	7.10

Notes: The table reports changes in municipality-level outcomes between consecutive years for all municipalities, and for those that increase, do not change, and decrease the number of layers. It displays changes in log annual contracts, log annual hours, and log average hourly wage. We detrend a variable by removing from it the yearly mean across all layers and municipalities. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.9: Average log change in contracts for municipalities that transition

Layers Before	Layers After	Layer	$d\,ln\,x_{it}$	SE	$d\,ln\,\tilde{x}_{it}$	SE	N
1	2	1	0.118	(0.072)	0.056	(0.072)	294
1	3	1	0.230*	(0.126)	0.170	(0.127)	225
1	4	1	0.396	-	0.342	-	54
2	1	1	0.065	(0.110)	0.008	(0.110)	217
2	3	1	0.122***	(0.023)	0.062***	(0.023)	1,218
2	3	2	0.291***	(0.030)	0.231***	(0.030)	1,218
2	4	1	0.164***	(0.063)	0.102	(0.064)	212
2	4	2	0.625***	(0.091)	0.563***	(0.090)	212
3	1	1	-0.107	(0.148)	-0.161	(0.148)	161
3	2	1	-0.024	(0.030)	-0.079***	(0.030)	736
3	2	2	-0.186***	(0.042)	-0.242***	(0.042)	736
3	4	1	0.096***	(0.014)	0.039***	(0.014)	1,646
3	4	2	0.167***	(0.019)	0.111***	(0.019)	1,646
3	4	3	0.142***	(0.024)	0.085***	(0.024)	1,646
4	1	1	-0.869**	(0.390)	-0.928**	(0.390)	43
4	2	1	-0.168	(0.124)	-0.226*	(0.123)	109
4	2	2	-0.547***	(0.155)	-0.605***	(0.154)	109
4	3	1	-0.028	(0.019)	-0.080***	(0.019)	1,228
4	3	2	-0.025	(0.024)	-0.077***	(0.024)	1,228
4	3	3	0.133***	(0.032)	0.081**	(0.032)	1,228

Notes: The table reports changes in log annual contracts between consecutive years, at each layer l among municipalities that transition from L to L' layers. It displays non-detrended log change in the transition ($d\,ln\,x_{it}$) and detrended log change in the transition ($d\,ln\,\tilde{x}_{it}$). The table uses all observed transitions in the sample among municipalities that have consecutively ordered layers. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.10: Average log change in hours for municipalities that transition

Layers Before	Layers After	Layer	$d\,ln\,x_{it}$	SE	$d\,ln\,\tilde{x}_{it}$	SE	N
1	2	1	0.131*	(0.072)	0.068	(0.072)	294
1	3	1	0.313**	(0.128)	0.252**	(0.128)	225
1	4	1	0.466	-	0.411	-	54
2	1	1	0.062	(0.111)	0.004	(0.111)	217
2	3	1	0.130***	(0.023)	0.070***	(0.023)	1,218
2	3	2	0.301***	(0.031)	0.240***	(0.031)	1,218
2	4	1	0.159**	(0.068)	0.096	(0.068)	212
2	4	2	0.597***	(0.090)	0.534***	(0.090)	212
3	1	1	-0.112	(0.150)	-0.166	(0.150)	161
3	2	1	-0.005	(0.031)	-0.062**	(0.031)	736
3	2	2	-0.175***	(0.043)	-0.231***	(0.043)	736
3	4	1	0.104***	(0.015)	0.047***	(0.015)	1,646
3	4	2	0.170***	(0.020)	0.113***	(0.020)	1,646
3	4	3	0.154***	(0.026)	0.098***	(0.026)	1,646
4	1	1	-0.873**	(0.390)	-0.932**	(0.390)	43
4	2	1	-0.206*	(0.125)	-0.264**	(0.124)	109
4	2	2	-0.557***	(0.159)	-0.614***	(0.158)	109
4	3	1	-0.021	(0.019)	-0.073***	(0.019)	1,228
4	3	2	-0.019	(0.024)	-0.070***	(0.024)	1,228
4	3	3	0.138***	(0.033)	0.086***	(0.033)	1,228

Notes: The table reports changes in log annual hours between consecutive years, at each layer l among municipalities that transition from L to L' layers. It displays non-detrended log change in the transition ($d\,ln\,x_{it}$) and detrended log change in the transition ($d\,ln\,\tilde{x}_{it}$). The table uses all observed transitions in the sample among municipalities that have consecutively ordered layers. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.11: Average log change in wages for municipalities that transition

Layers Before	Layers After	Layer	$d\,ln\,x_{it}$	SE	$d\,ln\,\tilde{x}_{it}$	SE	N
1	2	1	-0.053***	(0.017)	-0.037**	(0.017)	294
1	3	1	-0.204***	(0.028)	-0.187***	(0.028)	225
1	4	1	-0.155	-	-0.142	-	54
2	1	1	0.047*	(0.028)	0.059**	(0.028)	217
2	3	1	-0.030***	(0.009)	-0.013	(0.009)	1,218
2	3	2	-0.017	(0.012)	-0.000	(0.012)	1,218
2	4	1	-0.010	(0.021)	0.011	(0.021)	212
2	4	2	0.061*	(0.034)	0.082**	(0.034)	212
3	1	1	0.174***	(0.035)	0.193***	(0.035)	161
3	2	1	0.011	(0.011)	0.027**	(0.011)	736
3	2	2	-0.024	(0.016)	-0.008	(0.016)	736
3	4	1	-0.008	(0.007)	0.005	(0.007)	1,646
3	4	2	-0.006	(0.010)	0.008	(0.010)	1,646
3	4	3	-0.080***	(0.011)	-0.067***	(0.011)	1,646
4	1	1	0.144	(0.094)	0.160*	(0.094)	43
4	2	1	0.117***	(0.034)	0.127***	(0.034)	109
4	2	2	0.090	(0.060)	0.100*	(0.059)	109
4	3	1	0.003	(0.006)	0.016**	(0.006)	1,228
4	3	2	-0.015	(0.010)	-0.002	(0.010)	1,228
4	3	3	0.022*	(0.011)	0.035***	(0.011)	1,228

Notes: The table reports changes in log hourly wage between consecutive years, at each layer l among municipalities that transition from L to L' layers. It displays non-detrended log change in the transition ($d\,ln\,x_{it}$) and detrended log change in the transition ($d\,ln\,\tilde{x}_{it}$). The table uses all observed transitions in the sample among municipalities that have consecutively ordered layers. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.12: Descriptive information of discretionary federal grants by ministry

		Median			
Ministry	Proportion of Municipalities	Number of Grants	Value (R$)	Duration (years)	Yearly Value (R$)
Health	47.7%	4873	$ 321,286.4	4	$ 80,321.6
Cities	44.5%	4324	$ 487,038.8	4	$ 121,759.7
Education	33.6%	3297	$ 221,442.7	2	$ 110,721.4
Tourism	31.6%	2934	$ 393,877.7	4	$ 98,469.4
Sports	21.5%	1605	$ 340,797.5	4	$ 85,199.4
Agriculture	21.5%	1584	$ 257,343.5	3	$ 85,781.2
Social Development	16.1%	1231	$ 130,958.8	3	$ 43,652.9
National Integration	13.2%	935	$ 807,880.2	3	$ 269,293.4
Rural Development	11.6%	792	$ 160,982.1	3	$ 53,660.7
Science and Technology	5.9%	465	$ 823,143.5	4	$ 205,785.9
All	86.6%	21958	$ 483,682.0	3.8	$ 126,895.8

Notes: The table reports descriptive information of discretionary federal grants by granting ministry for the period 2003-2012. The second column presents the yearly mean proportion of municipalities that received at least one grant by the ministry. The third column indicates the median of the total number of grants enacted by the ministry per year. The fourth column reports the median of the total value of each grant enacted by the ministry. The fifth column presents the median years of duration of the grants enacted by the ministry. The last column reports the median yearly value of the grants (column 4 divided by column 5). All monetary values are expressed in 2012 R$. We do not report descriptive information of those ministries in which less than 20% of our sample of municipalities got a grant during the entire period 2003-2012.

Table 3.13: Discretionary federal grants estimates

	Pooled				Within			
	ln (grants)		quantity		*ln* (grants)		quantity	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Number of Layers	0.087***	0.109***	0.114***	0.135**	0.038*	0.068**	0.050*	0.098**
	(0.020)	(0.026)	(0.042)	(0.063)	(0.021)	(0.028)	(0.029)	(0.039)
Observations	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460
Number of Fixed Effects	-	-	-	-	4,870	4,146	4,870	4,146
R2	0.265	0.276	0.387	0.391	0.143	0.151	0.165	0.167

Notes: Pooled and within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. *ln* (grants) represents the log value of the grant, smoothed across the number of years of the grant agreement. Quantity represents the number of grant agreements signed by the municipality in year t. All regressions include year fixed effects, state fixed effects, year-state fixed effects, and second order population polynomials. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.14: Discretionary federal grants estimates - Span of control

	Pooled				Within			
	ln (grants)		quantity		*ln* (grants)		quantity	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Number of Layers	0.004	0.002	-0.001	-0.022	-0.022	0.001	0.002	0.013
	(0.035)	(0.042)	(0.064)	(0.077)	(0.035)	(0.043)	(0.046)	(0.057)
Span of Control	-0.331***	-0.494***	-0.459**	-0.745**	-0.270**	-0.362**	-0.198	-0.424*
	(0.128)	(0.165)	(0.233)	(0.326)	(0.122)	(0.156)	(0.170)	(0.229)
Number of Layers X Span of Control	0.120***	0.172***	0.165**	0.251**	0.078**	0.104**	0.067	0.130*
	(0.040)	(0.050)	(0.074)	(0.101)	(0.038)	(0.048)	(0.054)	(0.072)
P-Value	0.000	0.000	0.001	0.005	0.015	0.001	0.046	0.003
Observations	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460
Number of Fixed Effects	-		-		4,870	4,146	4,870	4,146
R2	0.265	0.276	0.387	0.391	0.143	0.152	0.165	0.167

Notes: Pooled and within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t interacted with the span of control of the highest layer. Span of control is defined as number of employees in layer $l-1$ divided by the number of employees in layer l. We express the span of control as an indicator of above/below (1/0) the yearly median distribution of sample municipalities. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. *ln* (grants) represents the log value of the grant, smoothed across the number of years of the grant agreement. Quantity represents the number of grant agreements signed by the municipality in year t. All regressions include year fixed effects, state fixed effects, year-state fixed effects and second order population polynomials. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; p-value of (Number of Layers + Number of Layers X Span of Control) $\neq 0$ is reported in the bottom part of the table.

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A
Chapter 1

A.1
Additional results

Demand-for-Research Experiment

Table A.1: Sample selection - Municipalities

Variables	Non-Participants	Participants	Difference	p-value
Male (Mayor)	86.80	87.31	0.51	0.77
Age (Mayor)	47.87	48.17	0.30	0.59
College or more (Mayor)	53.98	60.36	6.37**	0.01
2nd Term (Mayor)	22.02	21.38	-0.64	0.76
Leftist Political Party (Mayor)	33.29	39.64	6.36**	0.01
Population (2016 - Thousands)	28.14	25.29	-2.85	0.44
College Population (2010)	5.13	4.99	-0.14	0.33
Public Adm College (2016)	32.20	34.06	1.86**	0.01
Poverty (2010)	24.95	26.24	1.28	0.17
Gini (2010)	49.86	49.85	-0.00	0.99
Kids in School (0-3, 2010)	18.73	19.77	1.04*	0.08
Kids in School (4-5, 2010)	77.93	79.26	1.34*	0.10
Big South	45.09	44.32	-0.77	0.77
Per Capita Income (2010)	474.72	467.08	-7.64	0.54
Joint F-test				0.00

Notes: Sample mean of participants' municipalities and non-participants' municipalities and differences in means between those groups. Male-Leftist political party, are characteristics of the mayor that runs the municipality. College population indicates the share of adults with college degree. Public administration college indicates the share of municipal public employees with college degree. Poverty refers to municipalities' poverty rate. Gini refers to the Gini coefficient of municipality. Kids in school (0-3) indicates the share of children 0-3 years old that attend pre-K education. Kids in school (4-5) indicates the share of children 4-5 years old that attend pre-K education. Big south indicates the share of municipalities from the south, southeast and mid-west regions; and 0 are north and northeast regions. Per capita income indicates municipality monthly income per capita. p-value of mean-comparison t-tests between groups for full sample, and the joint significance F-test p-value. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Willingness to pay - Study characteristics - Same experimental design

VARIABLES	(1) WTP	(2) WTP	(3) WTP
Large	4.1066***	3.4787	4.3478***
	(0.7440)	(2.6063)	(1.0160)
Developing	0.3895	2.3357	-0.3308
	(0.7436)	(2.6082)	(1.0038)
Observations	2,417	605	1,812
Round	1 and 2	1	2
Clusters	605	605	605
Mean LHS	43.87	46.13	43.12

Notes: OLS results. The dependent variable is willingness to pay, which is elicited in two different rounds. Developing is a dummy which is equal to one for Jamaica and Colombia and zero otherwise. Large is a dummy which is equal to one for Colombia and US and zero otherwise. Mean LHS is the mean WTP on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Willingness to pay - Study characteristics - Continuous sample size

VARIABLES	(1) WTP	(2) WTP	(3) WTP	(4) WTP	(5) WTP	(6) WTP
ln(Sample Size)	1.2989***	0.8137	1.4899***			
	(0.2574)	(0.7830)	(0.3259)			
Developing	1.0788	2.0487	0.5209	1.7317**	2.4650	1.2627
	(0.7976)	(2.4568)	(1.0166)	(0.8237)	(2.5717)	(1.0493)
(Sample Size)^ 1/2				0.0910***	0.0573	0.1040***
				(0.0177)	(0.0542)	(0.0222)
Observations	2,578	766	1,812	2,578	766	1,812
Round	1 and 2	1	2	1 and 2	1	2
Clusters	766	766	605	766	766	605
Mean LHS	44.73	48.52	43.12	44.73	48.52	43.12

Notes: OLS results. The dependent variable is willingness to pay, which is elicited in two different rounds. Developing is a dummy which is equal to one for Jamaica and Colombia and zero otherwise. The sample size of the studies is expressed continuously instead of binary. Mean LHS is the mean WTP on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Willingness to pay - Other determinants

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

Notes: OLS results. The dependent variable is willingness to pay, which is elicited in two different rounds. We expressed all continuous variables as indicators of above/below the median of the distribution of municipalities. Mayors' characteristics: Male (1/0); Age above-below median (1/0); College (1/0); 2nd term (1/0); Leftist Political Party (1/0). Participants' 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. Municipalities' characteristics: Pre-K 0-3 above-below median (1/0) of the share of kids 0-3 years old that attend pre-K education; Pre-K 4-5 above-below median (1/0) of the share of kids 4-5 years old that attend pre-K education; Population above-below median (1/0); College population above-below median (1/0); College public administration above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Income per capita above-below median; Big south (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions). Mean LHS is the mean WTP on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Belief updating - Weight placed on study result - Same experimental design

VARIABLES	(1) Posterior	(2) Posterior
Prior	0.5704***	0.5512***
	(0.0327)	(0.0302)
Signal	0.3995***	0.4166***
	(0.0296)	(0.0301)
Observations	544	544
Round	1	1
Context	Municipality	Random Study
Clusters	544	544
Mean LHS	0.430	0.430

Notes: OLS results. The dependent variables is posterior beliefs, which are declared after successfully buying the results from a study in each round. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. Context is either the respondent's own municipality (columns 1) or one of the four possible studies (column 4). Mean LHS is the average posterior belief of the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Belief updating - Heterogeneous effects

VARIABLES	(1) Post	(2) Post	(3) Post	(4) Post	(5) Post	(6) Post	(7) Post	(8) Post	(9) Post	(10) Post	(11) Post	(12) Post	(13) Post	(14) Post	(15) Post	(16) Post	(17) Post	(18) Post	(19) Post
Characteristic	Male	Age	College	2nd Term	Leftist Party	Mayor	Prof Politician	Leftist Scale	Implem ECD	Heard ECD	Pre-K 0-3	Pre-K 4-5	Pop	College pop	College Adm	Poverty	Gini	Income pc	Big South
Prior	0.6098***	0.6462***	0.6491***	0.6732***	0.6677***	0.6617***	0.6665***	0.6796***	0.6560***	0.6758***	0.6644***	0.6599***	0.6670***	0.6510***	0.6483***	0.6377***	0.6408***	0.6697***	0.6629***
	(0.0267)	(0.0247)	(0.0236)	(0.0228)	(0.0236)	(0.0243)	(0.0227)	(0.0233)	(0.0248)	(0.0231)	(0.0238)	(0.0243)	(0.0250)	(0.0233)	(0.0240)	(0.0244)	(0.0243)	(0.0236)	(0.0239)
Signal	0.3554***	0.3379***	0.3556***	0.3227***	0.3245***	0.3391***	0.3320***	0.3169***	0.3418***	0.3293***	0.3400***	0.3076***	0.3125***	0.3321***	0.3633***	0.3395***	0.3383***	0.3389***	0.3464***
	(0.0502)	(0.0259)	(0.0242)	(0.0207)	(0.0224)	(0.0235)	(0.0210)	(0.0213)	(0.0257)	(0.0220)	(0.0241)	(0.0244)	(0.0256)	(0.0224)	(0.0229)	(0.0268)	(0.0251)	(0.0222)	(0.0228)
Signal*Characteristic	-0.0206	-0.0219	-0.0565**	-0.0256	-0.0071	-0.0373	-0.0419	0.0050	-0.0451*	-0.0315	-0.0333	0.0171	0.0056	-0.0158	-0.0892***	-0.0304	-0.0254	-0.0383	-0.0462*
	(0.0465)	(0.0257)	(0.0258)	(0.0411)	(0.0269)	(0.0256)	(0.0295)	(0.0335)	(0.0263)	(0.0306)	(0.0264)	(0.0256)	(0.0263)	(0.0269)	(0.0252)	(0.0261)	(0.0254)	(0.0267)	(0.0257)
Signal*Developing*Characteristic	0.0183*	0.0220	0.0228*	0.0430*	0.0136	0.0169	0.0249	0.0207	0.0342**	0.0086	0.0084	0.0279*	0.0265*	0.0195	0.0248*	0.0406***	0.0312**	0.0010	0.0035
	(0.0110)	(0.0136)	(0.0138)	(0.0255)	(0.0177)	(0.0132)	(0.0195)	(0.0247)	(0.0167)	(0.0211)	(0.0147)	(0.0155)	(0.0161)	(0.0141)	(0.0143)	(0.0153)	(0.0148)	(0.0140)	(0.0136)
Signal*Large*Characteristic	0.0539***	0.0412***	0.0369***	0.0237	0.0289*	0.0309**	0.0361**	0.0098	0.0357**	0.0218	0.0285**	0.0289**	0.0157	0.0454***	0.0509***	0.0426***	0.0424***	0.0265**	0.0321**
	(0.0108)	(0.0134)	(0.0122)	(0.0202)	(0.0153)	(0.0126)	(0.0180)	(0.0198)	(0.0147)	(0.0187)	(0.0129)	(0.0133)	(0.0138)	(0.0130)	(0.0135)	(0.0137)	(0.0136)	(0.0134)	(0.0133)
Observations	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188	1,188
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	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	1 and 2
Clusters	702	702	702	702	702	702	702	702	702	702	702	702	702	702	702	702	702	702	702
Mean LHS	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442	0.442

Notes: OLS results. The dependent variables is posterior beliefs, which are declared after successfully buying the results from a study in each round. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. Mayors' characteristics: Male (1/0); Age above-below median (1/0); College (1/0); 2nd term (1/0); Leftist Political Party (1/0). Participants' characteristics: Mayor (1/0); Professional Politician (1/0); Leftist Scale (1/0); Implemented ECD (1/0); Heard ECD (1/0). Municipalities' characteristics: Pre-K 0-3 above-below median (1/0) of the share of kids 0-3 years old that attend pre-K education; Pre-K 4-5 above-below median (1/0) of the share of kids 4-5 years old that attend pre-K education; Population above-below median (1/0); College population above-below median (1/0); College public administration above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Income per capita above-below median; Big south (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions). Mean LHS is the mean WTP on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Willingness to pay - Heterogeneous effects

VARIABLES	(1) WTP	(2) WTP	(3) WTP	(4) WTP	(5) WTP	(6) WTP	(7) WTP	(8) WTP	(9) WTP	(10) WTP	(11) WTP	(12) WTP	(13) WTP	(14) WTP	(15) WTP	(16) WTP	(17) WTP	(18) WTP	(19) WTP
Characteristic	Male	Age	College	2nd Term	Leftist Party	Mayor	Prof Politician	Leftist Scale	Implem ECD	Heard ECD	Pre-K 0-3	Pre-K 4-5	Pop	College pop	College Adm	Poverty	Gini	Income pc	Big South
Developing	-2.0812	1.0253	1.2470	0.2397	-0.7998	-0.3913	0.8585	-0.2625	0.2404	0.6399	-0.1640	-0.2799	-0.8271	-0.4852	0.0796	0.5975	-0.3971	0.4401	0.9758
	(2.1384)	(1.2325)	(1.3008)	(0.8830)	(0.9945)	(1.1552)	(0.9450)	(0.8940)	(1.0091)	(0.8909)	(1.1479)	(1.1877)	(1.0785)	(1.0980)	(1.1180)	(1.1260)	(1.1205)	(1.0983)	(1.1599)
Large	3.4874	4.0711***	2.0693*	3.5981***	3.1840***	3.4803***	3.2981***	3.7320***	4.7328***	3.6929***	3.1318***	4.3979***	1.7614	3.4702***	2.8722***	4.1586***	2.8991**	3.5828***	2.9713***
	(2.3844)	(1.2125)	(1.2330)	(0.8899)	(0.9622)	(1.1252)	(0.9891)	(0.8899)	(1.0019)	(0.9141)	(1.0564)	(1.1568)	(1.0782)	(1.1355)	(1.0895)	(1.1553)	(1.1599)	(1.0980)	(1.1294)
Characteristic	4.4693	-0.6160	0.5626	1.8386	-0.4330	-1.1998	-0.2966	-1.0289	9.7099***	0.8949	1.1546	2.8912	-0.8278	-2.2595	0.0825	2.6337	-0.8755	-1.8491	-2.3063
	(3.6320)	(2.4187)	(2.4322)	(2.9275)	(2.4607)	(2.4044)	(2.6521)	(2.9255)	(2.4549)	(2.7822)	(2.4007)	(2.4012)	(2.4036)	(2.4064)	(2.4060)	(2.4083)	(2.4251)	(2.4111)	(2.4014)
Developing*Characteristic	2.7479	-1.2442	-1.5741	0.5644	2.8668*	1.4231	-1.6746	2.7294	0.2894	-1.1933	1.0549	1.1535	2.2911	1.7492	0.5487	-0.5125	1.3176	-0.2788	-1.3266
	(2.3010)	(1.6003)	(1.6327)	(1.9851)	(1.6320)	(1.5814)	(1.7199)	(1.9109)	(1.6170)	(1.9038)	(1.5763)	(1.5880)	(1.5749)	(1.5811)	(1.5827)	(1.5798)	(1.5742)	(1.5816)	(1.5812)
Large*Characteristic	0.3140	-0.5604	2.9173*	0.9672	1.4874	0.5691	1.4991	0.1784	-2.3142	0.2797	1.3436	-1.1933	3.9532**	0.6823	1.8489	-0.6972	1.5775	0.4216	1.6210
	(2.5273)	(1.5978)	(1.6045)	(1.9387)	(1.6679)	(1.5834)	(1.6260)	(1.9402)	(1.6217)	(1.8283)	(1.5873)	(1.5836)	(1.5742)	(1.5765)	(1.5822)	(1.5866)	(1.5849)	(1.5822)	(1.5820)
Observations	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578	2,578
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	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	1 and 2
Clusters	766	766	766	766	766	766	766	766	766	766	766	766	766	766	766	766	766	766	766
Mean LHS	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73	44.73

Notes: OLS results. The dependent variable is willingness to pay, which is elicited in two different rounds. 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. Mayors' characteristics: Male (1/0); Age above-below median (1/0); College (1/0); 2nd term (1/0); Leftist Political Party (1/0). Participants' characteristics: Mayor (1/0); Professional Politician (1/0); Leftist Scale (1/0) Implemented ECD (1/0); Heard ECD (1/0). Municipalities' characteristics: Pre-K 0-3 above-below median (1/0) of the share of kids 0-3 years old that attend pre-K education; Pre-K 4-5 above-below median (1/0) of the share of kids 4-5 years old that attend pre-K education; Population above-below median (1/0); College population above-below median (1/0); College public administration above-below median (1/0); Poverty above-below median (1/0); Gini above-below median (1/0); Income per capita above-below median; Big south (1/0, where 1 are south, southeast and mid-west regions; and 0 are north and northeast regions). Mean LHS is the mean WTP on the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Belief updating - Weight placed on study characteristics - Different effect sizes

	(1)	(2)	(3)	(4)
VARIABLES	Posterior	Posterior	Posterior	Posterior
Prior	0.5678***	0.4644***	0.6820***	0.5671***
	(0.0372)	(0.0499)	(0.0467)	(0.0574)
Signal	0.3431***	0.4085***	0.2664***	0.3757***
	(0.0271)	(0.0365)	(0.0368)	(0.0428)
Signal*Developing	0.0126	-0.0048	0.0382	0.0140
	(0.0262)	(0.0363)	(0.0379)	(0.0443)
Signal*Large	0.3145***	0.3434***	0.3098***	0.2434***
	(0.0561)	(0.0713)	(0.0972)	(0.0932)
Prior*Conference	0.0379	0.0231	0.1017	0.0128
	(0.0555)	(0.0737)	(0.0627)	(0.0733)
Signal*Conference	0.0555	0.0684	0.0290	0.0513
	(0.0506)	(0.0701)	(0.0570)	(0.0683)
Signal*Developing*Conference	0.0367	0.0830	-0.0411	0.0256
	(0.0477)	(0.0671)	(0.0609)	(0.0721)
Signal*Large*Conference	-0.0984	-0.1125	-0.1610	-0.0913
	(0.0782)	(0.1139)	(0.1233)	(0.1267)
Observations	1,188	702	486	544
Round	1 and 2	1	2	1
Context	Municipality	Municipality	Municipality	Random Study
Clusters	702	702	486	544
Mean LHS	0.442	0.429	0.461	0.442

Notes: OLS results. The dependent variables is posterior beliefs, which are declared after successfully buying the results from a study in each round. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. WTP is the willingness-to-pay for research information. 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. Context is either the respondent's own municipality (columns 1, 2 and 3) or one of the four possible studies (column 4). Conference is a dummy which is equal to one (zero) for the six (eight) conferences in which the informed effect sizes were 0.26 (0.26) for Colombia, 0.38 (0.91) for Jamaica, 0.50 (0.87) for Michigan and 0.18 (0.15) for USA. Mean LHS is the average posterior belief of the left-hand side of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Effect of partisan message on priors - Alternative definitions of leftist

	Definition 1		Definition 2		Definition 3	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Prior	Prior	Prior	Prior	Prior	Prior
Message	0.0012	0.0365	0.0007	0.0055	-0.0091	0.0417
	(0.0207)	(0.0296)	(0.0225)	(0.0357)	(0.0284)	(0.0455)
Leftist	-0.0315	-0.0689	-0.0503	-0.1380***	-0.0656**	-0.0071
	(0.0429)	(0.0425)	(0.0423)	(0.0403)	(0.0278)	(0.0458)
Message*Leftist	0.0252	0.0191	-0.0479	0.0167	0.0212	-0.0626
	(0.0749)	(0.0564)	(0.0577)	(0.0656)	(0.0417)	(0.0643)
Observations	475	360	412	268	412	268
Context	Municipality	Random Study	Municipality	Random Study	Municipality	Random Study
Clusters	475	180	412	134	412	134
Mean LHS	0.376	0.393	0.372	0.403	0.372	0.403

Notes: OLS results. The dependent variable is respondents' priors. In definition 1 we define a participant as leftist if the mayor of the municipality is affiliated with PT. In definition 2 we define a participant as leftist if the participant of the municipality is affiliated with PT. In definition 3 we define a participant as leftist if the participant of the municipality is affiliated with a leftist party (PC do B, PDT, PMB, PMN, PPS, PSB, PSD, PSOL, PT, REDE, SD, PT do B). 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. Mean LHS is the average of the left-hand side variable of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Effect of partisan message on WTP - Alternative definitions of leftist

	Definition 1			Definition 2			Definition 3		
VARIABLES	(1) WTP	(2) WTP	(3) WTP	(4) WTP	(5) WTP	(6) WTP	(7) WTP	(8) WTP	(9) WTP
Message	-1.0938	-3.8522	0.8998	0.0984	-3.4901	2.9811	5.8783	-4.6836	13.8791*
	(3.8978)	(3.9376)	(4.9713)	(4.3932)	(4.3797)	(5.8992)	(5.5399)	(5.4554)	(7.4235)
Leftist	-18.0347***	-14.3397	-19.8412**	-8.0915	-7.1076	-7.5692	6.5332	0.0580	11.5445
	(6.8680)	(10.4071)	(7.6666)	(9.0199)	(10.3728)	(10.5847)	(6.3139)	(6.1829)	(8.2656)
Message*Leftist	15.4800	27.7397*	8.5262	3.1442	17.1810	-5.0235	-12.1770	6.5547	-24.8565**
	(11.7931)	(15.9910)	(12.1976)	(12.5786)	(15.7540)	(13.7005)	(8.2572)	(8.5716)	(10.7190)
Observations	881	341	540	686	284	402	686	284	402
Round	1 and 2	1	2	1 and 2	1	2	1 and 2	1	2
Clusters	341	341	180	284	284	134	284	284	134
Mean LHS	49.25	53.52	46.56	48.89	52.84	46.10	48.89	52.84	46.10

Notes: OLS results. The dependent variable willingness to pay for studies. In definition 1 we define a participant as leftist if the mayor of the municipality is affiliated with PT. In definition 2 we define a participant as leftist if the participant of the municipality is affiliated with PT. In definition 3 we define a participant as leftist if the participant of the municipality is affiliated with a leftist party (PC do B, PDT, PMB, PMN, PPS, PSB, PSD, PSOL, PT, REDE, SD, PT do B). 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. Mean LHS is the average of the left-hand side variable of each equation. Robust standard errors clustered at the individual level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Effect of partisan message on posteriors

VARIABLES	(1) Posterior	(2) Posterior	(3) Posterior
Prior	0.6558***	0.5787***	0.7633***
	(0.0366)	(0.0487)	(0.0475)
Signal	0.3141***	0.3541***	0.2559***
	(0.0308)	(0.0382)	(0.0418)
Signal*Message	0.0411	0.0446	0.0279
	(0.0358)	(0.0462)	(0.0484)
Signal*Message*Leftist	-0.0441	-0.0147	-0.0510
	(0.0516)	(0.0779)	(0.0569)
Observations	486	327	159
Round	1 and 2	1	2
Clusters	327	327	159
Mean LHS	0.458	0.437	0.503

Notes: OLS results. The dependent variables is posterior beliefs, which are declared after successfully buying the results from a study in each round. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. 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 for self-identifying leftist (0-4) in a 0-10 scale. Mean LHS is the average of the left-hand side variable of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Effect of partisan message on posteriors - Alternative definitions of leftist

	Definition 1			Definition 2			Definition 3		
VARIABLES	(1) Posterior	(2) Posterior	(3) Posterior	(4) Posterior	(5) Posterior	(6) Posterior	(7) Posterior	(8) Posterior	(9) Posterior
Prior	0.6549***	0.5772***	0.7649***	0.6684***	0.6141***	0.7687***	0.6691***	0.6095***	0.7679***
	(0.0366)	(0.0487)	(0.0473)	(0.0425)	(0.0564)	(0.0547)	(0.0425)	(0.0566)	(0.0546)
Signal	0.3145***	0.3549***	0.2550***	0.3043***	0.3370***	0.2306***	0.3040***	0.3393***	0.2310***
	(0.0308)	(0.0382)	(0.0417)	(0.0352)	(0.0421)	(0.0488)	(0.0352)	(0.0422)	(0.0488)
Signal*Message	0.0227	0.0346	0.0039	0.0261	0.0199	0.0442	0.0318	0.0278	0.0404
	(0.0330)	(0.0438)	(0.0430)	(0.0381)	(0.0486)	(0.0511)	(0.0417)	(0.0538)	(0.0573)
Signal*Message*Leftist	0.0861	0.1235	0.0481	0.1658**	0.2515***	-0.0243	0.0286	0.0416	0.0039
	(0.0725)	(0.0962)	(0.0933)	(0.0837)	(0.0908)	(0.1031)	(0.0583)	(0.0744)	(0.0693)
Observations	486	327	159	383	270	113	383	270	113
Round	1 and 2	1	2	1 and 2	1	2	1 and 2	1	2
Clusters	327	327	159	270	270	113	270	270	113
Mean LHS	0.458	0.437	0.503	0.465	0.447	0.509	0.465	0.447	0.509

Notes: OLS results. The dependent variables is posterior beliefs, which are declared after successfully buying the results from a study in each round. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. When dealing with a second update in posteriors, the first update is treated as a prior. In definition 1 we define a participant as leftist if the mayor of the municipality is affiliated with PT. In definition 2 we define a participant as leftist if the participant of the municipality is affiliated with PT. In definition 3 we define a participant as leftist if the participant of the municipality is affiliated with a leftist party (PC do B, PDT, PMB, PMN, PPS, PSB, PSD, PSOL, PT, REDE, SD, PT do B). 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. Mean LHS is the average of the left-hand side variable of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Incentives - Belief elicitation and valuation

VARIABLES	(1) Prior Avg	(2) WTP	(3) Posterior
Prior			0.5786***
			(0.0495)
Signal			0.4107***
			(0.0476)
Signal*Large			0.1532*
			(0.0809)
Signal*Developing			0.0222
			(0.0493)
Prior*Stake			0.0387
			(0.0669)
Signal*Stake			-0.0665
			(0.0660)
Signal*Large*Stake			0.0659
			(0.1248)
Signal*Developing*Stake			-0.0260
			(0.0704)
Large		5.0237	
		(3.7864)	
Developing		3.1787	
		(3.7757)	
Stake	-0.0055	0.8542	
	(0.0215)	(4.5909)	
Large*Stake		-1.0066	
		(5.1227)	
Developing*Stake		-2.0386	
		(5.1145)	
Observations	278	676	544
Round	1	1	1
Context	Random Study	-	Random Study
Clusters	278	676	544
Mean LHS	0.402	47.45	0.425

Notes: OLS results. In column 1 the dependent variable is respondents' average prior about random studies. In column 2 the dependent variable is willingness to pay for studies. In column 3 the dependent variable is posterior beliefs, which are declared after successfully buying the results from a study. Prior is the belief of the respondent about the effect, right before buying some study. Signal is the bought study's effect size. 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. Stake is a dummy which is equal to one when the randomly assigned stake-level is equal to 100 tickets, and 0 when is 10 tickets. Mean LHS is the average of the left-hand side variable of each equation. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: Posterior and valuation for implementation advice

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

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

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Table A.15: Probability of session participation

VARIABLES	(1) Information Session
Treatment Assignment	0.3727*** (0.0166)
Observations	1,818
Mean Control	0.0064

Notes: OLS estimation results. The dependent variable is a dummy which takes the value of 1 if the municipality's mayor attended the information session about taxpayers reminder letters. Treatment assignment is a dummy which takes the value of 1 if the mayor was assigned to the treatment group. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.16: ITT: Beliefs and confidence

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Belief-10%	Belief-10%	Belief-10%	Belief-10%	Belief-10%	Belief-10%
Treatment Assignment	-0.6102**	-0.6889*	-0.5587*	0.0703	-0.2489	0.2529
	(0.2484)	(0.3774)	(0.3080)	(0.2490)	(0.3829)	(0.3093)
Mean Control	6.775	6.824	6.742	7.172	6.857	7.382
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Belief-12%	Belief-12%	Belief-12%	Belief-12%	Belief-12%	Belief-12%
Treatment Assignment	-0.5232**	-0.5237*	-0.5286**	0.1223	-0.0805	0.2340
	(0.2049)	(0.3020)	(0.2628)	(0.2067)	(0.3144)	(0.2607)
Mean Control	6.968	6.859	7.041	7.006	6.719	7.198
Panel C	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Belief-20%	Belief-20%	Belief-20%	Belief-20%	Belief-20%	Belief-20%
Treatment Assignment	-0.2462	-0.0791	-0.3891	0.2857	0.3903	0.2188
	(0.2750)	(0.4085)	(0.3583)	(0.2867)	(0.4221)	(0.3638)
Mean Control	9.122	8.389	9.616	7.904	7.860	7.933
Panel D	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Confidence	Confidence	Confidence	Confidence	Confidence	Confidence
Treatment Assignment	0.0402	0.0543	0.0382	-0.0003	-0.1150	0.0771
	(0.0429)	(0.0711)	(0.0532)	(0.0440)	(0.0705)	(0.0540)
Mean Control	-0.0171	0.109	-0.102	-0.00291	0.126	-0.0894
Observations	2,186	860	1,326	2,156	845	1,311
Respondent	All	Mayor	Finance Staff	All	Mayor	Finance Staff
Policy	Reminder Letters	Reminder Letters	Reminder Letters	Financial Incentives	Financial Incentives	Financial Incentives
Clusters (Municipalities)	1440	860	1326	1432	845	1311

Notes: OLS estimation results. In panels A, B and C, the dependent variable is the absolute difference between self-reported beliefs about effect sizes of policy on local tax revenues, and the informed effect size of the reminder letters policy during the information session. In panel D, the dependent variable is self-reported confidence level about beliefs in a likert-scale of 1 to 5 (standardized to mean 0 and standard deviation 1).Treatment assignment is a dummy which takes the value of 1 if the mayor was assigned to the treatment group. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.17: ITT: Policy adoption - Reminder letters

VARIABLES	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted	(6) Adopted
Treatment Assignment	0.0402*	0.0359*	0.0458	0.0530	0.0390	0.0278
	(0.0208)	(0.0213)	(0.0324)	(0.0345)	(0.0249)	(0.0253)
Observations	2,271	2,055	913	785	1,358	1,270
Respondent	All	All	Mayor	Mayor	Finance Staff	Finance Staff
Placebo Included	Yes	No	Yes	No	Yes	No
Clusters (Municipalities)	1465	1413	913	785	1358	1270
Mean Control	0.317	0.298	0.367	0.342	0.283	0.270

Notes: OLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Treatment assignment is a dummy which takes the value of 1 if the mayor was assigned to the treatment group. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.18: Policy adoption - Reminder letters placebo

VARIABLES	(1) Adopted	(2) Adopted	(3) Adopted
Information Session	0.0469	-0.0102	0.1114
	(0.0657)	(0.0942)	(0.0875)
Observations	693	321	372
Respondent	All	Mayor	Finance Staff
Clusters (Municipalities)	595	321	372
Mean Control	0.158	0.211	0.111

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the placebo component of the policy was adopted in municipality, where the placebo component was "The tax reminders sent informed taxpayers that the Brazilian constitution was reformed in 1988". Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.19: Policy adoption with controls - Reminder letters

VARIABLES	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted	(6) Adopted
Information Session	0.1073**	0.1011*	0.1148	0.1419*	0.1076*	0.0798
	(0.0522)	(0.0539)	(0.0776)	(0.0843)	(0.0649)	(0.0660)
Observations	2,269	2,054	912	785	1,357	1,269
Respondent	All	All	Mayor	Mayor	Finance Staff	Finance Staff
Placebo Included	Yes	No	Yes	No	Yes	No
Mayor Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Clusters (Municipalities)	1464	1412	912	785	1357	1269
Mean Control	0.317	0.298	0.367	0.342	0.283	0.270

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College or more (1/0); 2nd Term (1/0) and Political party leftist (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); Monthly income per capita above-below median (1/0); Local tax revenues share 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). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.20: ITT: Policy adoption with controls - Reminder letters

VARIABLES	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted	(6) Adopted
Treatment Assignment	0.0422**	0.0392*	0.0469	0.0565*	0.0412*	0.0304
	(0.0206)	(0.0210)	(0.0321)	(0.0341)	(0.0250)	(0.0253)
Observations	2,269	2,054	912	785	1,357	1,269
Respondent	All	All	Mayor	Mayor	Finance Staff	Finance Staff
Placebo Included	Yes	No	Yes	No	Yes	No
Mayor Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Clusters (Municipalities)	1464	1412	912	785	1357	1269
Mean Control	0.317	0.298	0.367	0.342	0.283	0.270

Notes: OLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Treatment assignment is a dummy which takes the value of 1 if the mayor was assigned to the treatment group. Mayors' characteristics included in the model are: Male (1/0); Age above-below median (1/0); College or more (1/0); 2nd Term (1/0) and Political party leftist (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); Monthly income per capita above-below median (1/0); Local tax revenues share 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). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.21: Policy adoption mayor's Heterogeneities - Reminder letters

VARIABLES	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted
Characteristic	Male	Age	College	2nd Term	Leftist
Information Session	0.0136	0.1261*	0.1174	0.0837	0.0360
	(0.2153)	(0.0683)	(0.1079)	(0.0615)	(0.0713)
Information Session*Characteristic	0.0862	-0.0690	-0.0353	0.0593	0.1589
	(0.2227)	(0.1115)	(0.1251)	(0.1369)	(0.1113)
Characteristic	0.0161	0.0071	0.0156	-0.0284	-0.0357
	(0.0487)	(0.0291)	(0.0298)	(0.0391)	(0.0305)
Observations	2,055	2,055	2,055	2,055	2,055
Respondent	All	All	All	All	All
Placebo Included	No	No	No	No	No
Clusters (Municipalities)	1413	1413	1413	1413	1413
Mean Control	0.298	0.298	0.298	0.298	0.298

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Mayors' characteristics included as interactions are: Male (1/0); Age above-below median (1/0); College or more (1/0); 2nd Term (1/0) and Political party leftist (1/0, mayors belonging to a center-leftist party according to historical political platforms). Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.22: Policy adoption municipalities' Heterogeneities - Reminder letters

VARIABLES	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted	(6) Adopted	(7) Adopted	(8) Adopted
Characteristic	Pop	College pop	College Adm	Poverty	Gini	Income pc	Local Taxes	Big South
Information Session	0.1182	0.0667	0.1236	0.1179	0.1777**	0.0968	0.0751	0.1095
	(0.0784)	(0.0758)	(0.0754)	(0.0783)	(0.0843)	(0.0781)	(0.0768)	(0.0901)
Information Session*Characteristic	-0.0484	0.0492	-0.0626	-0.0450	-0.1458	-0.0024	0.0316	-0.0164
	(0.1100)	(0.1099)	(0.1100)	(0.1094)	(0.1107)	(0.1092)	(0.1100)	(0.1134)
Characteristic	0.0238	0.0429	0.0473	-0.0812***	-0.0631**	0.0991***	0.0347	0.1022***
	(0.0291)	(0.0291)	(0.0291)	(0.0290)	(0.0292)	(0.0289)	(0.0291)	(0.0297)
Observations	2,055	2,055	2,055	2,055	2,055	2,055	2,054	2,055
Respondent	All	All	All	All	All	All	All	All
Placebo Included	No	No	No	No	No	No	No	No
Clusters (Municipalities)	1413	1413	1413	1413	1413	1413	1412	1413
Mean Control	0.298	0.298	0.298	0.298	0.298	0.298	0.298	0.298

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. 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); Monthly income per capita above-below median (1/0); Local tax revenues share 2010-2015 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). Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.23: ITT: Policy adoption - Reminder letters information components

VARIABLES	(1) On Time	(2) Audit	(3) Social Norm	(4) Before Due	(5) Letter
Treatment Assignment	0.0345	0.0283	0.0427***	0.0247	0.0275*
	(0.0212)	(0.0187)	(0.0143)	(0.0206)	(0.0165)
Observations	2,055	2,055	2,055	2,055	2,055
Respondent	All	All	All	All	All
Placebo Included	No	No	No	No	No
Clusters (Municipalities)	1413	1413	1413	1413	1413
Mean Control	0.294	0.198	0.0944	0.275	0.142

Notes: OLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the information component of the policy was adopted in municipality, and 0 otherwise. On Time refers to a reminder message highlighting the tax payment deadline. Audit refers to a reminder message highlighting the risks of audits for not paying taxes on time. Social Norm refers to a reminder message highlighting the social norm of paying taxes. Before due refers to sending the reminder message before taxes' due date. Letter refers to sending the reminder message using a hard copy letter. Treatment assignment is a dummy which takes the value of 1 if the mayor was assigned to the treatment group. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.24: Policy adoption - Reminder letters information components: Mayors

VARIABLES	(1) On Time	(2) Audit	(3) Social Norm	(4) Before Due	(5) Letter
Information Session	0.1322	0.1210	0.1990***	0.0786	0.1100
	(0.0866)	(0.0782)	(0.0652)	(0.0855)	(0.0746)
Observations	785	785	785	785	785
Respondent	Mayor	Mayor	Mayor	Mayor	Mayor
Placebo Included	No	No	No	No	No
Clusters (Municipalities)	785	785	785	785	785
Mean Control	0.335	0.225	0.112	0.321	0.194

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if mayor says the information component of the policy was adopted in municipality, and 0 otherwise. On Time refers to a reminder message highlighting the tax payment deadline. Audit refers to a reminder message highlighting the risks of audits for not paying taxes on time. Social Norm refers to a reminder message highlighting the social norm of paying taxes. Before due refers to sending the reminder message before taxes' due date. Letter refers to sending the reminder message using a hard copy letter. Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.25: Policy adoption - Reminder letters information components: Finance staff

VARIABLES	(1) On Time	(2) Audit	(3) Social Norm	(4) Before Due	(5) Letter
Information Session	0.0689	0.0476	0.0590	0.0612	0.0542
	(0.0665)	(0.0580)	(0.0434)	(0.0647)	(0.0479)
Observations	1,270	1,270	1,270	1,270	1,270
Respondent	Finance Staff	Finance Staff	Finance Staff	Finance Staff	Finance Staff
Placebo Included	No	No	No	No	No
Clusters (Municipalities)	1270	1270	1270	1270	1270
Mean Control	0.268	0.181	0.0828	0.245	0.109

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if finance staff says the information component of the policy was adopted in municipality, and 0 otherwise. On Time refers to a reminder message highlighting the tax payment deadline. Audit refers to a reminder message highlighting the risks of audits for not paying taxes on time. Social Norm refers to a reminder message highlighting the social norm of paying taxes. Before due refers to sending the reminder message before taxes' due date. Letter refers to sending the reminder message using a hard copy letter. Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.26: Policy adoption - Financial incentives

VARIABLES	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted	(6) Adopted
Information Session	0.0092	0.0307	0.0900	0.1245	-0.0445	-0.0252
	(0.0567)	(0.0604)	(0.0819)	(0.0921)	(0.0702)	(0.0727)
Observations	2,208	2,002	866	748	1,342	1,254
Respondent	All	All	Mayor	Mayor	Finance Staff	Finance Staff
Placebo Included	Yes	No	Yes	No	Yes	No
Clusters (Municipalities)	1452	1393	866	748	1342	1254
Mean Control	0.602	0.590	0.602	0.583	0.603	0.595

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.27: Policy adoption - E-procurement

VARIABLES	(1) Adopted	(2) Adopted	(3) Adopted	(4) Adopted	(5) Adopted	(6) Adopted
Information Session	0.0228	0.0261	0.0119	0.0505	0.0357	0.0122
	(0.0676)	(0.0708)	(0.0928)	(0.1010)	(0.0840)	(0.0865)
Observations	1,696	1,542	707	617	989	925
Respondent	All	All	Mayor	Mayor	Finance Staff	Finance Staff
Placebo Included	Yes	No	Yes	No	Yes	No
Clusters (Municipalities)	1191	1133	707	617	989	925
Mean Control	0.449	0.434	0.503	0.483	0.410	0.400

Notes: 2SLS estimation results. The dependent variable is a dummy which takes the value of 1 if respondent says the policy was adopted in municipality. Information session is a dummy which takes the value of 1 if the municipality's mayor attended the information session about tax reminders. This last variable is instrumented with treatment assignment. Robust standard errors clustered at the municipality level are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

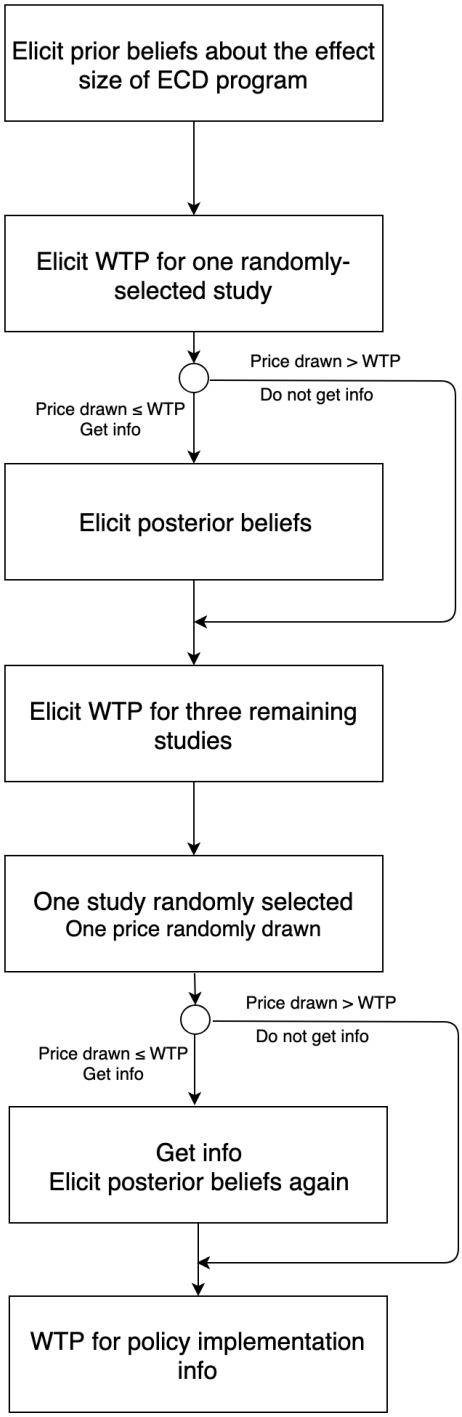
A.2
Figures

Demand-for-Research Experiment

Figure A.1: Experimental setting

Notes: Picture taken in the *Diálogo Municipalista* conference in the city of Canela, Rio Grande do Sul.

Figure A.2: Experiment structure



Notes: Survey experiment flowchart.

Figure A.3: ECD - Description

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

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

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

Notes: Survey experiment script.

Figure A.4: ECD - Goal and measure

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

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

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

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

Notes: Survey experiment script.

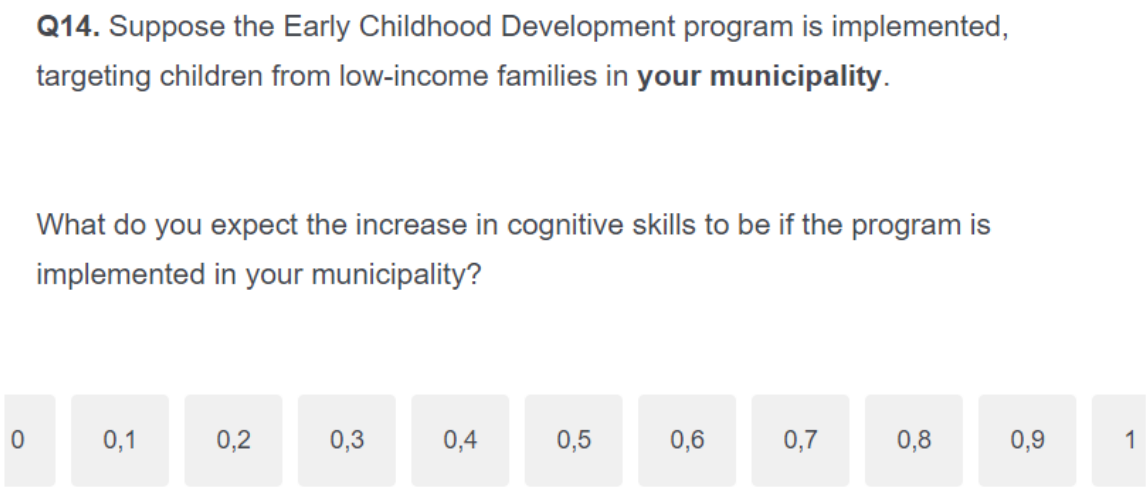
Figure A.5: ECD - Benchmarks

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 Família	0 0.1 **0.2** 0.3 0.4 0.5 0.6 0.7 0.8 0.9 1
Replacing group teaching by individual teaching	0 0.1 0.2 0.3 0.4 **0.5** 0.6 0.7 0.8 0.9 1

Notes: Survey experiment script.

Figure A.6: Prior eliciting



Notes: Survey experiment script.

Figure A.7: Prior eliciting - Other context

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

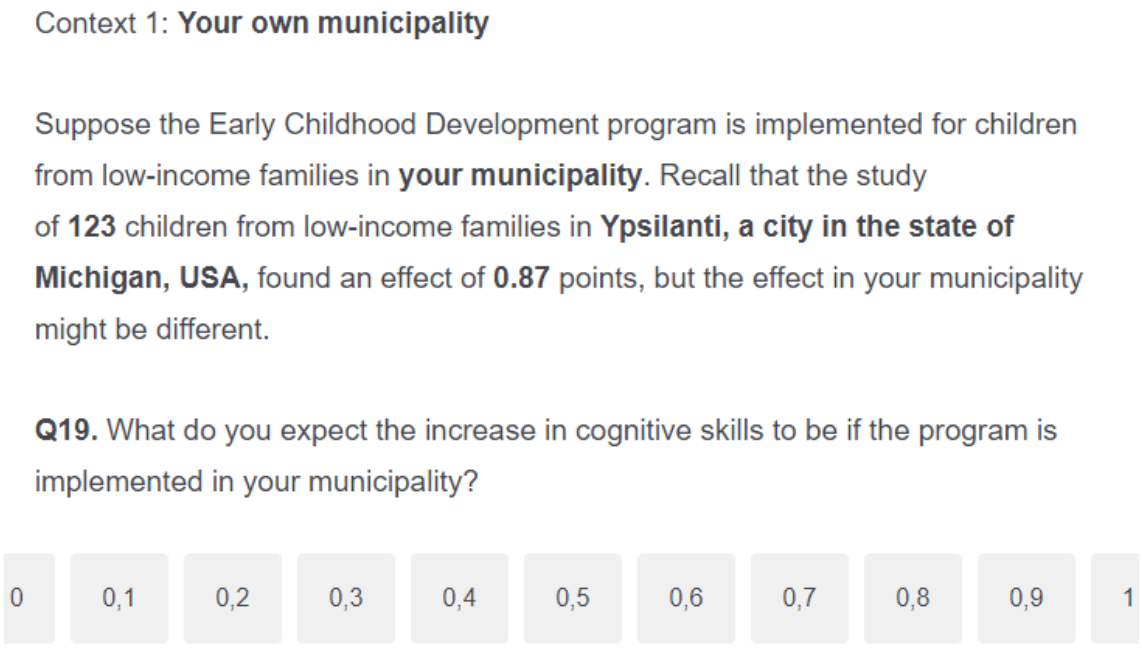
The early childhood development program was implemented for **123** children from low-income families in **Ypsilanti, a city in the state of Michigan, USA**. What do you think the effect of the program was there?

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

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

Notes: Survey experiment script.

Figure A.8: Posterior



Notes: Survey experiment script.

Supply-of-Research Experiment

Figure A.9: Policy brief - Page 1

COMO AUMENTAR AS RECEITAS TRIBUTÁRIAS LOCAIS

Informe de Política Pública Baseado em Pesquisas Científicas

INTRODUÇÃO

Aumentar a receita tributária em nível local é uma tarefa importante para os governos municipais brasileiros. Tributos locais incrementam o orçamento municipal, mas também proporcionam recursos não vinculados, os quais os municipios podem utilizar de acordo com as suas próprias prioridades. No entanto, os municipios brasileiros enfrentam um sério desafio no que tange ao recolhimento de tributos locais: muitas empresas e indivíduos não estão cumprindo com a legislação tributária ao não pagarem seus tributos em dia.

Em todo o mundo, inclusive no Brasil, os governos têm tentado diversos métodos inovadores para resolver esse problema. Mas o que funciona e o que não funciona? Este informe de política pública fornece resultados simples de pesquisas científicas sobre como os governos podem levar os contribuintes a pagarem seus tributos com o objetivo de aumentar as receitas tributárias locais.

UMA FORMA EFETIVA E DE BAIXO CUSTO DE AUMENTAR O CUMPRIMENTO DAS OBRIGAÇÕES TRIBUTÁRIAS: CARTAS LEMBRETE

Pesquisas realizadas na América Latina revelaram uma ação muito simples e de baixo custo que tem provado ser efetiva em aumentar o cumprimento das obrigações tributárias: **o envio de cartas lembrete aos contribuintes antes da data de vencimento dos tributos**.[1] Por exemplo, um pesquisador acadêmico trabalhou com dois governos municipais no Peru e constatou que o cumprimento das obrigações tributárias sobre a propriedade aumentou em 10%, simplesmente com o envio de uma carta para os contribuintes que os lembrava do prazo de pagamento do imposto![2] Resultados semelhantes foram encontrados em outros estudos, inclusive nos Estados Unidos, Áustria e no Reino Unido.[3]

1 Contribuintes são aqueles legalmente responsáveis por pagar os tributos. Por exemplo, os contribuintes do Imposto Predial e Territorial Urbano (IPTU) são os donos da propriedade (ou os inquilinos, se explicitamente estabelecido no contrato de locação). Os contribuintes do Imposto sobre Serviços de Qualquer Natureza (ISSQN) são os profissionais ou empresas que prestam o serviço.
2 Del Carpio (2013)
3 Coleman (1996), Hallsworth et al. (2014), Fellner et al. (2013)

Notes: Taxpayer reminder letters policy brief.

Figure A.10: Policy brief - Page 2

Pesquisas científicas também podem orientar em como fazer as cartas lembrete ainda mais efetivas. Uma lição importante de política pública é que **a carta deve enfatizar que a maioria das pessoas pagam seus tributos em dia**. O mesmo estudo no Peru observou que o cumprimento com as obrigações tributárias aumentou em 20%, caso a carta lembrete também incluisse uma frase como "A grande maioria de seus vizinhos pagam seus tributos em dia!" ou "75% dos seus vizinhos pagam seus impostos em dia!". Tal mensagem destaca que o pagamento de tributos em dia é uma "norma social", e aqueles que não pagam estão se desviando da norma social desejável.

Há uma última lição das pesquisas sobre a forma de aumentar a efetividade das cartas lembrete: **salientar a ameaça de auditorias ou penalidades em razão do não pagamento dos tributos em dia**. Por exemplo, um estudo na Argentina constatou que o envio de uma carta aos donos de propriedades (que supostamente devem pagar os impostos sobre a propriedade) enfatizando as possíveis multas e auditorias por evasão de impostos aumentou o cumprimento das obrigações tributárias em 12%.[4]

Um ponto importante a se ter em mente é que as cartas lembrete são pouco custosas de se enviar. Tudo que as autoridades fiscais precisam saber são os endereços dos potenciais contribuintes. Em muitos casos, cartas já estão sendo enviadas para esses contribuintes. Simplesmente escolhendo o conteúdo correto da carta, os governos têm sido capazes de aumentar o cumprimento das obrigações tributárias e as receitas locais, e têm reduzido a evasão fiscal. Por exemplo, incluindo um lembrete do prazo de pagamento, enfatizando as normas sociais e ressaltando a ameaça de auditorias ou penalidades. Esta pode ser uma política altamente custo-efetiva e é, além disso, fácil de implementar se comparada com outras estratégias disponíveis para aumentar as receitas tributárias.[5]

LIÇÕES DE POLÍTICA PÚBLICA

Para resumir, este informe oferece um total de **três lições de política pública:**

- Enviar cartas aos contribuintes lembrando-os do prazo para pagamento de tributos.
- Enfatizar na carta que a maioria das pessoas paga seus tributos em dia.
- Destacar as potenciais consequências negativas de não pagar os tributos: multas e auditorias.

Uma carta exemplo é fornecido na página 3 deste informe. Entre em contato com a equipe do projeto através do e-mail **contato@pesquisadoresdeharvardcnm.com** para receber uma cópia eletrônica da carta, e para esclarecer suas dúvidas em relação ao informe.

4 Castro and Scartascini (2013)
5 Uma ação custo-efetiva é aquela que produz bons resultados com baixo custo.

2 • Como Aumentar as Receitas Tributárias Locais

Notes: Taxpayer reminder letters policy brief.

Figure A.11: Policy brief - Page 3

EXEMPLO
DE CARTA LEMBRETE PARA O PAGAMENTO DE TRIBUTO

Informe de Política Pública Baseado em Pesquisas Científicas

APRESENTANDO NORMAS SOCIAIS E AMEAÇA DE PENALIDADES

Prezado Sr./Sra.,

O pagamento de seus tributos municipais deve ser feito até **1 de novembro de 2016**. Nossas estatísticas mostram que a **grande maioria de seus vizinhos pagarão seus tributos em dia.** Nós apreciaremos muito se você fizer o mesmo.

Não se esqueça de declarar seus tributos corretamente e em tempo para evitar **o risco de uma auditoria**, que é um processo demorado e custoso. Lembrando que a auditoria poderá levar a penalidades financeiras substanciais, entre outras punições, caso se identifique que sua declaração de tributos esteja errada.

É fácil pagar seus tributos. Por favor, siga as instruções em anexo para mais informações.

Se você já pagou seus tributos, muito obrigado! Se não, por favor, faça-o agora.

Atenciosamente,

Nome da Autoridade Fiscal

Notes: Taxpayer reminder letters policy brief.

Figure A.12: Policy brief - Page 4

REFERÊNCIAS DAS PESQUISAS ACADÊMICAS

Como Aumentar as Receitas Tributárias Locais

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

Analisa contribuintes de impostos regionais sobre a propriedade na Argentina. Encontra um forte efeito ao lembrar os contribuintes sobre a possibilidade de serem auditados se não pagarem seus impostos.

• Coleman, S. (1996). "The Minnesota Income Tax Compliance Experiment: State Tax Results." MPRA Paper No. 4827, Universidade de Munique.

Analisa contribuintes de imposto de renda nos Estados Unidos. Encontra um forte efeito em cartas lembrete enfatizando normas sociais e em cartas lembrando sobre a possibilidade de auditorias.

• 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.

Analisa contribuintes municipais no Peru. Encontra forte efeito das cartas lembrete e um efeito ainda mais forte de cartas que enfatizam normas sociais.

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

Analisa contribuintes de licença para assistir televisão na Áustria. Encontra forte efeito das cartas lembrete e um efeito ainda mais forte das cartas que lembram sobre a possibilidade de auditorias.

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

Analisa contribuintes de imposto de renda no Reino Unido. Encontra forte efeito das cartas lembrete e das cartas que enfatizam normas sociais.

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

Analisa contribuintes de imposto sobre empresas no Reino Unido. Encontra forte efeito das cartas lembrete sobre a possibilidade de auditorias.

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Universidade de Harvard e **Confederação Nacional de Municípios**

Notes: Taxpayer reminder letters policy brief.

A.3
Theoretical framework

A.3.1
Set-up

Policymakers have a prior about the effectiveness of the policy (i.e. the effect size). Policymaker j's prior S_j^{pr} follows a normal distribution, $S_j^{pr} \sim \mathcal{N}(\mu_j^{pr}, \Sigma_j^{pr})$, where μ_j^{pr} is the mean of policymaker j's prior and Σ_j^{pr} is the perceived variance or noise of prior.

In the experiment, we give policymaker j some information about the effectiveness of the policy. Information S_j^I follows a normal distribution, $S_j^I \sim \mathcal{N}(\mu^I, \Sigma_j^I)$, where μ^I is the mean of the signal (i.e. the effect size estimated by researchers). Σ_j^I is policymaker j's perceived variance of the information. We assume that Σ_j^I:

- Decreases with the sample size: as the sample size of the experiment gets larger, policymakers think the information is less noisy.
- Decreases with the similarity between the experimental lacation and own municipality: if the experiment was conducted in a location that was similar to the policymaker's own municipality, he or she thinks the information is more reliable.

In the experiment, we are able to manipulate μ^I and Σ_j^I by presenting policymakers with the results of a randomly-selected research study.

A.3.2
Solution concept

Policymaker j weights two signals to form his or her posterior:

$$S_j^{po} = (1 - \pi)S_j^{pr} + \pi S_j^I$$

The policymaker's goal is to have a more accurate belief about the effect size. So he or she will choose the optimal weight π^* such that the mean squared error $E(S_j^{po} - \mu_j^{po})^2$ is minimized, where $\mu_j^{po} = (1 - \pi)\mu_j^{pr} + \pi\mu^I$

A.3.3
Model predictions

A.3.3.1 The optimal weight

Suppose S_j^{pr} and S_j^I are uncorrelated, then solving the minimization problem gives us $\pi^* = \frac{\Sigma_j^{pr}}{\Sigma_j^{pr}+\Sigma_j^I}$

Here we can see that the weight of the information, π^*:

- Decreases with the perceived noise of the information
  - Increases with the sample size
  - Increases with the similarity between the experimental location and own municipality

We can see the mean of posterior μ_j^{po}

- Increases with the mean of the research information (i.e. the effect size estimated by researchers)

A.3.3.2 WTP for information

Suppose policymaker j's WTP for information is proportional to the improvement of information accuracy (i.e. the reduction in mean squared error)

$$WTP_j = k_j\Big(E(S_j^{pr} - \mu_j^{pr})^2 - E(S_j^{po} - \mu_j^{po})^2\Big) = k_j\pi^*\Sigma_j^{pr}$$

where k_j measures policymaker j's desire for learning about the policy or the return on information quality improvement. Here we can see that the WTP for information:

- Decreases with Σ_j^I, the perceived noise of information
  - Increases with the sample size
  - Increases with the similarity between the experimental location and own municipality

B
Chapter 2

B.1
Summary statistics

Table B.1 presents basic descriptive statistics for both mayors' and municipalities' characteristics, measured before the FPM shock took place. These statistics are displayed for the sample of municipalities used in the main analysis (column 1) and for the group of municipalities excluded from the analysis (column 2). Besides providing information on the average municipality's socioeconomic characteristics, the table also reports differences in the group means (column 3) and the standard errors of these differences (column 4).

As expected, considering the type of municipalities excluded from the analysis and the way in which we constructed the treatment variables, the municipalities in our sample tend to be different than the out-of-sample municipalities. Principally, on average, they are less populated and sparsely populated and are farther from the state capital. On the other hand, a higher proportion of municipalities are administered by mayors that have attended college or university, and that are in their first term in office.

Table B.1: Summary statistics - Characteristics of the municipalities

Variables	Sample Municipalities (1)	Out-of-Sample Municipalities (2)	Difference (3)	Standard Error (4)
Mayor Characteristics				
Male	0.921	0.917	0.004	0.007
Age	48.74	48.35	0.383	0.260
Education high	0.543	0.503	0.041***	0.013
First term	0.734	0.692	0.042***	0.012
Member PMDB	0.189	0.198	-0.009	0.011
Member PSDB	0.156	0.154	0.003	0.010
Member PT	0.074	0.071	0.003	0.007
Member PP	0.096	0.102	-0.006	0.008
Member PFL	0.141	0.139	0.002	0.009
Member PTB	0.074	0.078	-0.005	0.007
Aligned with federal government	0.077	0.073	0.004	0.007
Municipality Characteristics				
Population in 2006	18,481	33,956	-15,474***	1,702
Population growth in 2003-2006	0.008	0.009	-0.001	0.001
Population density in 2006	58.05	140.53	-82.48***	15.10
Urban population in 2000	0.589	0.580	0.009	0.006
College degree adults in 2000	0.024	0.022	0.002***	0.001
Literacy adults in 2000	0.723	0.725	-0.002	0.004
College degree public adm. in 2006	0.257	0.249	0.007**	0.003
Income per capita in 2000	339.3	332.6	6.710	5.115
Gini in 2000	0.550	0.543	0.007***	0.002
Distance to state capital	261.5	247.2	14.32***	4.457
Agricultural workers in 2000	0.427	0.427	0.001	0.006
Life expectancy in 2000	68.49	68.31	0.182*	0.107
Infant mortality in 2000	32.36	33.09	-0.722**	0.387
Population with sewage in 2000	0.126	0.145	-0.018***	0.004
Population with electricity in 2000	0.874	0.855	0.019***	0.005
HDI in 2000	0.525	0.520	0.004	0.003
Radio station in 2005	0.164	0.174	-0.009	0.010
Government website in 2006	0.428	0.412	0.016	0.013
Public library in 2005	0.858	0.838	0.019**	0.010
Local bus in 2005	0.248	0.250	-0.002	0.012
Municipal guard in 2006	0.116	0.164	-0.048***	0.009
Criminal court in 2006	0.248	0.239	0.009	0.012
Special civil court in 2004	0.326	0.288	0.037***	0.012
Touristic area in 2005	0.174	0.194	-0.020**	0.010
Industrial district in 2006	0.228	0.199	0.028***	0.011
Economic incentives in 2006	0.498	0.490	0.008	0.013
Zoning law in 2005	0.206	0.201	0.005	0.011
Education council in 2005	0.677	0.673	0.004	0.013
Civil defense council in 2005	0.260	0.243	0.017	0.012
Municipalities	2,950	2,589		

Notes: The table reports the mean characteristics of all the municipalities of our sample and out-of-the-sample municipalities measured before the year of the shock. Column (1) reports the mean for the municipalities included in the main analysis. Column (2) reports the mean for municipalities that are not included in the main analysis. Column (3) reports the difference in means and column (4) presents the standard error of the difference. The political and mayors' characteristics were constructed using data from Brazil's electoral commission (*Tribunal Superior Eleitoral*). The socioeconomic characteristics of the municipalities were constructed using data from IBGE and TCU. *Education high* is a dummy variable equal to 1 if mayor has attended college or university; *First term* is a dummy variable equal to 1 if mayor is in his/her first-term in office; *Member party* is a dummy variable equal to 1 if mayor belongs to party, where party: PMDB, PSDB, PT, PP, PFL, PTB are major political parties in Brazil and accounts for approximately 73% of the mayors in 2005-2008; *Aligned with federal government* is a dummy variable equal to 1 if mayor belongs to any of the political parties of the formal federal government coalition (PT, PRB, PCdoB). (*Population in 2006*) is the municipal estimated population in 2006; *Population growth in 2003-2006* is the annual mean growth rate of municipal estimated population between 2003 and 2006; *Population density in 2006* is municipal estimated population in 2006 divided by municipal area; *Urban population in 2000* is the share of 2000 census population that live in urban areas; *College degree adults in 2000* is the share of 2000 census population above 25 years with college degree; *Literacy adults in 2000* is the share of 2000 census population above 25 years that is literate; *College degree public adm. in 2006* is the share of 2006 local government employees with college degree; *Agricultural workers in 2000* is the share of 2000 census workers occupied in the agricultural sector; *Life expectancy in 2000* is the life expectancy at birth measured in 2000 census; *Life expectancy in 2000* is the life expectancy at birth measured in 2000 census; *Infant mortality in 2000* is the number of children who won't survive the first year of life in every 1,000 children born alive measured in 2000 census; *HDI in 2000* is the municipal Human Development Index measured with 2000 census; *Radio station in 2005* is a dummy variable equal to 1 if municipality has a radio station in 2005; *Government website in 2006* is a dummy variable equal to 1 if the municipal government has its own website in 2006; *Public library in 2005* is a dummy variable equal to 1 if the municipality has a public library in 2005; *Local bus in 2005* is a dummy variable equal to 1 if the municipality has a public bus in 2005; *Municipal guard in 2006* is a dummy variable equal to 1 if the municipality has a local police in 2006; *Criminal court in 2006* is a dummy variable equal to 1 if municipality has a criminal court in 2006; *Special civil court in 2004* is a dummy variable equal to 1 if municipality has a special civil court in 2004; *Touristic area in 2005* is a dummy variable equal to 1 if municipality was declared as a tourist area in 2005; *Industrial district in 2006* is a dummy variable equal to 1 if municipality has an industrial district in 2006; *Economic incentives in 2006* is a dummy variable equal to 1 if municipality provides economic incentives to business in 2006; *Zoning law in 2005* is a dummy variable equal to 1 if municipality has a zoning law in 2005; *Education council in 2006* is a dummy variable equal to 1 if municipality has an education council in 2006; *Civil defense council in 2006* is a dummy variable equal to 1 if municipality has a civil defense council in 2006. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.2
Actual FPM transfers

Table B.2: Actual and theoretical FPM transfers

	Log		Level	
Outcome Variable	Actual FPM (1)	Actual FPM (2)	Actual FPM (3)	Actual FPM (4)
Log theoretical FPM	1.006***	0.847***		
	(0.003)	(0.026)		
Level theoretical FPM			0.946***	0.920***
			(0.018)	(0.038)
Municipality FE	No	Yes	No	Yes
Observations	23,600	23,600	23,600	23,600
R-squared	0.993	0.953	0.952	0.850

Notes: Each cell reports the estimated coefficient of actual FPM transfers on theoretical FPM transfers. We expressed actual and theoretical FPM in logs and levels. All regressions include year and state fixed effects. In even columns municipality fixed effects are included. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B.1: Formula transfer (actual FPM)

(a) positive shock

(b) negative shock

Notes: The figure shows the results from equation 2-1 for actual FPM transfers. The top panel (a) shows effects for the positive shock and the bottom panel (b) shows effects for the negative shock. 95% confidence intervals indicated around the point estimates.

B.3
Other reduced form estimates

Table B.3: Reduced form - Revenues and expenditures: Mayor education heterogeneity

	Positive Shock		Negative Shock	
Outcome	ln(*expenditures*)	ln(*revenues*)	ln(*expenditures*)	ln(*revenues*)
Variables	(1)	(2)	(3)	(4)
Shock X 2008	0.007	0.078***	-0.070***	-0.063***
	(0.043)	(0.010)	(0.017)	(0.011)
Shock X 2009	0.093***	0.100***	-0.059***	-0.054***
	(0.010)	(0.010)	(0.010)	(0.010)
Shock X 2010	0.095***	0.095***	-0.049***	-0.039***
	(0.014)	(0.012)	(0.012)	(0.012)
Shock X 2011	0.079***	0.085***	-0.042***	-0.028**
	(0.016)	(0.015)	(0.012)	(0.013)
Shock X 2012	0.074***	0.088***	-0.035***	-0.020
	(0.016)	(0.016)	(0.013)	(0.014)
Shock X 2008 X High Education	0.039	-0.026	0.034	0.032*
	(0.048)	(0.025)	(0.023)	(0.019)
Shock X 2009 X High Education	-0.020	-0.014	0.040**	0.058***
	(0.025)	(0.026)	(0.018)	(0.019)
Shock X 2010 X High Education	-0.008	-0.006	0.043**	0.042**
	(0.027)	(0.027)	(0.020)	(0.020)
Shock X 2011 X High Education	0.016	0.017	0.041**	0.040*
	(0.029)	(0.029)	(0.020)	(0.021)
Shock X 2012 X High Education	0.026	0.022	0.043*	0.043*
	(0.030)	(0.030)	(0.022)	(0.022)
Shock X After	0.072***	0.093***	-0.044***	-0.041***
	(0.014)	(0.011)	(0.009)	(0.009)
Shock X After X High Education	0.022	0.018	0.038***	0.038***
	(0.019)	(0.017)	(0.014)	(0.014)
Observations	20,704	20,704	21,056	21,056
Municipalities	2,588	2,588	2,632	2,632

Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. In odd columns the dependent variable is the log of municipal expenditures, while in even columns is the log of total municipal revenues. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Reduced form - Tax collection: Local heterogeneities

	Positive Shock															
Interaction	First Term		Political Alignment		Gini		Adults Education		Literacy		Income pc		Urban Population		Radio	
Outcome	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Shock X 2008	0.068**	0.066**	0.043	0.045	-0.003	-0.002	0.020	0.023	0.015	0.015	0.021	0.019	0.044	0.046	-0.040	-0.033
	(0.028)	(0.029)	(0.030)	(0.031)	(0.044)	(0.044)	(0.086)	(0.084)	(0.083)	(0.080)	(0.084)	(0.081)	(0.086)	(0.084)	(0.092)	(0.088)
Shock X 2009	0.063	0.066	0.043	0.047	0.039	0.040	0.081*	0.085*	0.058	0.059	0.065	0.067	0.063	0.069	0.028	0.044
	(0.045)	(0.045)	(0.032)	(0.033)	(0.050)	(0.051)	(0.046)	(0.046)	(0.043)	(0.042)	(0.044)	(0.044)	(0.045)	(0.044)	(0.043)	(0.043)
Shock X 2010	0.080**	0.083**	0.055*	0.056*	0.015	0.010	0.093**	0.096**	0.097**	0.097**	0.107***	0.105***	0.087**	0.087**	0.039	0.052
	(0.038)	(0.038)	(0.032)	(0.033)	(0.050)	(0.051)	(0.040)	(0.041)	(0.038)	(0.038)	(0.039)	(0.040)	(0.039)	(0.040)	(0.038)	(0.039)
Shock X 2011	0.100**	0.108**	0.081**	0.084**	0.062	0.063	0.124**	0.126**	0.122**	0.122**	0.134***	0.131**	0.107**	0.112**	0.058	0.076
	(0.049)	(0.051)	(0.038)	(0.039)	(0.056)	(0.057)	(0.051)	(0.052)	(0.048)	(0.049)	(0.050)	(0.051)	(0.049)	(0.050)	(0.047)	(0.049)
Shock X 2012	0.085*	0.092*	0.079**	0.077*	0.068	0.073	0.134**	0.134**	0.136***	0.129**	0.153***	0.143***	0.119**	0.124**	0.041	0.063
	(0.052)	(0.054)	(0.039)	(0.040)	(0.060)	(0.061)	(0.055)	(0.056)	(0.051)	(0.052)	(0.052)	(0.054)	(0.053)	(0.054)	(0.051)	(0.053)
Shock X 2008 X Interaction	-0.121	-0.117	-0.372	-0.377	0.006	-0.020	-0.040	-0.016	-0.029	-0.008	-0.042	-0.024	-0.094	-0.137**	0.082	0.156*
	(0.092)	(0.080)	(0.367)	(0.364)	(0.093)	(0.081)	(0.097)	(0.106)	(0.093)	(0.079)	(0.094)	(0.079)	(0.096)	(0.060)	(0.101)	(0.092)
Shock X 2009 X Interaction	-0.064	-0.067	-0.134	-0.137	-0.010	-0.044	-0.109*	-0.116*	-0.063	0.016	-0.079	-0.038	-0.075	-0.031	-0.003	0.051
	(0.063)	(0.064)	(0.118)	(0.122)	(0.065)	(0.063)	(0.063)	(0.068)	(0.064)	(0.084)	(0.063)	(0.080)	(0.063)	(0.062)	(0.063)	(0.065)
Shock X 2010 X Interaction	-0.056	-0.068	-0.062	-0.073	0.064	0.019	-0.092	-0.014	-0.105*	-0.016	-0.128**	-0.113	-0.081	-0.033	0.012	0.068
	(0.058)	(0.058)	(0.083)	(0.085)	(0.062)	(0.060)	(0.060)	(0.071)	(0.061)	(0.093)	(0.060)	(0.095)	(0.060)	(0.056)	(0.059)	(0.060)
Shock X 2011 X Interaction	-0.051	-0.058	-0.086	-0.095	0.020	-0.033	-0.105	-0.050	-0.101	-0.007	-0.127*	-0.118	-0.075	-0.028	0.019	0.077
	(0.068)	(0.068)	(0.075)	(0.076)	(0.071)	(0.073)	(0.068)	(0.088)	(0.069)	(0.105)	(0.068)	(0.109)	(0.068)	(0.074)	(0.068)	(0.072)
Shock X 2012 X Interaction	-0.019	-0.033	-0.036	-0.045	0.010	-0.052	-0.117*	-0.041	-0.120*	-0.020	-0.155**	-0.151	-0.094	-0.058	0.053	0.129*
	(0.071)	(0.070)	(0.084)	(0.084)	(0.075)	(0.080)	(0.070)	(0.084)	(0.071)	(0.101)	(0.070)	(0.103)	(0.070)	(0.071)	(0.071)	(0.073)
Shock X After	0.077**	0.084***	0.074***	0.076***	0.053*	0.050*	0.089**	0.096**	0.077**	0.076**	0.086**	0.085**	0.079**	0.087**	0.031	0.047
	(0.031)	(0.032)	(0.023)	(0.023)	(0.029)	(0.030)	(0.037)	(0.037)	(0.035)	(0.035)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
Shock X After X Interaction	-0.036	-0.042	-0.152	-0.155*	0.014	-0.008	-0.063	-0.069	-0.034	0.029	-0.057	-0.059	-0.044	-0.031	0.045	0.086*
	(0.045)	(0.043)	(0.095)	(0.094)	(0.045)	(0.045)	(0.044)	(0.060)	(0.043)	(0.061)	(0.044)	(0.065)	(0.044)	(0.044)	(0.045)	(0.048)
Other Interactions	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	20,704	20,704	20,704	20,704	20,704	20,704	20,704	20,704	20,704	20,704	20,704	20,704	20,704	20,704	20,704	20,704
Municipalities	2,588	2,588	2,588	2,588	2,588	2,588	2,588	2,588	2,588	2,588	2,588	2,588	2,588	2,588	2,588	2,588

Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. The dependent variable is the log of municipal tax revenues. First term refers to municipalities where the elected mayor in 2008 is in its first term in office. Political alignment refers to municipalities where the elected mayor in 2008 is affiliated to the same political party than the Brazilian President (Labor Part - PT). Gini refers to the Gini coefficient of the municipality. Adults education refers to the share of adults with college degree in the municipality. Literacy refers to the literacy rate of the municipality. Income pc refers to the income per capita of the municipality. Urban population refers to the share of urban population of the municipality. Radio indicates whether the municipality has a local radio station. We expressed all continuous variables included in the interactions as indicators of above/below (1/0) the median of the distribution of sample municipalities. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Reduced form - Tax collection: Local heterogeneities

	Negative Shock															
Interaction	First Term		Political Alignment		Gini		Adults Education		Literacy		Income pc		Urban Population		Radio	
Outcome	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Shock X 2008	0.028	0.016	-0.013	-0.016	-0.045	-0.053*	-0.056	-0.055	-0.026	-0.023	-0.031	-0.030	-0.056	-0.059	-0.060**	-0.062**
	(0.040)	(0.037)	(0.026)	(0.025)	(0.030)	(0.030)	(0.037)	(0.038)	(0.032)	(0.033)	(0.032)	(0.034)	(0.038)	(0.039)	(0.030)	(0.030)
Shock X 2009	0.050	0.036	0.040	0.032	-0.012	-0.018	0.014	0.011	0.016	0.022	0.022	0.026	0.027	0.025	-0.014	-0.012
	(0.045)	(0.044)	(0.027)	(0.027)	(0.028)	(0.030)	(0.039)	(0.040)	(0.043)	(0.043)	(0.043)	(0.043)	(0.039)	(0.039)	(0.038)	(0.039)
Shock X 2010	0.089*	0.067	0.078***	0.067**	0.018	-0.001	0.041	0.027	0.053	0.051	0.055	0.054	0.075	0.070	0.009	0.005
	(0.048)	(0.047)	(0.030)	(0.030)	(0.033)	(0.035)	(0.042)	(0.042)	(0.047)	(0.047)	(0.048)	(0.049)	(0.046)	(0.046)	(0.042)	(0.042)
Shock X 2011	0.035	0.005	0.044	0.030	-0.014	-0.031	0.002	-0.019	-0.013	-0.021	0.000	-0.007	0.028	0.023	-0.049	-0.050
	(0.050)	(0.050)	(0.033)	(0.033)	(0.036)	(0.037)	(0.049)	(0.048)	(0.048)	(0.047)	(0.053)	(0.052)	(0.053)	(0.052)	(0.049)	(0.048)
Shock X 2012	0.069	0.021	0.060*	0.037	0.001	-0.023	0.033	0.015	0.018	0.004	0.033	0.017	0.053	0.046	-0.010	-0.013
	(0.052)	(0.050)	(0.034)	(0.034)	(0.036)	(0.037)	(0.053)	(0.052)	(0.054)	(0.052)	(0.058)	(0.057)	(0.056)	(0.055)	(0.052)	(0.051)
Shock X 2008 X Interaction	-0.070	-0.053	-0.017	-0.018	0.068	0.083	0.073	0.067	0.016	-0.023	0.025	-0.053	0.067	0.062	0.078*	0.044
	(0.049)	(0.047)	(0.061)	(0.062)	(0.048)	(0.054)	(0.048)	(0.086)	(0.046)	(0.090)	(0.046)	(0.126)	(0.049)	(0.064)	(0.046)	(0.051)
Shock X 2009 X Interaction	-0.027	-0.003	-0.057	-0.074	0.106**	0.136**	0.035	-0.008	0.027	0.069	0.018	-0.033	0.005	-0.026	0.076	0.060
	(0.054)	(0.052)	(0.074)	(0.073)	(0.053)	(0.062)	(0.051)	(0.070)	(0.053)	(0.083)	(0.053)	(0.102)	(0.052)	(0.063)	(0.051)	(0.052)
Shock X 2010 X Interaction	-0.039	-0.006	-0.114	-0.112	0.110*	0.129*	0.044	0.047	0.019	0.047	0.018	-0.015	-0.023	-0.082	0.088	0.085
	(0.059)	(0.057)	(0.072)	(0.073)	(0.057)	(0.070)	(0.056)	(0.093)	(0.058)	(0.127)	(0.059)	(0.153)	(0.058)	(0.072)	(0.056)	(0.060)
Shock X 2011 X Interaction	-0.002	0.052	-0.087	-0.091	0.109*	0.154**	0.056	-0.009	0.072	0.141	0.052	-0.014	0.003	-0.095	0.132**	0.122*
	(0.063)	(0.063)	(0.077)	(0.079)	(0.063)	(0.076)	(0.062)	(0.098)	(0.062)	(0.167)	(0.064)	(0.182)	(0.065)	(0.073)	(0.063)	(0.066)
Shock X 2012 X Interaction	-0.021	0.041	-0.016	-0.024	0.126*	0.180**	0.041	-0.036	0.058	0.130	0.033	-0.001	-0.002	-0.083	0.096	0.085
	(0.065)	(0.062)	(0.089)	(0.089)	(0.065)	(0.076)	(0.065)	(0.088)	(0.066)	(0.157)	(0.068)	(0.165)	(0.067)	(0.069)	(0.065)	(0.065)
Shock X After	0.013	0.005	0.021	0.012	-0.024	-0.033	-0.007	-0.014	0.005	0.009	0.010	0.012	0.016	0.020	-0.031	-0.026
	(0.032)	(0.032)	(0.021)	(0.021)	(0.023)	(0.024)	(0.033)	(0.033)	(0.035)	(0.034)	(0.037)	(0.036)	(0.036)	(0.035)	(0.034)	(0.034)
Shock X After X Interaction	0.010	0.043	-0.026	-0.022	0.099**	0.122**	0.045	0.046	0.019	0.059	0.012	-0.046	-0.002	-0.040	0.073*	0.065
	(0.041)	(0.042)	(0.062)	(0.063)	(0.040)	(0.047)	(0.040)	(0.061)	(0.042)	(0.093)	(0.043)	(0.105)	(0.042)	(0.055)	(0.041)	(0.044)
Other Interactions	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21,056	21,056	21,056	21,056	21,056	21,056	21,056	21,056	21,056	21,056	21,056	21,056	21,056	21,056	21,056	21,056
Municipalities	2,632	2,632	2,632	2,632	2,632	2,632	2,632	2,632	2,632	2,632	2,632	2,632	2,632	2,632	2,632	2,632

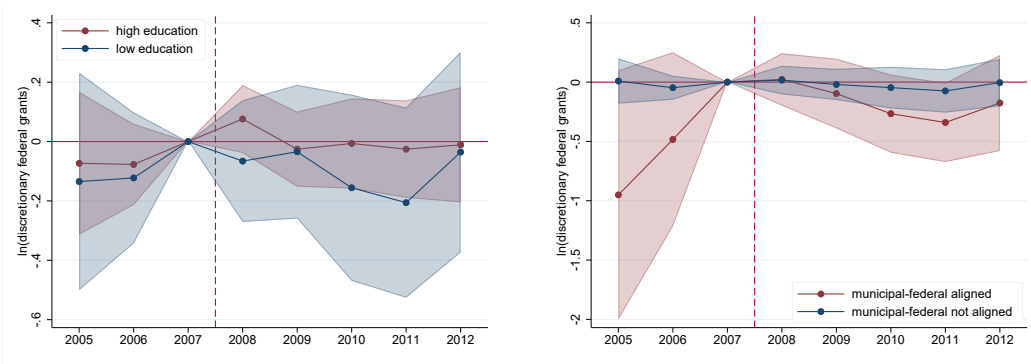
Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. The dependent variable is the log of municipal tax revenues. First term refers to municipalities where the elected mayor in 2008 is in its first term. Political alignment refers to municipalities where the elected mayor in 2008 is affiliated to the same political party than the Brazilian President (Labor Part - PT). Gini refers to the Gini coefficient of the municipality. Adults education refers to the share of adults with college degree in the municipality. Literacy refers to the literacy rate of the municipality. Income pc refers to the income per capita of the municipality. Urban population refers to the share of urban population of the municipality. Radio indicates whether the municipality has a local radio station. We expressed all continuous variables included in the interactions as indicators of above/below (1/0) the median of the distribution of sample municipalities. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Reduced form - Discretionary federal grants

	Positive Shock	Negative Shock
Outcome Variables	ln(*discret. fed grants*) (1)	ln(*discret. fed grants*) (2)
Shock X 2008	0.015	0.019
	(0.056)	(0.077)
Shock X 2009	-0.030	-0.001
	(0.061)	(0.096)
Shock X 2010	-0.071	0.033
	(0.081)	(0.096)
Shock X 2011	-0.103	0.095
	(0.085)	(0.093)
Shock X 2012	-0.021	0.110
	(0.093)	(0.119)
Shock X After	0.022	-0.007
	(0.076)	(0.067)
Observations	20,704	21,056
Municipalities	2,588	2,632

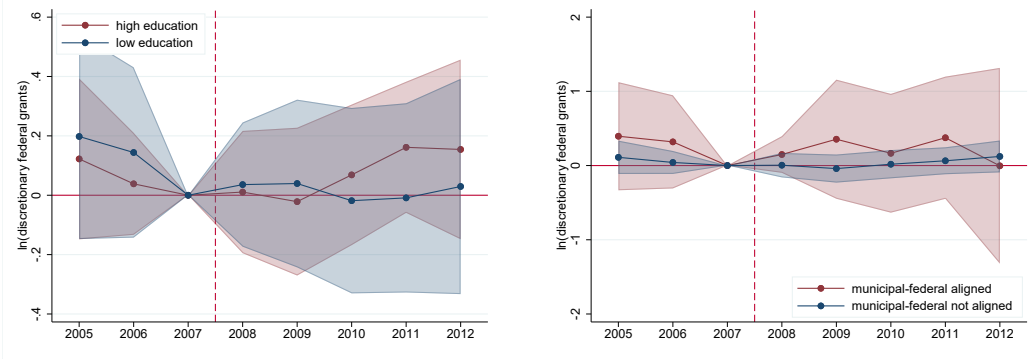
Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. The dependent variable is the log value of the discretionary federal grant, smoothed across the number of year of the grant agreement. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B.2: Discretionary federal grants: Mayor education and political alignment heterogeneities



(a) positive shock - mayor education

(b) positive shock - political alignment



(c) negative shock - mayor education

(d) negative shock - political alignment

Notes: The figure shows the results from equation 2-1 for revenues from discretionary federal grants, interacting the shock with an indicator of the human capital of the elected mayor in 2008 (left panel) and interacting the shock with an indicator of whether the elected mayor in 2008 is from the same political party than the Brazilian President (right panel). Red illustrates municipalities where the elected mayor in 2008 has at least high school education, and municipalities where the elected mayor in 2008 is politically aligned with the federal government. Blue illustrates municipalities where the elected mayor in 2008 has at most high school education, and municipalities where the elected mayor in 2008 is politically aligned with the federal government. The top panel shows effects for the positive shock and the bottom panel shows effects for the negative shock. 95% confidence intervals indicated around the point estimates. Table B.7 present the corresponding estimates.

Table B.7: Reduced form - Discretionary federal grants: Heterogeneities

	Positive Shock		Negative shock	
Outcome Variables	ln(*discret. fed grants*) (1)	ln(*discret. fed grants*) (2)	ln(*discret. fed grants*)	ln(*discret. fed grants*)
Shock X 2008	-0.066	0.017	0.036	0.005
	(0.104)	(0.062)	(0.107)	(0.084)
Shock X 2009	-0.034	-0.020	0.039	-0.041
	(0.115)	(0.067)	(0.144)	(0.097)
Shock X 2010	-0.156	-0.046	-0.018	0.018
	(0.160)	(0.090)	(0.159)	(0.097)
Shock X 2011	-0.206	-0.075	-0.009	0.064
	(0.163)	(0.093)	(0.162)	(0.093)
Shock X 2012	-0.036	-0.004	0.030	0.123
	(0.172)	(0.102)	(0.185)	(0.110)
Shock X 2008 X Interaction	0.142	0.006	-0.025	0.144
	(0.120)	(0.127)	(0.150)	(0.150)
Shock X 2009 X Interaction	0.009	-0.077	-0.061	0.396
	(0.132)	(0.163)	(0.192)	(0.417)
Shock X 2010 X Interaction	0.149	-0.220	0.087	0.147
	(0.178)	(0.189)	(0.200)	(0.416)
Shock X 2011 X Interaction	0.180	-0.266	0.170	0.310
	(0.183)	(0.193)	(0.198)	(0.426)
Shock X 2012 X Interaction	0.025	-0.172	0.125	-0.126
	(0.199)	(0.228)	(0.241)	(0.675)
Shock X After	-0.015	-0.013	-0.079	-0.007
	(0.131)	(0.074)	(0.087)	(0.071)
Shock X After X Interaction	0.066	0.300	0.110	-0.008
	(0.158)	(0.330)	(0.128)	(0.220)
Interaction - Mayor Education	Yes	No	Yes	No
Interaction - Political Alignment	No	Yes	No	Yes
Observations	20,704	20,704	21,056	21,056
Municipalities	2,588	2,588	2,632	2,632

Notes: Each cell reports the estimated yearly coefficient of an event study model, and the average effect for the entire post-treatment period of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007. The dependent variable is the log value of the discretionary federal grant, smoothed across the number of year of the grant agreement. Interaction Mayor Education refers to municipalities where the elected mayor in 2008 has more than high school education. Interaction Political Alignment refers to municipalities where the elected mayor in 2008 is from the same political party than the Brazilian President (Labor Party - PT). All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.4
Instrumental variables estimates

Table B.8: IV - Revenues and expenditures

	Positive Shock		Negative Shock	
Outcome Variables	ln(*expenditures*) (1)	ln(*revenues*) (2)	ln(*expenditures*) (3)	ln(*revenues*) (4)
Shock X 2008	0.084	0.185***	-0.101***	-0.088***
	(0.066)	(0.043)	(0.025)	(0.023)
Shock X 2009	0.237***	0.268***	-0.066***	-0.035*
	(0.044)	(0.046)	(0.020)	(0.021)
Shock X 2010	0.257***	0.261***	-0.042**	-0.023
	(0.046)	(0.045)	(0.021)	(0.021)
Shock X 2011	0.267***	0.290***	-0.030	-0.006
	(0.050)	(0.050)	(0.019)	(0.020)
Shock X 2012	0.268***	0.306***	-0.015	0.012
	(0.050)	(0.052)	(0.022)	(0.021)
Shock X After	0.386***	0.471***	-0.179***	-0.144**
	(0.042)	(0.041)	(0.066)	(0.062)
Observations	20,704	20,704	21,056	21,056
Municipalities	2,588	2,588	2,632	2,632
F-Test	1760.0	1760.0	427.4	427.4

Notes: Each cell reports the estimated yearly coefficient, and the average effect for the entire post-treatment period, of theoretical FPM (instrumented by crossing a population threshold after the update of local population in 2007). In odd columns the dependent variable is the log of municipal expenditures, while in even columns is the log of total municipal revenues. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.9: IV - Tax collection

	Positive Shock	Negative Shock
Outcome Variables	ln(*taxes*) (1)	ln(*taxes*) (2)
Shock X 2008	0.001	-0.030
	(0.146)	(0.048)
Shock X 2009	0.081	0.064
	(0.091)	(0.048)
Shock X 2010	0.137	0.124**
	(0.083)	(0.051)
Shock X 2011	0.216**	0.061
	(0.102)	(0.053)
Shock X 2012	0.224**	0.103*
	(0.106)	(0.055)
Shock X After	0.260**	0.171
	(0.104)	(0.177)
Observations	20,704	21,056
Municipalities	2,588	2,632
F-Test	1760.0	427.4

Notes: Each cell reports the estimated yearly coefficient, and the average effect for the entire post-treatment period (last row), of theoretical FPM (instrumented by crossing a population threshold after the the update of local population in 2007). The dependent variable is the log of total municipal taxes. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.10: IV - Tax collection: Mayor education heterogeneity

	Positive Shock		Negative Shock	
Outcome	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)
Variables	(1)	(2)	(3)	(4)
Shock X 2008	-0.040	-0.047	-0.153*	-0.193*
	(0.302)	(0.379)	(0.088)	(0.115)
Shock X 2009	0.045	0.064	-0.109	-0.121
	(0.136)	(0.176)	(0.080)	(0.106)
Shock X 2010	0.179*	0.234*	0.056	0.082
	(0.104)	(0.140)	(0.087)	(0.117)
Shock X 2011	0.197	0.277*	-0.076	-0.108
	(0.122)	(0.168)	(0.093)	(0.120)
Shock X 2012	0.239*	0.312*	-0.057	-0.121
	(0.131)	(0.179)	(0.103)	(0.131)
Shock X 2008 X High Education	0.074	0.032	0.178*	0.143
	(0.322)	(0.259)	(0.107)	(0.110)
Shock X 2009 X High Education	0.064	0.109	0.256**	0.264**
	(0.183)	(0.193)	(0.101)	(0.112)
Shock X 2010 X High Education	-0.075	-0.083	0.105	0.101
	(0.162)	(0.173)	(0.109)	(0.128)
Shock X 2011 X High Education	0.036	0.015	0.204*	0.184
	(0.197)	(0.220)	(0.114)	(0.122)
Shock X 2012 X High Education	-0.024	-0.038	0.235*	0.247*
	(0.204)	(0.227)	(0.123)	(0.129)
Shock X After	0.179	0.164	-0.211	-0.270
	(0.159)	(0.159)	(0.192)	(0.194)
Shock X after X High Education	0.152	0.205	0.754**	0.708*
	(0.210)	(0.206)	(0.351)	(0.391)
Other Interactions	No	Yes	No	Yes
Observations	20,704	20,704	21,056	21,056
Municipalities	2,588	2,588	2,632	2,632
F-Test	914.0	257.5	232.9	68.1

Notes: Each cell reports the estimated yearly coefficient, and the average effect for the entire post-treatment period, of theoretical FPM (instrumented by crossing a population threshold after the update of local population in 2007). The dependent variable is the log of municipal tax revenues. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. Other interactions include: first term in office, political party alignment with the federal government (Labor Party - PT), Gini index, share of adults with college degree, literacy rates, per capita income, share of urban population and existence of local radio stations. We expressed all continuous variables included in the interactions as indicators of above/below (1/0) the median of the distribution of sample municipalities. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.11: IV - Tax workers: Mayor education heterogeneity

	Positive Shock			
Outcome	Intensive Margin		Wage Bill	
Variable	(1)	(2)	(3)	(4)
Shock X 2008	0.005	0.062	-0.675	-0.604
	(0.082)	(0.111)	(0.547)	(0.750)
Shock X 2009	-0.093	-0.092	-0.920	-1.062
	(0.124)	(0.164)	(0.871)	(1.159)
Shock X 2010	-0.202	-0.229	-1.606	-1.904
	(0.136)	(0.178)	(1.015)	(1.352)
Shock X 2011	-0.162	-0.171	-0.964	-1.116
	(0.146)	(0.196)	(1.093)	(1.476)
Shock X 2012	-0.032	-0.019	-0.010	0.081
	(0.171)	(0.228)	(1.124)	(1.527)
Shock X 2008 X High Education	-0.078	-0.080	0.271	0.476
	(0.119)	(0.118)	(0.830)	(0.790)
Shock X 2009 X High Education	0.123	0.064	1.252	0.939
	(0.173)	(0.186)	(1.241)	(1.259)
Shock X 2010 X High Education	0.188	0.221	1.857	2.037
	(0.185)	(0.206)	(1.379)	(1.466)
Shock X 2011 X High Education	0.242	0.285	1.562	2.094
	(0.212)	(0.225)	(1.529)	(1.644)
Shock X 2012 X High Education	0.297	0.349	1.937	2.203
	(0.244)	(0.259)	(1.617)	(1.700)
Shock X After	-0.041	-0.026	-0.726	-0.499
	(0.152)	(0.151)	(1.026)	(1.028)
Shock X After X High Education	-0.083	-0.091	0.615	0.648
	(0.243)	(0.244)	(1.721)	(1.756)
Other Interactions	No	Yes	No	Yes
Observations	20,704	20,704	20,704	20,704
Municipalities	2,588	2,588	2,588	2,588
F-Test	913.9	257.6	913.9	257.6

Notes: Each cell reports the estimated yearly coefficient, and the average effect for the entire post-treatment period, of theoretical FPM (instrumented by crossing a population threshold after the update of local population in 2007). In columns 1 and 2, the dependent variable is the log of the total number of tax auditors and/or tax technicians employed by municipality i in year t. In columns 3 and 4, the dependent variable is the log of the total salaries paid to tax auditors and/or tax technicians by municipality i in year t. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. Other interactions include: first term in office, political party alignment with the federal government (Labor Party - PT), Gini index, share of adults with college degree, literacy rates, per capita income, share of urban population and existence of local radio stations. We expressed all continuous variables included in the interactions as indicators of above/below (1/0) the median of the distribution of sample municipalities. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.12: IV - Tax workers: Mayor education heterogeneity

	Negative Shock			
Outcome	Intensive Margin		Wage Bill	
Variable	(1)	(2)	(3)	(4)
Shock X 2008	-0.012	0.028	-0.745	-0.672
	(0.087)	(0.116)	(0.610)	(0.836)
Shock X 2009	-0.111	-0.108	-1.756**	-1.983*
	(0.123)	(0.163)	(0.806)	(1.085)
Shock X 2010	-0.196	-0.209	-2.079**	-2.271*
	(0.145)	(0.189)	(0.900)	(1.190)
Shock X 2011	-0.073	-0.069	-1.189	-1.247
	(0.125)	(0.163)	(0.822)	(1.087)
Shock X 2012	0.039	0.042	-0.323	-0.301
	(0.139)	(0.182)	(0.988)	(1.303)
Shock X 2008 X High Education	0.058	0.070	0.775	0.754
	(0.107)	(0.112)	(0.697)	(0.728)
Shock X 2009 X High Education	0.288*	0.327**	2.386***	2.440**
	(0.148)	(0.165)	(0.920)	(1.004)
Shock X 2010 X High Education	0.390**	0.418**	2.682***	2.359**
	(0.175)	(0.182)	(1.040)	(1.063)
Shock X 2011 X High Education	0.243	0.222	1.533	1.354
	(0.154)	(0.170)	(0.959)	(1.037)
Shock X 2012 X High Education	0.184	0.147	0.938	0.653
	(0.171)	(0.186)	(1.126)	(1.212)
Shock X After	0.016	0.080	-1.524	-0.892
	(0.319)	(0.329)	(2.105)	(2.200)
Shock X after X High Education	0.955	1.109*	3.725	4.938
	(0.600)	(0.670)	(3.334)	(3.530)
Other Interactions	No	Yes	No	Yes
Observations	21,056	21,056	21,056	21,056
Municipalities	2,632	2,632	2,632	2,632
F-Test	233.0	68.1	233.0	68.1

Notes: Each cell reports the estimated yearly coefficient, and the average effect for the entire post-treatment period, of theoretical FPM (instrumented by crossing a population threshold after the update of local population in 2007). In columns 1 and 2, the dependent variable is the log of the total number of tax auditors and/or tax technicians employed by municipality i in year t. In columns 3 and 4, the dependent variable is the log of the total salaries paid to tax auditors and/or tax technicians by municipality i in year t. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. Other interactions include: first term in office, political party alignment with the federal government (Labor Party - PT), Gini index, share of adults with college degree, literacy rates, per capita income, share of urban population and existence of local radio stations. We expressed all continuous variables included in the interactions as indicators of above/below (1/0) the median of the distribution of sample municipalities. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.5
Robustness

B.5.1
Population controls

Table B.13: RF & IV - Tax collection

	Positive Shock		Negative Shock	
	RF	IV	RF	IV
Outcome Variables	ln(*taxes*) (1)	ln(*taxes*) (2)	ln(*taxes*) (3)	ln(*taxes*) (4)
Shock X 2008	-0.011	-0.044	-0.022	-0.038
	(0.051)	(0.152)	(0.024)	(0.054)
Shock X 2009	0.010	0.015	0.027	0.057
	(0.032)	(0.102)	(0.026)	(0.055)
Shock X 2010	0.040	0.090	0.047*	0.097
	(0.030)	(0.097)	(0.028)	(0.063)
Shock X 2011	0.059*	0.150	0.014	0.035
	(0.034)	(0.120)	(0.031)	(0.067)
Shock X 2012	0.058	0.142	0.037	0.076
	(0.036)	(0.133)	(0.032)	(0.070)
Shock X After	0.003	0.013	0.046**	0.449**
	(0.024)	(0.116)	(0.019)	(0.190)
Population Controls	Yes	Yes	Yes	Yes
Observations	20,704	20,704	21,056	21,056
Municipalities	2,588	2,588	2,632	2,632
F-Test	-	2346.0	-	529.6

Notes: Each cell reports the estimated yearly coefficient and the average effect for the entire post-treatment period: i) of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007 (columns 1 and 3); ii) of theoretical FPM instrumented by crossing a population threshold after the update of local population in 2007 (columns 2 and 4). The dependent variable is the log of municipal tax revenues. Population controls include up to third order population polynomials interacted with state dummies. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.14: RF & IV - Tax collection: Mayor education heterogeneity

	Positive Shock		Negative Shock	
	RF	IV	RF	IV
Outcome	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)
Variables	(1)	(2)	(3)	(4)
Shock X 2008	-0.016	-0.067	-0.058	-0.150
	(0.102)	(0.301)	(0.035)	(0.093)
Shock X 2009	0.014	0.006	-0.038	-0.094
	(0.050)	(0.162)	(0.036)	(0.090)
Shock X 2010	0.065*	0.142	0.020	0.032
	(0.037)	(0.130)	(0.039)	(0.103)
Shock X 2011	0.064	0.142	-0.038	-0.094
	(0.041)	(0.153)	(0.043)	(0.111)
Shock X 2012	0.079*	0.174	-0.028	-0.076
	(0.045)	(0.182)	(0.047)	(0.126)
Shock X 2008 X High Education	0.012	0.048	0.057	0.165
	(0.109)	(0.324)	(0.048)	(0.116)
Shock X 2009 X High Education	0.001	0.028	0.109**	0.232**
	(0.066)	(0.214)	(0.050)	(0.116)
Shock X 2010 X High Education	-0.045	-0.103	0.046	0.102
	(0.058)	(0.198)	(0.055)	(0.132)
Shock X 2011 X High Education	-0.011	0.003	0.087	0.198
	(0.066)	(0.244)	(0.061)	(0.140)
Shock X 2012 X High Education	-0.035	-0.063	0.105*	0.231
	(0.070)	(0.274)	(0.063)	(0.154)
Shock X After	0.011	0.049	0.007	0.050
	(0.035)	(0.156)	(0.028)	(0.210)
Shock X After X High Education	-0.009	-0.039	0.062*	0.732**
	(0.046)	(0.218)	(0.038)	(0.357)
Population Controls	Yes	Yes	Yes	Yes
Observations	20,704	20,704	21,056	21,056
Municipalities	2,588	2,588	2,632	2,632
F-Test	-	1199.0	-	275.0

Notes: Each cell reports the estimated yearly coefficient and the average effect for the entire post-treatment period: i) of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007 (columns 1 and 3); ii) of theoretical FPM instrumented by crossing a population threshold after the update of local population in 2007 (columns 2 and 4). The dependent variable is the log of municipal tax revenues. Population controls include up to third order population polynomials interacted with state dummies. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

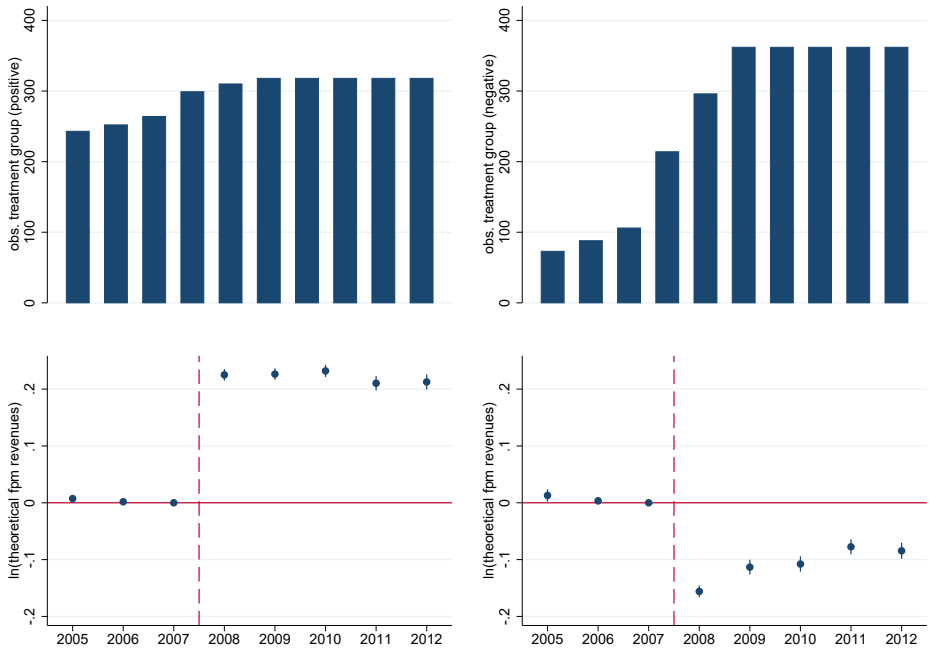
B.5.2
Dynamic Sample

Instead of holding the set of treatment municipalities fixed even if they crossed a population threshold in the pre-treatment years, we define an alternative sample in which treatment municipalities enter the study in the pre-treatment year after they have crossed any threshold for the last time. As before, we keep all municipalities at treatment in the post period, even though some of them might have shifted backward.

We do not use this design to define our main sample considering the possibility that part of the findings could be explained by a composition effect. The group of municipalities included in the pre-treatment period is changing (increasing) year after year and, therefore, the estimates might be capturing differences arising from the changes in the group of treatment municipalities instead than from the transfer shock.

The top panel of figure B.3 shows the number of treated observations included in each year, whereas the bottom panel shows the results from equation 2-1 for theoretical FPM transfers. The left panel analyzes the positive shock and right panel the negative one. For both sets, pre-trends do not exists as the estimated coefficient before 2008 is generally insignificant and the point estimate around zero. From 2008 onwards, however, our instrument starts to work as municipalities face the consequences of the positive (negative) update of their population number, which shuffled them around the bracket cut-off. This effect reverts slightly for the negative shock because of natural population growth. Some municipalities which were placed to the left (but probably close to the threshold) will switch back in subsequent years.

Figure B.3: Sample and formula transfer (theoretical FPM)



Notes: Dynamic sample. Instead of holding the set of treatment municipalities fixed even if they crossed a population threshold in the pre-treatment years, we define an alternative sample in which treatment municipalities enter the study in the pre-treatment year after they have crossed any threshold for the last time. As before, we keep all municipalities at treatment in the post period, even though some of them might have shifted backward. The top panel shows the count of treated observations in the sample. The bottom panel the results from equation 2-1 for theoretical FPM transfers. The left panel show effects for the positive shock and the right panel for the negative shock. 95% confidence intervals indicated around the point estimates.

Table B.15: RF & IV - Tax collection

	Positive Shock		Negative Shock	
	RF	IV	RF	IV
Outcome	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)	ln(*taxes*)
Variables	(1)	(2)	(3)	(4)
Shock X 2008	0.003	0.009	-0.006	-0.009
	(0.051)	(0.147)	(0.027)	(0.057)
Shock X 2009	0.031	0.088	0.043	0.083
	(0.032)	(0.092)	(0.029)	(0.056)
Shock X 2010	0.051*	0.144*	0.076**	0.143**
	(0.030)	(0.084)	(0.031)	(0.060)
Shock X 2011	0.074**	0.224**	0.044	0.079
	(0.035)	(0.103)	(0.033)	(0.060)
Shock X 2012	0.077**	0.232**	0.067*	0.122*
	(0.036)	(0.106)	(0.034)	(0.062)
Shock X After	0.060**	0.273**	0.031	0.280
	(0.023)	(0.107)	(0.024)	(0.214)
Observations	20,623	20,623	20,586	20,586
Municipalities	2,588	2,588	2,632	2,632
F-Test	-	1709.0	-	360.7

Notes: Dynamic sample. Instead of holding the set of treatment municipalities fixed even if they crossed a population threshold in the pre-treatment years, we define an alternative sample in which treatment municipalities enter the study in the pre-treatment year after they have crossed any threshold for the last time. As before, we keep all municipalities at treatment in the post period, even though some of them might have shifted backward. Each cell reports the estimated yearly coefficient and the average effect for the entire post-treatment period: i) of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007 (columns 1 and 3); ii) of theoretical FPM instrumented by crossing a population threshold after the update of local population in 2007 (columns 2 and 4). The dependent variable is the log of municipal tax revenues. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.16: RF & IV - Tax collection

	Positive Shock		Negative Shock	
	RF	IV	RF	IV
Outcome Variables	ln(*taxes*) (1)	ln(*taxes*) (2)	ln(*taxes*) (3)	ln(*taxes*) (4)
Shock X 2008	-0.010	-0.029	-0.057	-0.146
	(0.107)	(0.304)	(0.038)	(0.098)
Shock X 2009	0.020	0.056	-0.043	-0.102
	(0.048)	(0.137)	(0.038)	(0.090)
Shock X 2010	0.068*	0.190*	0.029	0.062
	(0.037)	(0.104)	(0.043)	(0.102)
Shock X 2011	0.069*	0.208*	-0.031	-0.069
	(0.041)	(0.123)	(0.044)	(0.097)
Shock X 2012	0.083*	0.250*	-0.022	-0.050
	(0.044)	(0.132)	(0.049)	(0.112)
Shock X 2008 X High Education	0.024	0.067	0.077	0.192
	(0.114)	(0.324)	(0.052)	(0.121)
Shock X 2009 X High Education	0.019	0.058	0.136**	0.269**
	(0.065)	(0.185)	(0.054)	(0.115)
Shock X 2010 X High Education	-0.030	-0.081	0.072	0.117
	(0.058)	(0.163)	(0.060)	(0.127)
Shock X 2011 X High Education	0.009	0.029	0.118*	0.216*
	(0.066)	(0.198)	(0.063)	(0.124)
Shock X 2012 X High Education	-0.010	-0.031	0.138**	0.247*
	(0.069)	(0.206)	(0.067)	(0.136)
Shock X After	0.039	0.164	-0.029	-0.205
	(0.039)	(0.163)	(0.033)	(0.228)
Shock X After X High Education	0.036	0.204	0.096**	0.915**
	(0.048)	(0.215)	(0.046)	(0.416)
Observations	20,623	20,623	20,586	20,586
Municipalities	2,588	2,588	2,632	2,632
F-Test	-	884.3	-	191.9

Notes: Dynamic sample. Instead of holding the set of treatment municipalities fixed even if they crossed a population threshold in the pre-treatment years, we define an alternative sample in which treatment municipalities enter the study in the pre-treatment year after they have crossed any threshold for the last time. As before, we keep all municipalities at treatment in the post period, even though some of them might have shifted backward. Each cell reports the estimated yearly coefficient and the average effect for the entire post-treatment period: i) of a difference-in-difference model, where treatment is equal to one for municipalities that crossed a population threshold after the update of local population in 2007 (columns 1 and 3); ii) of theoretical FPM instrumented by crossing a population threshold after the update of local population in 2007 (columns 2 and 4). The dependent variable is the log of municipal tax revenues. High Education refers to municipalities where the elected mayor in 2008 has more than high school education. All regressions include year fixed effects, municipality fixed effects and dummies for jumps in the pre-treatment period (2005-2007). Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C
Chapter 3

C.1
Additional results

Table C.1: Data description by year - Median

		Median				
Year	Munic-Year	Population	Contracts	Hours	Wage	N Layers
2003	5,281	10,642	153.00	284,106	6.67	3.00
2004	5,313	10,719	158.00	296,347	6.36	3.00
2005	5,381	10,817	177.00	334,799	6.13	3.00
2006	5,424	10,872	188.00	360,855	6.40	3.00
2007	5,426	10,883	199.00	377,445	6.41	3.00
2008	5,413	11,283	223.00	425,295	6.30	3.00
2009	5,427	11,360	241.00	457,327	6.29	3.00
2010	5,457	11,035	243.00	462,069	6.21	3.00
2011	5,455	11,097	255.00	480,558	5.98	3.00
2012	5,456	11,192	252.00	476,193	6.05	3.00
Total	5,532	11,016	208.00	395,472	6.26	3.00

Notes: The table reports for each year, the number of municipalities in the data set and the corresponding median across all municipalities for selected variables. Contracts is the median number of annual contracts from RAIS. Hours is the median number of total annual hours from RAIS. Wage is the median hourly wage from RAIS in 2012 reais. N Layers is the median number of layers across municipalities in each year.

Table C.2: Discretionary federal grants estimates - Without outliers

	Pooled				Within			
	ln (grants)		quantity		*ln* (grants)		quantity	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Number of Layers	0.078***	0.100***	0.123***	0.147**	0.041*	0.078***	0.055*	0.096**
	(0.020)	(0.027)	(0.043)	(0.060)	(0.023)	(0.029)	(0.031)	(0.040)
Observations	44,350	38,950	44,350	38,950	44,350	38,950	44,350	38,950
Number of Fixed Effects	-	-	-	-	4,435	3,895	4,435	3,895
R2	0.274	0.285	0.379	0.376	0.147	0.154	0.175	0.178

Notes: Pooled and within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t. We restrict the sample to municipalities within the 99th percentile of the annual change of wages and contracts. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. *ln* (grants) represents the log value of the grant, smoothed across the number of years of the grant agreement. Quantity represents the number of grant agreements signed by the municipality in year t. All regressions include year fixed effects, state fixed effects, year-state fixed effects, and second order population polynomials. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Discretionary federal grants estimates - Lag dependent Variable

	Pooled				Within			
	ln (grants)		quantity		*ln* (grants)		quantity	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Dependent Variable t-1	0.602***	0.599***	0.642***	0.643***	0.362***	0.354***	0.293***	0.288***
	(0.011)	(0.012)	(0.038)	(0.038)	(0.013)	(0.014)	(0.039)	(0.042)
Number of Layers	0.045***	0.056***	0.046***	0.053**	0.038**	0.070***	0.061**	0.099***
	(0.010)	(0.014)	(0.017)	(0.026)	(0.017)	(0.024)	(0.028)	(0.038)
Observations	43,830	37,314	43,830	37,314	43,830	37,314	43,830	37,314
Number of Fixed Effects	-	-	-	-	4,870	4,146	4,870	4,146
R2	0.548	0.553	0.649	0.652	0.270	0.273	0.244	0.242

Notes: Pooled and within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. *ln* (grants) represents the log value of the grant, smoothed across the number of years of the grant agreement. Quantity represents the number of grant agreements signed by the municipality in year t. All regressions include past values of the dependent variable, year fixed effects, state fixed effects, year-state fixed effects, and second order population polynomials. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: Discretionary federal grants estimates - Number of bureaucrats

	Pooled				Within			
	ln (grants)		quantity		*ln* (grants)		quantity	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Number of Layers	0.117***	0.087***	0.209***	0.068	0.037*	0.070**	0.045	0.108***
	(0.020)	(0.026)	(0.045)	(0.067)	(0.021)	(0.028)	(0.029)	(0.039)
Observations	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460
Number of Fixed Effects	-	-	-	-	4,870	4,146	4,870	4,146
R2	0.200	0.261	0.299	0.299	0.143	0.151	0.161	0.166

Notes: Pooled and within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. *ln* (grants) represents the log value of the grant, smoothed across the number of years of the grant agreement. Quantity represents the number of grant agreements signed by the municipality in year t. All regressions include year fixed effects, state fixed effects, year-state fixed effects, and second order polynomials of the number of bureaucrats in the organizational structure. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Discretionary federal grants estimates - Political alignment

	Pooled				Within			
	ln (grants)		quantity		*ln* (grants)		quantity	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Number of Layers	0.085***	0.108***	0.110***	0.131**	0.037*	0.068**	0.050*	0.095**
	(0.019)	(0.026)	(0.041)	(0.063)	(0.021)	(0.028)	(0.029)	(0.039)
Observations	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460
Number of Fixed Effects	-	-	-	-	4,870	4,146	4,870	4,146
R2	0.269	0.280	0.396	0.391	0.145	0.153	0.173	0.175

Notes: Pooled and within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. *ln* (grants) represents the log value of the grant, smoothed across the number of years of the grant agreement. Quantity represents the number of grant agreements signed by the municipality in year t. All regressions include a dummy variable which takes the value of one when the municipal mayor is affiliated to the same political party than the Brazilian President (Labor Party - PT), year fixed effects, state fixed effects, year-state fixed effects, and second order population polynomials. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.6: Discretionary federal grants estimates - Alternative definition

	Pooled				Within			
	ln (grants I)		*ln* (grants II)		*ln* (grants I)		*ln* (grants II)	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Number of Layers	0.164***	0.195***	0.161***	0.183***	0.052	0.112*	0.071	0.131*
	(0.041)	(0.053)	(0.043)	(0.055)	(0.050)	(0.068)	(0.055)	(0.070)
Observations	48,700	41,460	44,350	38,950	48,700	41,460	44,350	38,950
Number of Fixed Effects	-	-	-	-	4,870	4,146	4,870	3,895
R2	0.205	0.205	0.206	0.208	0.150	0.149	0.148	0.147

Notes: Pooled and within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. *ln* (grants I) represents the log value of the grant signed in year t. *ln* (grants II) represents the log value of the grant signed in year t restricting the sample to municipalities within the 99th percentile of the annual change of wages and contracts. All regressions include year fixed effects, state fixed effects, year-state fixed effects, and second order population polynomials. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.7: Discretionary federal grants within estimates by ministry - *ln (grants)*

	Agriculture		Cities		Education		Health		Nat. Integration	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)
Number of Layers	0.027	0.032	0.076	0.124*	0.041	0.115	0.011	0.050	0.098**	0.133*
	(0.054)	(0.073)	(0.055)	(0.073)	(0.057)	(0.078)	(0.050)	(0.066)	(0.050)	(0.070)
Observations	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460
Number of Fixed Effects	4,870	4,146	4,870	4,146	4,870	4,146	4,870	4,146	4,870	4,146
R2	0.122	0.127	0.150	0.156	0.169	0.163	0.060	0.062	0.131	0.136
	Rural Dev.		Science & Tech.		Social Dev.		Sports		Tourism	
Variables	(6a)	(6b)	(7a)	(7b)	(8a)	(8b)	(9a)	(9b)	(10a)	(10b)
Number of Layers	-0.042	-0.050	0.028	0.016	0.083*	0.034	-0.042	0.066	0.043	0.143**
	(0.048)	(0.065)	(0.028)	(0.041)	(0.049)	(0.069)	(0.059)	(0.081)	(0.053)	(0.072)
Observations	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460
Number of Fixed Effects	4,870	4,146	4,870	4,146	4,870	4,146	4,870	4,146	4,870	4,146
R2	0.079	0.074	0.132	0.138	0.052	0.054	0.065	0.069	0.359	0.366

Notes: Within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. *ln* (grants) represents the log value of the grant, smoothed across the number of years of the grant agreement. All regressions include year fixed effects, state fixed effects, year-state fixed effects, and second order population polynomials. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.8: Discretionary federal grants within estimates by ministry - *quantity*

	Agriculture		Cities		Education		Health		Nat. Integration	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)
Number of Layers	0.006	-0.000	0.018**	0.025**	0.014	0.034***	0.009	0.016	0.008*	0.009
	(0.005)	(0.007)	(0.009)	(0.012)	(0.010)	(0.013)	(0.011)	(0.014)	(0.005)	(0.007)
Observations	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460
Number of Fixed Effects	4,870	4,146	4,870	4,146	4,870	4,146	4,870	4,146	4,870	4,146
R2	0.072	0.073	0.153	0.156	0.093	0.094	0.154	0.154	0.140	0.145
	Rural Dev.		Science & Tech.		Social Dev.		Sports		Tourism	
Variables	(6a)	(6b)	(7a)	(7b)	(8a)	(8b)	(9a)	(9b)	(10a)	(10b)
Number of Layers	-0.003	-0.004	-0.001	0.004	0.003	0.010	-0.001	-0.000	0.006	0.014
	(0.004)	(0.006)	(0.004)	(0.004)	(0.005)	(0.007)	(0.005)	(0.007)	(0.008)	(0.011)
Observations	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460	48,700	41,460
Number of Fixed Effects	4,870	4,146	4,870	4,146	4,870	4,146	4,870	4,146	4,870	4,146
R2	0.076	0.079	0.034	0.036	0.082	0.084	0.045	0.048	0.151	0.157

Notes: Within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. Quantity represents the number of grant agreements signed by the municipality in year t. All regressions include year fixed effects, state fixed effects, year-state fixed effects, and second order population polynomials. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.9: Discretionary federal grants estimates - Span of control without outliers

	Pooled				Within			
	ln (grants)		quantity		*ln* (grants)		quantity	
Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Number of Layers	0.001	-0.005	0.020	-0.006	0.008	0.009	0.025	0.012
	(0.039)	(0.043)	(0.069)	(0.078)	(0.040)	(0.045)	(0.052)	(0.060)
Span of Control	-0.301**	-0.487***	-0.422*	-0.757**	-0.145	-0.365**	-0.105	-0.398*
	(0.142)	(0.170)	(0.252)	(0.315)	(0.135)	(0.162)	(0.192)	(0.236)
Number of Layers X Span of Control	0.110**	0.169***	0.142*	0.243**	0.042	0.106**	0.044	0.128*
	(0.044)	(0.052)	(0.081)	(0.099)	(0.043)	(0.050)	(0.061)	(0.074)
P-Value	0.000	0.000	0.002	0.002	0.042	0.000	0.061	0.005
Observations	44,350	38,950	44,350	38,950	44,350	38,950	44,350	38,950
Number of Fixed Effects	-		-		4,435	3,895	4,435	3,895
R2	0.274	0.286	0.379	0.376	0.147	0.155	0.175	0.178

Notes: Pooled and within model estimates, where the key regressor is the number of knowledge layers of the municipality in year t interacted with the span of control of the highest layer. Span of control is defined as number of employees in layer $l-1$ divided by the number of employees in layer l. We express the span of control as an indicator of above/below (1/0) the yearly median distribution of sample municipalities. We restrict the sample to municipalities within the 99th percentile of the annual change of wages and contracts. In columns (a) it is assumed that one layers municipalities have one supervisor (the mayor). In columns (b) one layers municipalities are excluded. *ln* (grants) represents the log value of the grant, smoothed across the number of years of the grant agreement. quantity represents the number of grant agreements signed by the municipality in year t. All regressions include year fixed effects, state fixed effects, year-state fixed effects and second order population polynomials. Robust standard errors clustered at the municipality level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; p-value of (Number of Layers + Number of Layers X Span of Control) $\neq 0$ is reported in the bottom part of the table.