**Voluntary Audits: Experimental Evidence on a New Approach to Monitoring Front-Line Bureaucrats**[1]

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ABSTRACT

Can bureaucratic effort and motivation be improved by voluntary, rather than mandatory, forms of oversight? Drawing on insights from psychology and public administration, we argue that voluntary forms of oversight increase bureaucrats' sense of autonomy, and therefore their motivation and effort. Empirically, we work with a provincial auditing body in Argentina to implement an encouragement design in which school principals are invited to participate in a voluntary audit of their administration of a free school meal program. We employ a two-level randomization, in which areas are first assigned to a high or low density of invitations, and then schools are randomly assigned to treatment or control. Contrary to conventional expectations that bureaucrats resist oversight, we find that approximately one-third of school principals accept the invitation to participate in a voluntary audit. We find divergent effects of treatment based on the density of treatment; in low density areas, we find the anticipated increase in motivation and a small decrease in school closings. In contrast, in high density areas, we observe the opposite effect. Drawing on qualitative interview data, we speculate that a high density of invitations may generate pressure to accept the invitation and therefore undermine the positive effects of volunteering.

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## I. Introduction

Can voluntary forms of oversight improve public sector employees' motivation, performance and even social outcomes? Street-level bureaucrats (Lipsky 1980) are those public-sector employees working at the front lines, providing services to citizens in schools, health centers, and welfare offices.[2] While these public employees do not formulate policy, they are central to citizen experiences with the state and the quality of public goods provision, even as they often work with limited resources and for low wages. As a result, a growing literature in economics and political science seeks to understand the factors that affect frontline bureaucrats' motivation and effort in their jobs, especially in lower and middle-income countries (Bertelli et al. 2020; Finan, Olken, and Pande 2017; Nathan and White 2021; Pepinsky, Pierskalla, and Sacks 2017). Most of this work focuses on mandatory forms of oversight designed to punish shirking and corruption and motivate effort.

In this paper, we develop and explore the dynamics and consequences of a new possible approach to bureaucratic oversight—offering bureaucrats the opportunity to *volunteer* for an audit. Drawing on literature from psychology and public administration, we argue that the opportunity to volunteer for oversight offers bureaucrats autonomy and the opportunity to give and receive feedback, which are associated with greater job satisfaction and intrinsic motivation (Esteve and Schuster 2019; Ryan and Deci 2000). Contrary to traditional principal-agent models, we therefore expect positive take-up of an invitation to volunteer for oversight, and positive effects of such an invitation on bureaucrats' motivation and effort.

Empirically, we partner with the Provincial Auditing Office (PAO) in Chaco, Argentina, which has authority to oversee school principals' implementation of a free-meal program and typically fulfills its oversight responsibility via a very small number of mandatory, surprise audits of schools. Our study employs two-level randomization and an encouragement design. Within our study group, we first assign areas to a high or low density of treatment, and then within areas, we randomly assign schools principals to receive an invitation to a voluntary audit from the PAO or to be part of the control group, where schools face a small (but non-zero) likelihood of a mandatory audit.[3] We find about one-third of school principals accept the invitation to an audit, and we do not document any systematic differences between those who accept and do not accept the invitation.

We measure the effects of the intervention with a combination of administrative data and a survey of school principals carried out approximately eight months after the intervention.[4] Focusing on the intent to treat effect, we find that receiving the invitation to a voluntary audit increases motivation and decreases school closings in the low-density treatment areas only. In contrast, we find unexpected negative effects on motivation and effort in high-density treatment areas. We use qualitative interviews to explore the reasons for the relatively high take-up rate of the invitation and for the divergent effects of treatment across low versus high density areas. These interviews suggest that the desire to give feedback to policymakers encourages volunteering for oversight. In high-density areas, we also

[2] Throughout this paper, we refer interchangeably to "street-level bureaucrats" (SLB) and "frontline service providers" (FLSP).

[3] We use the term "areas" and "regions" interchangeably in the paper—these are a mix of municipalities and, in the case of the provincial capital, researcher-defined geographically compact zones within that city.

[4] Hypotheses were pre-registered with EGAP after the intervention and while endline data collection was ongoing (and before the PIs had access to any of the endline data).

speculate that the higher rate of invitations and communication between principals creates peer pressure on treated school principals to accept the invitation and may thereby undermine the posited mechanism through which the invitation can increase a sense of autonomy.

To our knowledge, this is the first attempt to explore the dynamics and consequences of a voluntary form of oversight of frontline service providers in the developing world. Our work contributes to a growing literature that points to the promise of non-financial incentives for motivating bureaucratic effort. It demonstrates that some bureaucrats will voluntarily accept oversight and suggests a new avenue for motivating bureaucratic effort in contexts where bureaucrats feel isolated from policy-makers.

**II. The importance and difficulty of motivating front-line bureaucrats**

Front-line service providers are crucial to the nature and quality of government services that citizens receive. A substantial literature, much of it focused on the United States, demonstrates the consequences for citizens of their interactions with these street-level bureaucrats. For example, teacher quality affects student learning (e.g., Rivkin, Hanushek, and Kain 2005), experiences with welfare state officials affects access to benefits and attitudes towards the state (e.g., Maynard-Moody and Musheno 2003; Michener 2018), and encounters with police affect individuals' lives, political attitudes, and behavior (Epp, Maynard-Moody, and Haider-Markel 2014; Soss and Weaver 2017; Weaver, Prowse, and Piston 2019). Moving outside the US, the consequences of bureaucratic motivation and effort for citizens are likely to be equally, if not more important, in lower and middle-income countries. Recent decades have seen the expansion of direct welfare provision to the poor in many lower and middle-income countries (e.g., World Bank 2018)., while, at the same time, constraints on state resources, presence, and capacity in these contexts mean these mandates are not fulfilled in a uniform manner (Kruks-Wisner 2018). These phenomena increase the influence of the street-level bureaucrats charged with providing many of these services. As Pepinsky, Pierskalla and Sacks (2017) write, increasing decentralization and direct service delivery means that front-line bureaucrats have "become a central plank in the global development agenda in the past 30 years."

Given the importance of these FLSP, what are the factors that affect street-level bureaucrats' motivation, effort, and performance? Incentivizing and controlling front-line civil servants presents a number of challenges. Street-level bureaucrats, as Lipsky (1980) described in his classic work, are dispersed through space, conduct their work "in the field," enjoy discretion over their day to day activities, and are faced with many conflicting demands on their time. Furthermore, the nature of employment contracts in the public sector makes it difficult to tie compensation to performance (Esteve and Schuster 2019; Finan, Olken, and Pande 2017). Even where such compensation schemes are possible, the nature of front-line work makes it difficult to identify employee output separately from outcomes (Wilson 1989), with the result that such schemes may lead to perverse incentives for street-level bureaucrats (see the summary in Finan, Olken, and Pande 2017, 471).

Although these challenges affect bureaucracies everywhere, they are likely to be more acute in many middle and low income countries. Lack of adequate resources can make it difficult for bureaucrats to carry out their jobs and is also linked to decreased motivation (Mathauer and Imhoff 2006; WHO 2006, chap. 3). Poor public infrastructure means that bureaucrats are more likely to work in areas that are remote or inaccessible (e.g., Tendler 1997), making direct supervision difficult. State institutions

charged with oversight functions may themselves be constrained by limited resources and capacity, and in many contexts the judiciary is extremely slow or dysfunctional (i.e., Helmke and Rios-Figueroa 2011). As evidence of the difficulty of motivating these critical workers, absenteeism among street-level bureaucrats—especially teachers and health-care workers—is a well-documented and widespread phenomenon in many lower and middle-income countries (Chaudhury et al. 2006).

A recent and growing experimental literature on front-line service providers in the developing world draws on the principal-agent framework to explore a variety of strategies for improving bureaucratic performance in these contexts (see Finan, Olken and Pande (2017), Pepinksy, Pierskall and Sacks (2017), and Dustan, Maldonado and Hernandez-Agramonte (2018) for reviews). A number of these studies examine the effects of financial or material incentives, often in combination with mandatory monitoring, on bureaucratic effort. There is some evidence that financial incentives can work -- for example, they generate gains in teacher and health worker attendance and student outcomes in India (Duflo, Hanna, and Ryan 2012; Muralidharan and Sundararaman 2011). However, this research program also shows substantial evidence of the limitations and unanticipated consequences of this approach to motivating bureaucrats. In some cases, gains are limited to only the specific incentivized metric (e.g., Glewwe, Ilias, and Kremer 2010), or to certain districts depending on their political relevance (Callen et al. 2016). Elsewhere, monitoring and incentives may even produce perverse effects, wherein front-line bureaucrats may collude with their supervisors (e.g., Banerjee, Duflo, and Glennerster 2008) or citizens (Khan, Khwaja, and Olken 2016) to subvert and undermine monitoring systems. These mixed results suggest the limits of traditional principal-agent models for understanding the sources of bureaucratic effort and motivation.

Our own work builds on a small but growing experimental literature that explores the responsiveness of bureaucrats and other public-service providers to non-financial inducements, information, and novel forms of oversight.[5] For example, in Zambia, Ashraf, Bandiera, and Jack (2014) find that private citizens are most incentivized to provide a public service (subsidized condoms) by a non-financial reward—a star chart displayed publicly in their salons. Dustan and colleagues (2018) find that messages appealing to social norms improve compliance within the education bureaucracy in Peru.[6] And in a study of procurement officers in Pakistan, Bandiera and colleagues (2020) find that increased bureaucratic autonomy improved efficiency more than performance pay. Together, this work suggests the utility of looking beyond traditional approaches to bureaucratic oversight.

*A Theory of Voluntary Oversight*

We build on this emerging literature, as well as established findings from psychology and public administration, to explore how street-level bureaucrats will respond to the opportunity to *volunteer* for

[5] Separately, a long-standing literature in public administration (Perry, Hondeghem, and Wise 2010; Perry and Wise 1990) and a more recent literature in economics (e.g., Banuri and Keefer 2016; Dal Bó, Finan, and Rossi 2013) point to the importance of public service motivation and explore the effects of recruitment strategies on bureaucratic effort and motivation.

[6] Dustan and colleagues find that informational reminders and threats of monitoring improve performance, as well. Also note that Dal Bó, Finan, and Rossi (2013) and Ashraf, Bandiera, and Lee (2016) find that stronger financial incentives (rather than appealing to pro-social norms) results in the recruitment of higher performing employees and do not crowd out those with pro-social attitudes.

oversight that is not tied to financial rewards or career advancement. The principal-agent framework suggests that bureaucrats will shirk unless they are closely monitored and offered incentives or the risk of punitive action tied to their effort. Under this framework, we should not expect bureaucrats would ever voluntarily accept oversight, especially if it is linked to the possibility of some punitive outcome if performance is poor and offers no possibility of reward for good performance. However, looking outside the P-A framework, there are a number of reasons to believe that at least some bureaucrats will accept an invitation to participate in voluntary forms of oversight, and that voluntary forms of oversight may increase motivation and effort.

We know from psychology that individuals value tasks that provide them with a sense of autonomy, relatedness to others, and competence (Deci and Ryan 1985; Ryan and Deci 2000). There is ample evidence from across the social sciences that people report greater job satisfaction and exert more effort in jobs – in both the private and public sector—that address these three psychological needs (see the summary in Cassar and Meier (2018)). We argue that being invited to participate in a voluntary oversight scheme creates an opportunity for street-level bureaucrats to exercise autonomy and feel competent in their work—two of the key psychological needs identified by Ryan and Deci.

A voluntary oversight process creates an opportunity for self-direction and increases autonomy by giving bureaucrats a real choice as to whether to receive oversight or not. This is in sharp contrast with traditional forms of oversight, which are obligatory and are commonly associated with bureaucratic resentment and/or efforts by bureaucrats to resist oversight (Lipsky 1980, 19; Prottas 1978).

Voluntary oversight also offers bureaucrats an opportunity to feel competent and to display that competence to others. As Ellingsen and Johannesson (2007) argue, both personal and social recognition foster a sense of competence. The act of volunteering itself inherently creates opportunities for recognition, as SLBs who volunteer can conceive of themselves as having made a positive choice, and they can also share their decision to volunteer with others, generating social recognition. As such, even if, for example, auditors follow the same procedures whether an audit is mandatory or voluntary, bureaucrats being audited are likely to experience a greater sense of personal satisfaction and have greater possibilities for receiving social recognition when they have volunteered for that audit. Recognition is important for motivation; for example, according to Wiley (1997)"full appreciation for work done" is the only element that consistently ranked among the top two dimensions that motivate U.S. workers throughout the post-World War II period (cited in Ellingsen and Johannesson 2007).

In addition to anticipating this sense of competence and wanting to exercise their autonomy, some bureaucrats might accept an invitation to participate in voluntary oversight when they believe it serves as a setting through which they can offer meaningful feedback to policy-makers. Although it has received somewhat less attention that other factors, studies of public sector workers show that they value the opportunity to give input into policy formulation and decision-making (Petter et al. 2002; Rasul and Rogger 2018).[7] Accepting an invitation for oversight might present street-level bureaucrats with the opportunity to share their experiences "on the ground" with those farther up the hierarchy, and thus to contribute their perspective to policymakers. Volunteering to receive oversight should be especially

[7] In a study of SLBs in a state-level human services agency, Petter et al report that "[w]hen participants talked about what they like and disliked in their jobs, one of the first things they noted was wanting to be allowed input into agency or office decision making" (Petter et al. 2002, 398).

appealing in a context where oversight of any kind is relatively rare, perhaps due to limits on the capacity of oversight organizations and officials. In these contexts, bureaucrats may respond positively to the opportunity to share their performance with experts – even if the only payoff is a so-called "warm glow" rather than any concrete career rewards (Esteve and Schuster 2019).

In summary, an offer to participate in a voluntary audit offers street-level bureaucrats an opportunity to exercise autonomy, display competence, and offer policy feedback. As such, contrary to conventional understandings that bureaucrats will resist, oversight, we expect at least some bureaucrats will accept an invitation to participate in voluntary audits and that the offer of voluntary oversight will lead to increased effort and motivation.

## III. Important roles, lack of recognition: Oversight and motivation of school principals in Chaco, Argentina

We study the dynamics surrounding the take-up and consequences of voluntary oversight by conducting an experimental intervention into the oversight of school principals' implementation of a free meal program in the province of Chaco, Argentina. Located in the northeast region, Chaco is one of Argentina's poorest provinces, with a GDP per capita less than half the national average (Ministerio de Economía y Finanzas Públicas de la Nación 2015). According to the 2010 census, approximately 23% of *Chaqueños* live in households with unsatisfied basic needs, the third highest rate in the country (Ministerio de Economía y Finanzas Públicas de la Nación 2010), and Chaco is one of the few provinces where over 5% of young children were found to be underweight in a study conducted by the Ministry of Health (Dirección Nacional de Maternidad e Infancia 2006, 52). In this context, free school meals are an important policy intervention, and school principals are the key actor in the implementation of the meal program.[8] They are responsible for preparing menus, managing foodstuffs, coordinating and overseeing food preparation on site, and ensuring that food is distributed to students during each school day. These are tasks made more difficult by the fact that in Chaco, as is the case more broadly in Argentina, school canteens suffer from a lack of resources, both for program administration and to fund sufficient food to meet student needs (Diaz Langou et al. 2014).

In our study, we examine how school principals respond to an invitation to participate in a voluntary audit of their implementation of a school free meal program. This invitation comes from the provincial auditing body, which is legally charged with oversight of this program and conventionally carries out its oversight through a small number of mandatory audits. Before turning to the details of our experimental intervention, we first describe the nature of the education bureaucracy in which school principals operate, along with what we know about their main sources of oversight and motivation. Our study population involves a sample of 188 school principals in the province; in this section, we draw on our endline study of these principals, focusing on those in the Control group, as well as qualitative interviews with a smaller subset and secondary literature and interviews with local experts. We highlight that school principals' employment, possibilities for advancement, and risk of

[8] See Diaz-Langou et al (2014) and Britos et al (2016) for more details on the history of school meal programs in Argentina. A previous provincial Minister of Education shared his assessment that, given Argentina's ongoing economic crises, the food served in schools "is not a complement like previously, but instead one of the children's principle meals or even their only meal of the day" (Interview M1, June 2018).

sanction are largely, if not entirely, delinked from actual effort or performance in their jobs. Our data also show that principals have varying levels of motivation to address the needs of students and families and exert varying levels of effort in their roles.

*Bureaucratic structure: rules-based, few rewards, little communication*

Scholarship across countries notes that a lack of positive recognition within the public sector is common, and that public sector managers "often have more tools to prevent employees from doing something wrong than to motivate them to do something right" (Behn 1995; cited in Esteve and Schuster 2019, 3). This seems an apt description of Chaco's educational bureaucracy, which offers school principal few rewards and recognition and generally weak oversight overall.

Chaco's educational bureaucracy is extremely hierarchical, with limited rewards or recognition for good performance. Teacher hiring and opportunities for advancement, including promotion to administrative positions like that of school principal, follow a point-based system based on exam scores, seniority, and credentials, rather than on the job performance. In interviews, school principals and past administration officials emphasized that these rules for obtaining administrative positions within Chaco's public schools are strictly followed. School principals repeatedly emphasized that their performance on the job is not part of any advancement process and that there are no incentives or rewards, or even public recognition, for particularly good performance.[9] Indeed, a relatively common theme among school principals in qualitative interviews was a lack of recognition at all. This lack of recognition of principal effort and lack of opportunities for rewards is also reflected in quantitative data from the endline survey we carried out as part of our study. In that survey, we asked school principals how much they agreed with a series of statements that provide possible reasons they personally devote time to two key activities—overseeing the free meal program and supervising teachers. In both cases, school principals overwhelmingly disagreed with the statement that "I could receive some recognition (*reconocimiento*)" for engaging in these activities.[10]

In terms of possible punishment for poor performance, the provincial Ministry of Education does have standard processes for reviewing accusations against school principals for malfeasance, including for financial mismanagement or child abuse. In qualitative interviews, all principals we interviewed were aware of these procedures, though they are seldom used. In addition, the provincial audit body, with which we partner for our experiment, is empowered to review school principals' financial administration of any programs that employ funds from the central government. These audits are relatively rare and the PAO does not have the power to punish principals; instead, it refers possible

[9] For example, one school principal told us, "you can be an excellent principal in the way you do your job, but they don't take it into account, you go and do poorly on the test and you don't move up. They don't connect job performance with the exam" (Interview P1, June 2020). This sentiment was widely repeated.

[10] Within schools in Control, 92-93% of our respondents say it is "not at all true" or "not very true" that they engage in these activities because they could receive some recognition. The questions about motivation with respect to teacher supervision were designed as a placebo test (see the PAP) and in fact we see no effect of our intervention on responses to this question. Motivation in the meal program might be affected by treatment, as we explain below. For descriptive purposes here we report data for both outcomes for schools in Control only.

violations to the provincial Ministry of Education.[11] Data from our endline survey suggests that principals are sensitive to the possibility of consequences for malfeasance; for example, about 48% of respondents said it was true or very true that they spend time on the school meal program because they "don't want anyone from the government to sanction." Even if the true risk of sanction is small and actual sanctions are infrequent, principals do express a strong willingness to comply with rules that govern their responsibilities in their roles. Over 80% of principals say it is true or very true that they supervise teachers and spend time on the meal program because "those are the rules."

While these data suggest there is some sensitivity to oversight among school principals, other data suggests that bureaucratic consequences for effort or performance within the normal course of the job—especially for positive performance—are peripheral to the experiences of most school principals. In qualitative interviews, principals repeatedly shared that they rarely, if ever, receive feedback from the provincial Ministry of Education.[12] Quantitative data we collected is consistent with that characterization. In our endline survey, we asked principals, "At the time of being held accountable for your general performance in the school, whose opinion matters to you most?"[13] When asked to name the single most important audience, only 3% named the provincial Ministry of Education, 16% named their direct supervisors, and not a single one mentioned the PAO.[14] Although almost all principals are in regular contact with their direct supervisors (94% in our survey report being a part of a WhatsApp group with that person) there are very few mechanisms of direct accountability linked to performance on the job. In summary, the system in which school principals work is hierarchical and strongly rule-bound. It offers little to no chance of recognition for positive performance and some chance of negative consequences for poor performance.

*Self-Perceptions of Motivation and Effort*

In a context where promotion and existing oversight are largely disconnected from performance on the job, qualitative interviews and our endline survey give us insight into principals' effort and *other* sources of motivation in their work. In that survey, we ask principals a series of questions to assess their intrinsic and extrinsic motivation to work on two key tasks—supervising teachers and implementation the school meal program.[15] For both tasks, intrinsic motivation exceeds extrinsic motivation.[16] Examining schools in the control group only, for the meal program, mean internal motivation is 3.2 on a 4 point scale, versus 2.4 for external motivation. We see an even larger difference for motivation in supervising

[11] Of the 126 schools in our control group, the Tribunal selected 4, or about 3%, for a surprise, mandatory audit in 2018. As far as we are aware, none of those "normal" audits or any of those conducted as part of the experiment described here resulted in any citation or action by the Ministry of Education with respect to any of those audited.
[12] Interviews with AA, MG, VD, and M, current and former school principals and vice-principals in the province, Feb-March 2018. All expressed the view that they never receive information on their performance in the implementation of the meal program we study.
[13] Again, these responses are from the Control group only to avoid the possibility that responses are influenced by Treatment; it is worth noting that results for Treated schools are very similar.
[14] In contrast, 45% of principals named parents, 22% mentioned students, and 15% named teachers.
[15] See below and the PAP for a description of these indices; items were designated in advance as belonging to one index or the other.
[16] Building on work by Ryan and Deci (2000), we ask principals the strength of their agreement (on a four point scale) with a variety of statements intended to capture intrinsic and extrinsic motivation for engaging in these activities. We pre-registered which items correspond to internal versus external motivation.

teachers, where average intrinsic motivation across items is 3.5 on a 4-point scale, as compared to an average of 2.5 for extrinsic motivation. While internal motivation exceeds extrinsic motivation, these data also show significant variation across individuals. For example, for intrinsic motivation in the meal program, among schools in Control, the bottom 25% of principals report an average motivation level of 2.8 or below, while the top 25% report an average motivation level of 3.4 or above, a substantively significant difference over this four-point scale.

Data on effort, measured by hours worked in the past week, also provide evidence of variation across principals. When asked about hours worked, of the 122 schools assigned to Control for which we have data, 97 principals reported working more than 40 hours a week. This relatively large number of self-reported hours exists alongside significant variation—about 23% of principals in Control schools report working 40 hours or fewer in the past week, whereas about 15% reported working 55 hours or more. When asked about hours worked on the meal program in the past week, 15% of principals of schools in Control reported working 1 hour or less on the program, whereas 20% reported working 8 hours or more, with the average number of hours about 5.

These quantitative findings are also broadly consistent with our qualitative work on the meal program, in which many school principals pointed to their own significant effort, while acknowledging variation in motivation and effort exerted by their peers.[17] According to one principal, "there are directors for whom the meal program is a concern, and others for whom it is not."[18] In sum, these data suggest that while many principals in Chaco believe their work is important, invest significant effort into it, and are motivated to do so, there is also significant variation in principal motivation and effort.[19] They also show that school principals' primary sources of motivation lay outside of traditional models of top-down, punitive, mandatory oversight.

In summary, then, school principals operate in a rules-based, hierarchical system where promotion and advancement is not linked to performance in their positions, but rather seniority and test scores. School principals perceive that there are few, if any, established methods for recognition or reward based on performance, and there is some small chance of punishment for malfeasance. School principals report being frustrated by the absence of opportunities to share their experiences with policy makers and leadership in the provincial Ministry of Education. They are also operating in a context in which social needs are substantial. Motivation and effort vary across principals. Our field research thus suggests a disconnect between forms of oversight—hierarchical, punitive, infrequent—and principals' expressed relatively high levels of intrinsic motivation and their desire to share input. It is in this institutional and social context that we carry out our intervention jointly with the local auditing body.

**IV. The Intervention**

[17] Obviously, these comments should be interpreted with caution, as principals invariably compared their own greater effort to lesser effort by others. Nonetheless, they offer some additional evidence that principals themselves believe motivation and effort vary amongst their peers.

[18] Interview with P4, June 2020.

[19] This is consistent with literature that points out that many front line service providers are motivated by a desire to serve the public interest, while also acknowledging that motivation varies across individual public servants (i.e., Besley and Burgess 2002; Perry and Wise 1990).

We test our theory about the effects of voluntary oversight on school principals in Chaco through an experimental encouragement design. The Provincial Auditing Office (PAO, *Tribunal de Cuentas* in Spanish) is legally empowered to oversee the implementation of the school meal program in Chaco and carried out the intervention, in which it encouraged schools to volunteer for oversight.[20] For our intervention, we selected 188 urban and semi-urban schools across 13 regions (based on the PAO's capacity to travel and conduct audits in the province) to be included in the study.[21] First, we randomly assigned each region into high or low density treatment with a probability of .5. Then, within regions, we randomly assigned schools to treatment and control. In high density regions, the probability of treatment was .5; in low density regions, it was .16. A total of 126 schools were assigned to Control and the remaining 62 assigned to Treatment.

The treatment group was encouraged to volunteer for an audit carried out by the PAO through a written invitation, which the PAO sent on official letterhead via postal mail in July 2018 (see Appendix A for the full text of the letter). We are able to confirm that 58 of the 62 schools in Treatment received the invitation to a voluntary audit, and no schools in Control received the invitation.[22] The encouragement was quite effective: 23 out of the 62 invited school principals agreed to volunteer and all of those 23 schools received an audit, at a time coordinated between the principal and the PAO staff in advance. To formally test whether assignment to treatment lead to a voluntary audit, we specified a regression where the dependent variable takes the value of 1 if the school principal received a voluntary audit, and zero otherwise. The independent variables included assignment to treatment and region fixed effects. This model confirms that schools assigned to treatment were 35 percentage points more likely to receive a voluntary audit compared to control schools (p-value =0). The F-statistic of this model is 27.

All audits took place between August and October 2018. During the audits, PAO auditors reviewed and collected information on spending and receipts[23] and administered a short interview and questionnaire with the school principal. The questionnaire covered a variety of topics, including the frequency of unanticipated school closings, the menu served over the past five days, and the principal's knowledge of any past audits by the PAO. Upon the completion of the visit, the audit team also delivered an official document (*Acta de Cierre*) indicating that the school had volunteered to receive the audit team, describing the accounts that were audited, and noting that the audit had been completed (see Appendix B for an example).

Schools in the control group were subject to the PAO's default system of oversight; using its own criteria, the PAO additionally audited 4 of the 126 schools in this group. These audits were mandatory

[20] The research team selected the sample of schools (using PAO capacity to reach all schools as a criteria), implemented the random assignment to treatment and control, and offered logistical support (i.e., preparing photocopies, addressing envelopes, and in a few cases delivering the letter after it had been returned via postal mail) to the PAO, but the intervention was otherwise carried out entirely by the PAO and did not include any mention of the research team.

[21] The 13 "regions" include 7 municipalities and the provincial capital of Resistencia, which we divided into 6 areas based on geographic proximity.

[22] We consider a letter to be confirmed as received if we have a postal confirmation, if the school contacted the PAO in response to the letter, or the letter was hand delivered (in the case of letters which were returned to the PAO by the postal service, our local field representative delivered them in person). For the four remaining schools, we do not have evidence either way.

[23] Prior to July 2018, school principals were also responsible for food procurement.

and carried out by surprise, and the PAO did not leave an official document from the audit team at the end of these audits. Although not it's normal practice, the audit team did administer the same short questionnaire to schools in Control who received a mandatory audit.[24] We have no evidence of crossover from Control to Treatment—no school in the control group received the letter of invitation or a voluntary audit arranged in advance. In addition, no school assigned to Treatment received a mandatory, surprise audit.

From a theoretical perspective, we can think of the encouragement treatment as having two components. The invitation acknowledged the pro-social role of the school meal program and the program's importance to children in the province, and it extended an invitation to participate in the voluntary audit.[25] We believe that latter is likely to be the stronger component of the treatment. The effects of the intervention could manifest themselves at two points in time: school principals could have experienced a change in motivation and behavior in anticipation to the audit, and after the experience of the voluntary audit. As we discuss below, some of our outcomes allow us to test which of these mechanisms is present in our data.

As noted above, we expect that opportunities to volunteer for oversight can increase bureaucrats' felt autonomy, as well as their sense of recognition and competence, and consequently affect motivation and effort. To evaluate the effects of the intervention, endline data on both treatment and control schools was collected by an independent survey group approximately eight months after the completion of the intervention (see below for more details on the endline survey and outcomes). One module in the survey asked respondents to report data on attendance and school closings before and after the intervention. These data come from schools' internal records, which are collected contemporaneously. This administrative data circumvents potential problems associated to self-reported data, and offer us the opportunity to estimate the short and medium run effects of the intervention in a subset of outcomes.

For all outcomes of interest, we estimate and report the intent to treat effect, which compares schools assigned to treatment with those assigned to control, regardless of whether or not they accepted the audit.

*Balance Tests and Endline Data*

Before turning to the main outcomes of theoretical interest, we conduct a series of balance tests to compare the treatment and control groups on a variety of school-level characteristics and explore the effects of the intervention on perceived sources of oversight. We check for balance across 11 variables measured at the school level. These include some collected via administrative data (including 2016 school enrollment, the nature of meals served at the school, and the type of school), as well as two variables that capture school distance from the provincial capital. Others were collected via our own endline survey, including school closings and school attendance prior to the intervention (as recorded in contemporaneous school record books) and enumerator assessment of the

[24] The PAO presumably did this because the majority of audits they carried out during this cycle were part of our intervention, and their audit teams were regularly administering the questionnaire.

[25] In our study, all those who volunteered for an audit received one, and so for the vast majority of our outcomes, we cannot distinguish between the effect of volunteering for an audit and actually receiving one. One exception is school closings, which we examine over time, as we discuss below.

sociodemographic characteristics of the school neighborhood.[26] In Table 1, we compare values for these variables between treatment and control taking into account assignment to treatment at the school level only.[27] In Table 2, we compare values for these variables between treatment and control within low density and high density areas separately. The table shows that randomization produced good balance in baseline characteristics across the treatment and control groups. As a robustness check, we also run all the analyses presented below with control variables selected via lasso. Results are substantively similar in all cases and are presented in Tables A3, A4, and A5 in the appendix.

*Insert Tables 1, 2 about here*

As preliminary to our main data analysis, and as noted in the PAP, we anticipated that the intervention would increase school principals' felt accountability to the PAO. This expectation is not dependent on the voluntary nature of our intervention and as such does not speak to our main theoretical interests, but it does help us establish the plausibility of finding an effect from the intervention. We examine this expectation using our endline survey of school principals carried out in June-July 2019. Note that this was nearly a year after the intervention started—letters of invitation to the treatment group were sent in July 2018—and eight months after the intervention was completed (the final voluntary audits were carried out in October 2018).[28] This lag means that any changes that we find in terms of school principals' beliefs, attitudes, or self-reported effort are quite persistent. The endline survey was administered by a team from an independent public academic institution (the Escuela de Gobierno of the province of Chaco) with the approval of the provincial Ministry of Education. The survey team had no relationship with the PAO that implemented the intervention, and the survey covered a large number of topics.[29]

In order to examine accountability, we ask each principal to assess whose opinion is most important to her "at the moment of being held accountable ("*rendir cuentas*") for her general performance in the school.[30] Principals were asked to list the three most important audiences from a list of ten potential actors, including parents, supervisors, teachers, provincial ministries, and the PAO. The

[26] We did not conduct a baseline survey of school principals, as we were concerned this might affect responses to Treatment and hence the external validity of the study.

[27] Relatedly, see Appendix Tables A6, A7, and A8 for a comparison of baseline characteristics *within* the treatment group between compliers and non-compliers. Although we cannot rule out differences in unobservables, school characteristics on observable characteristics are largely indistinguishable between those who accepted the invitation to participate in a voluntary audit and those who did not. There is evidence that schools that accepted the audit have somewhat lower baseline average attendance. Recall that we rely on the ITT for all our estimations.

[28] Our original intention was to complete the endline survey in Nov 2018, but this was delayed due to bureaucratic hurdles.

[29] Only 5 school principals refused to answer the endline survey: 3 in control and 2 in treatment. Attrition is statistically indistinguishable across treatment and control. We formally test this with a regression where the dependent variable takes the value of 1 if the school principal refused to answer the survey, and zero otherwise. The independent variables are dummies for each experimental group. The F-test from this model has a p-value of 0.2243. We also find no evidence that item-level non-response is systematically related to assignment to treatment.

[30] In Spanish, "Ahora cambiando de tema, a la hora de rendir cuentas sobre su desempeño general en la escuela, ¿la opinión de quien le preocupa mas?"

principal was then asked which single actor was most important. The PAP stated our expectation that the treatment would increase the rate at which respondents identified the PAO as an important actor. Notably, any mentions of the PAO were quite rare. Indeed, not a single school principal listed the PAO when asked to name the most important audience she faced, and of the 180 respondents to our endline survey, only seven respondents (approximately 4%) listed the PAO as one of her top three audiences. Among other potential supervisory actors, mentions of the provincial Ministry of Education are also relatively low, with about 17% of respondents in the control group mentioning the Ministry. In contrast, many principals—approximately 55% in the control group—mention their direct supervisors as among their top three audiences.

Although the number of principals who name the PAO is quite small, the pattern of responses across the treatment and control groups is in the direction we would expect from our intervention. About 2.5% of principals in Control schools (3 out of 117) mention the PAO, whereas about 7% of principals in Treatment schools (4 out of 60) list the PAO as one of the top three actors whose opinion matters to them when being held accountable for their performance. To more completely examine the effects of Treatment on perceived audiences, we use regression analyses (presented in Table A5 in the appendix). Without controls, we estimate a positive, but not statistically significant effect of Treatment on naming the PAO. In a regression with controls selected via lasso, we estimate that principals in the treatment group had an about 8 percentage point increase in the likelihood of naming the PAO as one of the top three relevant actors when being held accountable for their performance, and this effect is significant at the .1 level (column 1 of the bottom panel). Given the very low share of principals who name the PAO in the control group (fewer than 3%), this is a substantively significant increase.[31] In contrast, we see no significant effect of assignment to treatment on the rate at which school principals name either supervisors or the provincial Ministry of Education as one of their job audiences. In summary, these results show that, 8 months after our intervention, being assigned to the treatment group had a persistent impact on perceived accountability to the PAO. These results help demonstrate that the intervention had lasting effects on school principals' beliefs that can be ascertained many months after the intervention.

**V. Measuring Effects on Motivation, Effort, and School-level Outcomes**

The study allows us draw causal inferences on our main theoretical question of interest: whether the introduction of voluntary audits generates a direct effect on school principals' motivation and effort, and possibly downstream effects on school-level outcomes. As outlined above, we expect that principals in the treatment group will be more motivated and exert more effort in the implementation of the school meal program compared to the control group which, in turn, could lead to improved downstream outcomes for the children attending and receiving food at these schools.[32]

[31] Column two in panel B similarly shows an increased rate of naming the PAO in treatment schools in both high and low density areas, although these differences are not significant. The results suggest an increased in expected share of school principals naming the PAO in low density areas from 4% in Control to 14% in Treatment, and an increased in the expected share of principals naming the PAO in high density areas from 0 in Control to 4% in Treatment.

[32] Appendix D copies the hypotheses and corresponding variables included in the pre-analysis plan. Note that in the PAP, we anticipated possible effects of treatment on the nature and diversity of foods served in the schools,

We consider three broad categories of outcomes. The first captures school principal's self-reported intrinsic and extrinsic motivation, both in the school meal program and, as a placebo test, in supervising teachers. The second category examines school principals' self-reported behavior, captured by their self-report of hours worked, both in the school meal program and in other areas. Finally, the third category measures school closings, which we treat as a proxy for school principals' effort.[33]

As noted above, the experiment was structured such that we first randomly assigned areas to either high density or low density of treatment, and then within each region, schools were randomly assigned to either treatment or control. Within high density areas, schools were assigned to treatment with a probability of .5; within low density areas, schools were assigned to treatment with a probability of .16.

All the analyses below, which are pre-registered in our PAP, follow the same basic structure. For each set of outcome variables, we report ITT results, which we estimate using weighted least square regressions. We weight observations by their inverse probability of being assigned to treatment. For each outcome, we first compare schools assigned to treatment and control, without taking into account a school's location in a high versus low-density treatment region. These regressions also include locality fixed effects (not reported). In the subsequent column, we examine the possibility that the effect of treatment on the outcome of interest varied between high and low density areas. We do this by including an indicator variable that takes on the value of one for high-density areas and an interaction term that takes on the value of one for schools assigned to treatment within high-density areas.[34] At the bottom of each column, we also include for reference the mean value of the outcome variable in the comparison group—using all control schools in the pooled analysis, and control schools in low density regions only in analyses that include the interaction term. We also include the p-value for the sum of assignment to treatment and the interaction term (assignment to treatment X high density)—thus indicating whether the effect of treatment is statistically significant in high density regions.

To preview our findings, we find evidence of divergent effects depending on the density of treatment. In low density areas, we see the anticipated effects, as school principals in Treatment within low density regions report increased motivation in their work in the school meal program, especially in intrinsic motivation. These treated schools in low density regions also report fewer school closings after the intervention. In contrast, we see no such effect, and even evidence of a negative effect, of assignment to treatment within high density areas on both motivation and school closings. As anticipated, we no find effect of the intervention on our placebo test, motivation to oversee school teachers. We also find no effect of Treatment on hours worked in either high or low density regions.

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and, as a result, on attendance. An administrative decision made immediately before our intervention (June 2018) changed the system of food provision from a decentralized one, wherein principals were responsible for procurement, to a centralized one, where principals receive food purchased centrally. Although we were aware of that policy change at the time of writing our PAP, we were not aware of the extent to which this would diminish principal discretion over what foods were served in school canteens. The fact that the policy change effectively eliminated principal discretion over which foods were served became clear during informal conversations carried out during the endline survey and via qualitative interviews conducted after our endline survey. Results for attendance and food composition is reported in Table A9, and we do not find any effect of treatment on either.

[33] We exclude from this measure school closings that are a result of national or regional holidays.

[34] In the appendix, we present results for each table for the same set of regressions, additionally with control variables selected via lasso.

*Motivation*

We measure principals' intrinsic and extrinsic motivation in two separate tasks: working on the meal program and supervising teachers. As noted in the PAP, we consider the latter to be a placebo test. We expect that the treatment, which focused explicitly on the meal program, will increase principal motivation with respect to the meal program, but it will have no effect on principal motivation to supervise teachers. Also as noted in the PAP, we examine intrinsic and extrinsic motivation separately, because we expect that the voluntary audit program may have differential effects on these two types of motivation. The opportunity for autonomy and self-direction created by the voluntary audit program should be particularly linked to higher intrinsic motivation (e.g., Zuckerman et al. 1978).

In order to create measures of intrinsic and extrinsic motivation for each task (working in the meal program and supervising teachers) from the endline survey, the enumerator asked the school principal to consider why she dedicated time to that task. The enumerator then read the principal a list of 15 reasons, one at a time. The principal responded on a 4 point scale, indicating the extent to which that given reason was true for her personally (ranging from "nada cierta" to "muy cierta" in Spanish).[35] The scales were constructed drawing on existing work in psychology (Ryan and Connell 1989; Ryan and Deci 2000) and each reason was pre-classified as corresponding to extrinsic or intrinsic motivation. So, for example, for the school meal program, statements that the respondent works on the program because "those are the rules" or because "I want parents to appreciate the work I do" are coded as capturing extrinsic motivation. In contrast, statements that the respondent works on the program because "I like to do it" or because "I'm interested in understanding how the canteen works" are coded as capturing intrinsic motivation. For each set of items, we calculate the mean response to create a corresponding index of motivation, and higher numbers indicate greater motivation in this area.

*Insert Table 3 about here*

Table 3 presents the results. Columns 1-4 present the results for motivation within the meal program (external motivation in columns 1-2 and internal motivation in columns 3-4), while columns 5-8 present the placebo tests, measuring external and internal motivation to supervise teachers. Note that, across both outcomes, control group intrinsic motivation is consistently higher than extrinsic motivation. Columns 1 and 3, which pool Treatment and Control across high and low-density regions, suggest no effect of treatment on motivation to work in the meal program in the pooled sample. However, once we include an additional indicator variable for Treatment locality (which takes on a value of 1 for high-density treatment areas) and an interaction term between treatment school and the high density locality indicator, as outlined in our PAP, the regression shows some significant, and divergent, effects of treatment.

[35] In each scale, 10 items corresponded to extrinsic motivation and 5 items corresponded to intrinsic motivation. Items were modified as needed so that they described the appropriate task, either teacher supervision or meal supervision. The order of items was assigned at random for each of the two tasks, and the items were presented in the same order for each task for all respondents.

Beginning with intrinsic motivation, column 4 shows that, within low density regions, school principals in the treatment group are expected to report higher levels of motivation. This increase of approximately .35 points (from a baseline of 3.13 in Control schools in low density areas) is both statistically and substantively significant effect on the 4-point scale. In contrast, the effect of Treatment on internal motivation in high-density areas is estimated to be negative. The expected motivation for control schools in high density is 3.18, whereas treated schools in high density regions are expected to report an internal motivation of 2.98. This difference is marginally significant at the .1 level, as the final row in column 4 shows. Column 2 shows a similar pattern for external motivation, although the results are weaker for low density schools. For low-density of treatment regions, assignment to Treatment is associated with an increase in motivation of .16 points on the 4-point scale, over a baseline of 2.4, although this difference is not statistically significant. Column 2 also shows that assignment to Treatment in high density areas is associated with a decrease in motivation. Compared to controls in high density, which have an expected motivation value of 2.49, schools assigned to treatment in high density are expected to have an average external motivation approximately .24 points lower. This difference is statistically significant at the $p<.05$ level.

Columns 5-8 present our placebo tests. They show, as expected, no result of the treatment on either internal or external motivation to supervise teachers. This is true within the pooled sample, as well as when we examine high and low density areas separately.

*Principal effort: Hours worked*

Next, we examine principal effort, measured through the number of hours the school principal reports working total and on a variety of specific tasks. In order to improve data validity, the endline survey asked the school principal to think about the week prior to the survey when answering these questions.[36] Principals were asked to choose from a range of hours, and then prompted to state, within this range, the specific total number of hours they had worked on "activities related to the school" in the previous week. They were then asked how many hours they spent working on the meal program, along with on a series of other tasks, including supervising teachers, speaking to parents, and working on curricular or pedagogical tasks.[37] For the present analysis, we examine the effects of treatment on total hours worked, hours worked on the meal program, and hours worked in other activities. As indicated in the PAP, we expected a positive effect of treatment on hours worked overall and in the meal program, and we had no clear expectations on how treatment might affect hours worked in other activities.[38]

*Insert Table 4 about here*

[36] Again, given the large time gap between the intervention and the survey, the only differences we will be able to detect will be long-term, persistent changes in hours worked.

[37] The survey was administered in paper, and so principals were not constrained in any way to report hours in the varied tasks that summed to the total number of hours they reported. In three cases principals reported working an implausible number of hours—80 hours in the meal program, more than 100 hours in other tasks, or 90 hours total. For the purposes of our analyses, we recode these observations to missing.

[38] The final question in this module asked the principal how many of her hours worked in the previous week she considered to be "extra." We find no significant effect of Treatment on this outcome.

Table 4 presents the results. As the summary statistics for the comparison group mean shows, principals in the control group report having worked about 46-47 hours in the week prior to survey implementation. They report spending on average about five hours on the administration of the meal program. Once again, the first column for each outcome (columns 1, 3, and 5) report results for the pooled sample, while columns 2, 4, and 6 include an indicator variable for high density treatment areas as well as an interaction term between treatment school and high density treatment area. Here, in contrast to our results for motivation, we find no statistically significant effect of treatment on hours worked in either high or low density areas, or in the pooled sample. When we look specifically at hours worked in the meal program, column 4 shows that the direction of results are consistent with those of Table 3. School principals of Treatment schools in low density areas are expected to report working about 1.5 hours more on the meal program as compared to principals of Control schools in low density areas. Within high density areas, in contrast, assignment to Treatment is associated with a lower expected number of hours worked in the meal program. Nonetheless, these coefficients are estimated very imprecisely and none approach traditional levels of statistical significance. Given the long time lag between the intervention and the endline survey, we unfortunately cannot evaluate if the treatment had short term effects on hours worked.

*School-level outcomes*

Finally, we examine whether the treatment had any downstream effect on school-level outcomes. Here, we focus on unanticipated school closings—that is, days a school is expected to be open but nonetheless reports zero attendance. Data from qualitative work suggests a variety of reasons schools have unanticipated closures, including teacher strikes, heavy rain, or a death in the school community. In 2018, there were numerous teacher strikes in the province, many with uneven participation (there are a variety of competing active teachers unions in the province). According to qualitative data, most teacher strikes in 2018 were carried out with teacher presence in the schools—that is, teachers came to the school but did not lead instruction. In those cases, school principals largely used their discretion and could choose to continue to offer meal service or to cancel meal service for the day. In addition, qualitative interviews suggested that principals can exercise their discretion to decide whether or not to record attendance during these days. We collect data on "zero attendance" days during our endline survey using contemporaneous record books that school principals maintain to track attendance (see Appendix C for a photo).[39] During the endline survey, we asked school principals to report attendance for 16 dates—1 randomly selected day for each of 16 weeks, beginning with the 4 weeks prior to the date when the voluntary audit invitations were mailed, and continuing with 12 subsequent weeks.[40]

[39] As noted in a footnote above, we also expected to find an effect of Treatment on the quality of food served and hence student attendance. However, a change in program rules in June 2018 centralized procurement and took the power to design meals away from school principals. These results are reported in Table A9. As a result, we focus here on school closings (which we define as days of zero attendance), because of the belief that these are more likely to reflect principal effort, rather than student responses to food quality.

[40] We collected attendance for one day per week because looking through old attendance records was time consuming, and we were concerned principals would not have patience to look through multiple days for week over the entire period of interest. When selecting dates, we excluded any dates of known closures (provincial or

*Insert Table 5 about here*

Table 5 shows that unanticipated school closings are relatively infrequent. For all columns, the outcome takes on the value of the proportion of dates for which we have data for which a school reported zero attendance. In Columns 1 and 2, we examine closures for all weeks after the invitations for voluntary audits were sent out. As indicated at the bottom of Column 1, schools in the control group report unanticipated closings around 1.5% of the days for which we collect data in this period. This rate, averaged over an 180-day school year, would mean schools experience unanticipated for 2-3 days. Such closures are likely to have a detrimental effect on students' learning and welfare, especially in a province like Chaco, where student test scores lag behind the rest of the country (Fernandez 2013) and child poverty rates are high (Direc Nacional de Maternidad e Infancia 2007). Column 1 suggests no effect of treatment on school closures in the pooled sample. In Columns 3-6, we examine the possibility (outlined in the PAP), that the intensity of treatment might fade with time. Column 3 focuses only on school closings in the 2 weeks after an audit, and Column 5 focuses on school closings in the 2 weeks after the invitation.[41] Here, again, in the pooled sample, we do not see any evidence of a treatment effect. However, columns 2, 4, and 6, which include the indicator variable for high density localities and the interaction term, tell a different story. Starting in column 4, we see the anticipated effect of Treatment—that is lowers the likelihood of irregular school closings—holds in low-density areas only. In the low-density arm, schools assigned to treatment have about 4 percentage point fewer closures. In contrast, among schools assigned to the high-density arm, those assigned to Treatment actually report about a 4 percentage point increase in school closures immediately after the audit. Both of these results are statistically significant at conventional level. Results reported in columns 2 and 6 are consistent with this pattern, though weaker. We interpret the stronger results for the 2 weeks after the audit to suggest that the audits themselves, rather than the invitation, were the stronger component of treatment.

**VI. Discussion**

Of the results reported in the section above, perhaps most striking is the finding that treatment has divergent results across high and low density treatment areas. With respect to motivation to work in the meal program and unanticipated school closings, the treatment had significant effects in the expected directed in the low-density treatment arm only. In areas of low-density treatment, principals of schools assigned to Treatment report higher levels of intrinsic motivation to work in the meal

national holidays). In a few cases, we later learned about municipality-specific holidays. School reports of zero attendance during these days were recoded to missing. The 12 subsequent weeks were not immediately after mailing—letters of invitation went out in early July, right before the two-weeks winter holiday. Our "baseline" closing data thus comes from the 4 immediately prior weeks in June. Our "post-intervention" closing data begins in the second week of August. We did not collect attendance data for any weeks in July or early August because school reopening dates after the holidays are often changed at the last minute, and school attendance is often more variable immediately after holidays.

[41] The precise 2 weeks vary by school. For schools in T who accept the audit, we examine closings in the two weeks after their audit. For schools in C and those in T who do not accept the audit, we examine closings in the two weeks after the latest audit in their region. We follow a similar logic for columns 5 and 6, using invitation dates rather than audit dates.

program—a statistically and substantively significant effect that we detect in a survey conducted fully eight months after the intervention. Contemporary records also show that these schools report fewer unanticipated closings in the weeks immediately after the audit. Furthermore, although not statistically significant, principals in treated schools in high-density areas also report working more hours on the meal program. Thus, within the low-density region, the effects of the voluntary audit encouragement are consistent with our theoretical expectations.

In contrast, the effect of treatment in high-density regions appears to be the opposite of the anticipated. In high-density regions, principals of Treatment schools report lower motivation to work in the meal program eight months later. Contemporaneous record keeping shows that these schools experienced more unanticipated closings immediately after the intervention. Finally, although not statistically significant, principals in treated schools in high density report working fewer hours on the meal program than principals in control schools in areas of high density of treatment.

Our theory was built on the assumption that the opportunity to volunteer for oversight would increase bureaucrats' sense of autonomy because it offered them a real choice in a context where bureaucrats rarely enjoy such opportunities. What might explain why Treatment would have the anticipated effect in areas where relatively few school were assigned to treatment, but the opposite effect in areas where many schools were assigned to treatment?

We speculate on two possible reasons that we might see a negative effect of Treatment in high density areas. First, the very fact that many schools were assigned to treatment might create pressure for school principals to accept the invitation and therefore undermine the sense of autonomy our intervention was designed to foster. Unfortunately, we do not have direct evidence that speaks to this possibility. However, we do note that schools assigned to the treatment group in high density areas accepted the encouragement at a higher rate than those assigned to Treatment in low density areas. In high density areas, 45 percent of school principals assigned to treatment accepted the voluntary audit, whereas in low density areas 25 percent of school principals assigned to treatment accepted the voluntary audit. We also know from our survey that school principals are often in WhatsApp groups with other principals of nearby schools. This suggests that school principals in areas with a high density of treatment may have felt less autonomous, rather than more autonomous, as a result of the invitation.[42]

Relatedly, it is possible that the anticipation and actual experience of the voluntary audit was disappointing for some school principals. Qualitative interviews with school principals (from both Control and Treatment groups) conducted after the endline survey suggest that responses to the invitation and voluntary audit were mixed.[43] Some school principals said they welcomed the opportunity to share their experiences with oversight officials, anticipated that these voluntary audits would be different from regular forms of oversight, and expressed satisfaction with the audit. Others expressed the view that the audits might take time away from other tasks and ultimately make little difference on levels of program support received from higher levels of government. If school principals who accepted

[42] A higher acceptance rate might mean that different types of schools accept the invitation in high density versus low density areas. However, we focus our analysis on the ITT only, so even if a larger number of "less motivated" principals accept the invitation in high density areas, that should not affect our estimates of the causal effect of the intervention.

[43] We asked principals in Treatment about their experiences with the invitation and (if applicable) audit and all principals about how they and their peers might view such an invitation.

the invitation due to peer pressure (due to the higher density of invitations) were more likely to experience disappointment and annoyance with the audit, this might explain the negative effect of Treatment in high density areas.

**VII. Conclusions**

Street-level, front-line service providers exercise enormous influence on how public policies are implemented on the ground and thus on the quality of public services citizens enjoy and citizen perceptions of government performance. Traditional models of oversight, both those proposed by scholars and those implemented by governments, assume a punitive model and seek to control possible bureaucratic malfeasance. In this project, we suggest that an alternative model of oversight that provides bureaucrats with autonomy, choice, and the possibility of positive recognition might improve their performance. We examine this through an intervention carried out in conjunction with the provincial auditing office (PAO) in the province of Chaco, Argentina. Our intervention randomly assigned some school principals to be invited to accept a voluntary audit of an important school meal program.

To our knowledge, this is the first experimental intervention that seeks to explore the effects of voluntary oversight on street-level bureaucrats' motivation and effort, as well as on downstream measures of the quality of services provided to citizens. It is noteworthy that, contrary to many principal-agent models, a large number of bureaucrats do accept the invitation. Our qualitative interviews with school principals suggests that part of the appeal of the invitation was the opportunity it presented to offer feedback to policy-makers in a setting in which such opportunities are extremely rare. For example, one principal described Ministry officials as distant from their day to day work, saying "I think they are distant from reality . . . there are unsatisfied needs that they are unfamiliar with."[44] Similarly, with respect to the limited resources they receive for the meal program, another expressed her desire to "let those in charge (*los gobernantes*) know how one manages with what they give us."[45] While some scholars include the opportunity for bureaucratic input into policy formulation and implementation into indices of autonomy (Rasul and Rogger 2018), the consequences of offering such opportunities has rarely been examined. Indeed, in their summary of the literature, Esteve and Schuster (2019) identify only one paper that examines how bureaucratic input into policy affects individual performance. Our qualitative work and the relatively high rate of acceptance of the invitation to an audit among school principals suggests this could be fruitful approach to motivating bureaucrats.

Our intervention finds the expected, positive effects of the voluntary audit for one group of school principals—those assigned to the low density treatment group. The fact that we are able to detect effects on self-reported motivation 8 months after the intervention suggest that effects on motivation and morale could be quite long-lasting. At the same time, the unexpected negative effects of Treatment on principals suggest that when many bureaucrats are invited, the sense of autonomy voluntary oversight is intended to create may be undermined.

This study also contributes to the ongoing discussion about the relative advantages and drawbacks of working directly with a government partner without significantly supporting their implementation capacity (e.g., Peters, Langbein, and Roberts 2018). Our implementation relied very

[44] Interview P2, June 2020.
[45] Interview P3, June 2020.

heavily on the existing capacity of our government partner, the PAO. Like many such oversight institutions, it is highly capacity constrained, in terms of personnel, budget, and resources, and our support was minimal. In particular, the PAO staffed the audits entirely with their existing staff levels, and staffing levels were the major constraint on the number of schools assigned to Treatment in the experiment. While these staffing restrictions reduced the experiment's sample size, they may also offer some reassurance that such an intervention could be offered autonomously by oversight bodies in contexts where oversight bodies are similarly constrained.

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Table 1: Balance in baseline characteristics by treatment group (pooled sample)

| | Control | | Treatment | |
|---|---|---|---|---|
| Variable | N | Mean/SD | N | Mean/SD |
| Number of students | 126 | 343.02<br>(232.08) | 62 | 364.27<br>(199.80) |
| Avg. attendance | 126 | 219.55<br>(152.34) | 62 | 251.48<br>(134.97) |
| School closings | 126 | 0.11<br>(0.14) | 62 | 0.08<br>(0.13) |
| School offers breakfast | 126 | 0.19<br>(0.39) | 62 | 0.13<br>(0.34) |
| School offers lunch | 126 | 0.25<br>(0.44) | 62 | 0.23<br>(0.42) |
| School offers snack | 126 | 0.82<br>(0.39) | 62 | 0.89<br>(0.32) |
| School offers dinner | 126 | 0.17<br>(0.37) | 62 | 0.10<br>(0.30) |
| Neighborhood income group | 126 | 2.94<br>(1.25) | 62 | 2.98<br>(1.27) |
| Pre-school | 126 | 0.48<br>(0.50) | 62 | 0.42<br>(0.50) |
| Radius 100 km | 126 | 0.62<br>(0.49) | 62 | 0.71<br>(0.46) |
| Radius 200 km | 126 | 0.85<br>(0.36) | 62 | 0.87<br>(0.34) |
| F-test, p-value | 0.8855 | | | |

Table 2: Balance in baseline characteristics by treatment group

| | (1) | | (2) | | (3) | | (4) | |
|---|---|---|---|---|---|---|---|---|
| | Low density | | | | High density | | | |
| | Control | | Treatment | | Control | | Treatment | |
| Variable | N | Mean/SD | N | Mean/SD | N | Mean/SD | N | Mean/SD |
| Number of students | 82 | 337.44 (237.69) | 16 | 341.88 (174.73) | 44 | 353.43 (223.56) | 46 | 372.07 (209.03) |
| Avg. attendance | 82 | 204.40 (141.40) | 16 | 221.72 (113.24) | 44 | 247.78 (168.97) | 46 | 261.84 (141.41) |
| School closings | 82 | 0.12 (0.16) | 16 | 0.08 (0.12) | 44 | 0.09 (0.11) | 46 | 0.08 (0.14) |
| School offers breakfast | 82 | 0.22 (0.42) | 16 | 0.19 (0.40) | 44 | 0.14 (0.35) | 46 | 0.11 (0.31) |
| School offers lunch | 82 | 0.18 (0.39) | 16 | 0.25 (0.45) | 44 | 0.39 (0.49) | 46 | 0.22 (0.42) |
| School offers snack | 82 | 0.83 (0.38) | 16 | 0.81 (0.40) | 44 | 0.80 (0.41) | 46 | 0.91 (0.28) |
| School offers dinner | 82 | 0.20 (0.40) | 16 | 0.19 (0.40) | 44 | 0.11 (0.32) | 46 | 0.07 (0.25) |
| Neighborhood income group | 82 | 2.96 (1.15) | 16 | 2.93 (1.29) | 44 | 2.91 (1.44) | 46 | 3.00 (1.28) |
| Pre-school | 82 | 0.50 (0.50) | 16 | 0.31 (0.48) | 44 | 0.43 (0.50) | 46 | 0.46 (0.50) |
| Radius 100 km | 82 | 0.55 (0.50) | 16 | 0.62 (0.50) | 44 | 0.75 (0.44) | 46 | 0.74 (0.44) |
| Radius 200 km | 82 | 0.84 (0.37) | 16 | 0.88 (0.34) | 44 | 0.86 (0.35) | 46 | 0.87 (0.34) |
| Chi-squared test, p-value | 0.1003 | | | | | | | |

Table 3: Effect of voluntary audits on external and internal motivation

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | external lunch program | | internal lunch program | | external supervise teachers (placebo) | | internal supervise teachers (placebo) | |
| Treatment school | -0.04 | 0.16 | 0.07 | 0.35*** | -0.02 | 0.12 | -0.09 | -0.06 |
| | (0.09) | (0.14) | (0.08) | (0.11) | (0.09) | (0.13) | (0.09) | (0.18) |
| Treatment locality | | 0.08 | | 0.05 | | 0.09 | | 0.00 |
| | | (0.11) | | (0.09) | | (0.11) | | (0.08) |
| Interaction | | -0.40** | | -0.55*** | | -0.29 | | -0.07 |
| | | (0.18) | | (0.16) | | (0.18) | | (0.20) |
| Constant | 2.45*** | 2.41*** | 3.16*** | 3.13*** | 2.53*** | 2.48*** | 3.53*** | 3.53*** |
| | (0.06) | (0.07) | (0.05) | (0.06) | (0.06) | (0.06) | (0.04) | (0.04) |
| Observations | 182 | 182 | 182 | 182 | 183 | 183 | 183 | 183 |
| R-squared | 0.07 | 0.04 | 0.12 | 0.11 | 0.04 | 0.02 | 0.09 | 0.01 |
| Comparison Group Mean | 2.444 | 2.415 | 3.152 | 3.135 | 2.518 | 2.485 | 3.529 | 3.529 |
| Treat_school+Interaction (p-value) | | 0.0462 | | 0.101 | | 0.175 | | 0.156 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Effect of voluntary audits on hours worked

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Hours worked | | Hours worked in lunch program | | Hours worked in other activities (placebo) | |
| Treatment school | -1.73 | -1.09 | 0.11 | 1.45 | 1.13 | 2.26 |
| | (1.51) | (2.97) | (0.86) | (1.36) | (2.58) | (4.54) |
| Treatment locality | | 0.55 | | 0.95 | | 2.43 |
| | | (1.75) | | (1.10) | | (2.82) |
| Interaction | | -1.38 | | -2.69 | | -3.48 |
| | | (3.60) | | (1.80) | | (5.46) |
| Constant | 46.61*** | 46.37*** | 5.53*** | 5.09*** | 28.97*** | 28.10*** |
| | (0.86) | (0.99) | (0.55) | (0.40) | (1.31) | (1.72) |
| Observations | 175 | 175 | 171 | 171 | 176 | 176 |
| R-squared | 0.19 | 0.01 | 0.13 | 0.02 | 0.15 | 0.00 |
| Comparison Group Mean | 46.56 | 46.37 | 5.414 | 5.088 | 28.93 | 28.10 |
| Treat_school+Interaction (p-value) | | 0.227 | | 0.295 | | 0.687 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect of voluntary audits on school closings

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | School closings | | School closings 2 weeks after audit | | School closings 2 weeks after invite | |
| Treatment school | 0.00 | -0.01 | 0.00 | -0.04** | 0.03 | 0.02 |
| | (0.01) | (0.01) | (0.01) | (0.02) | (0.03) | (0.05) |
| Treatment locality | | -0.02** | | -0.04** | | -0.04* |
| | | (0.01) | | (0.02) | | (0.02) |
| Interaction | | 0.04** | | 0.08*** | | -0.02 |
| | | (0.01) | | (0.03) | | (0.06) |
| Constant | 0.01*** | 0.02*** | 0.02** | 0.04** | 0.03** | 0.05*** |
| | (0.00) | (0.01) | (0.01) | (0.02) | (0.01) | (0.02) |
| Observations | 177 | 177 | 177 | 177 | 177 | 177 |
| R-squared | 0.09 | 0.04 | 0.06 | 0.04 | 0.16 | 0.03 |
| Comparison Group Mean | 0.0151 | 0.0219 | 0.0254 | 0.0390 | 0.0381 | 0.0519 |
| Treat_school+Interaction (p-value) | | 0.0120 | | 0.0400 | | 0.625 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

# Appendix

- Table A1: Summary statistics
- Table A2: Effect of voluntary audits on external and internal motivation (with lasso-selected controls
- Table A3: Effect of voluntary audits on hours worked (with lasso-selected controls)
- Table A4: Effect of voluntary audits on school closings (with lasso-selected controls)
- Table A5: Effect of voluntary audits on potential audiences
- Table A6: Baseline characteristics of schools that accepted and did not accept the voluntary audit (pooled sample)
- Table A7: Baseline characteristics of schools that accepted and did not accept the voluntary audit in low density regions
- Table A8: Baseline characteristics of schools that accepted and did not accept the voluntary audit in high density regions
- Table A9: Effect of voluntary audits on outcomes farther away from school principals' control

Table A1: Summary statistics

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| **Outcomes** | | | | | |
| PAO top 3 audience | 0.04 | 0.19 | 0 | 1 | 180 |
| External motivation (lunch program) | 2.41 | 0.59 | 1 | 3.7 | 182 |
| Internal motivation (lunch program) | 3.14 | 0.55 | 1.4 | 4 | 182 |
| Hours worked | 45.93 | 9.13 | 6 | 70 | 175 |
| Hours worked in lunch program | 5.35 | 4.52 | 0 | 40 | 171 |
| School closings | 0.02 | 0.06 | 0 | 0.38 | 177 |
| School closings 2 weeks after audit | 0.03 | 0.12 | 0 | 0.5 | 177 |
| School closings 2 weeks after invite | 0.04 | 0.14 | 0 | 1 | 177 |
| **Placebo outcomes** | | | | | |
| Supervisors top 3 audience | 0.52 | 0.5 | 0 | 1 | 180 |
| Min. Education top 3 audience | 0.18 | 0.39 | 0 | 1 | 180 |
| External motivation (supervise teachers) | 2.5 | 0.55 | 1.1 | 3.9 | 183 |
| Internal motivation (supervise teachers) | 3.49 | 0.43 | 1.6 | 4 | 183 |
| Hours worked in other activities | 29.14 | 14.5 | 6 | 75 | 176 |
| **Baseline variables** | | | | | |
| Num. students | 350.03 | 221.64 | 44 | 1134 | 188 |
| Avg. attendance | 230.08 | 147.25 | 40.75 | 751 | 188 |
| School closings | 0.1 | 0.14 | 0 | 0.5 | 188 |
| Breakfast | 0.17 | 0.38 | 0 | 1 | 188 |
| Lunch | 0.24 | 0.43 | 0 | 1 | 188 |
| Snack | 0.84 | 0.37 | 0 | 1 | 188 |
| Dinner | 0.14 | 0.35 | 0 | 1 | 188 |
| NH income group | 2.96 | 1.26 | 1 | 5 | 188 |
| Pre-school | 0.46 | 0.5 | 0 | 1 | 188 |
| Radius 100 km | 0.65 | 0.48 | 0 | 1 | 188 |
| Radius 200 km | 0.86 | 0.35 | 0 | 1 | 188 |

Table A2: Effect of voluntary audits on external and internal motivation (with lasso-selected controls)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | external lunch program | | internal lunch program | | external supervise teachers (placebo) | | internal supervise teachers (placebo) | |
| Treatment school | -0.05 | 0.16 | 0.07 | 0.35*** | -0.02 | 0.12 | -0.10 | -0.06 |
| | (0.09) | (0.14) | (0.08) | (0.11) | (0.09) | (0.13) | (0.09) | (0.18) |
| Treatment locality | | 0.08 | | 0.05 | | 0.09 | | 0.00 |
| | | (0.11) | | (0.09) | | (0.11) | | (0.08) |
| Interaction | | -0.40** | | -0.55*** | | -0.29 | | -0.07 |
| | | (0.18) | | (0.16) | | (0.18) | | (0.20) |
| Constant | 2.55*** | 2.41*** | 3.09*** | 3.13*** | 2.62*** | 2.48*** | 3.61*** | 3.53*** |
| | (0.18) | (0.07) | (0.17) | (0.06) | (0.20) | (0.06) | (0.16) | (0.04) |
| Observations | 182 | 182 | 182 | 182 | 183 | 183 | 183 | 183 |
| Comparison Group Mean | 2.444 | 2.415 | 3.152 | 3.135 | 2.518 | 2.485 | 3.529 | 3.529 |
| Treat_school+Interaction (p-value) | | 0.0424 | | 0.0953 | | 0.168 | | 0.150 |

Note: Lasso-selected controls include whether the school is within 100 km from the center of Resistencia, within 200 km from the center of Resistencia, and if the school offers lunch in columns 1, 3, 5, and 7. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Effect of voluntary audits on hours worked (with lasso-selected controls)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Hours worked | | Hours worked in lunch program | | Hours worked in other activities (placebo) | |
| Treatment school | -1.52 | -1.09 | 0.07 | 1.45 | 1.06 | 2.26 |
| | (1.46) | (2.93) | (0.83) | (1.35) | (2.52) | (4.49) |
| Treatment locality | | 0.55 | | 0.95 | | 2.43 |
| | | (1.73) | | (1.09) | | (2.79) |
| Interaction | | -1.38 | | -2.69 | | -3.48 |
| | | (3.56) | | (1.78) | | (5.40) |
| Constant | 47.80*** | 46.37*** | 8.06*** | 5.09*** | 34.47*** | 28.10*** |
| | (3.95) | (0.98) | (1.52) | (0.40) | (5.26) | (1.70) |
| Observations | 175 | 175 | 171 | 171 | 176 | 176 |
| Comparison Group Mean | 46.56 | 46.37 | 5.414 | 5.088 | 28.93 | 28.10 |
| Treat_school+Interaction (p-value) | | 0.220 | | 0.287 | | 0.683 |

Note: Lasso-selected controls include whether the school is within 100 km from the center of Resistencia, within 200 km from the center of Resistencia, and if the school offers lunch in columns 1, 3, 5, and 7. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Effect of voluntary audits on school closings (with lasso-selected controls)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | School closings | | School closings 2 weeks after audit | | School closings 2 weeks after invite | |
| Treatment school | 0.01 | -0.01 | 0.00 | -0.04** | 0.03 | 0.02 |
| | (0.01) | (0.01) | (0.01) | (0.02) | (0.03) | (0.05) |
| Treatment locality | | -0.02** | | -0.04** | | -0.04* |
| | | (0.01) | | (0.02) | | (0.02) |
| Interaction | | 0.04*** | | 0.08*** | | -0.02 |
| | | (0.01) | | (0.03) | | (0.06) |
| Constant | 0.00 | 0.02*** | 0.03 | 0.04** | 0.18** | 0.05*** |
| | (0.01) | (0.01) | (0.03) | (0.02) | (0.09) | (0.02) |
| Observations | 177 | 177 | 177 | 177 | 177 | 177 |
| Comparison Group Mean | 0.0151 | 0.0219 | 0.0254 | 0.0390 | 0.0381 | 0.0519 |
| Treat_school+Interaction (p-value) | | 0.0102 | | 0.0374 | | 0.620 |

Note: Lasso-selected controls include whether the school is within 100 km from the center of Resistencia, within 200 km from the center of Resistencia, and if the school offers lunch in columns 1, 3, 5, and 7. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5: Effect of voluntary audits on potential audiences

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | PAO | | Supervisors | | Ministry Education | |
| Panel A: without controls | | | | | | |
| Treatment school | 0.07 | 0.10 | -0.07 | 0.03 | 0.02 | -0.02 |
| | (0.04) | (0.10) | (0.08) | (0.15) | (0.06) | (0.10) |
| Treatment locality | | -0.04* | | 0.02 | | 0.01 |
| | | (0.02) | | (0.10) | | (0.07) |
| Interaction | | -0.06 | | -0.18 | | 0.09 |
| | | (0.10) | | (0.18) | | (0.14) |
| Constant | 0.02 | 0.04* | 0.56*** | 0.54*** | 0.17*** | 0.16*** |
| | (0.01) | (0.02) | (0.05) | (0.06) | (0.04) | (0.04) |
| Observations | 180 | 180 | 180 | 180 | 180 | 180 |
| R-squared | 0.18 | 0.05 | 0.11 | 0.02 | 0.09 | 0.01 |
| Comparison Group Mean | 0.0250 | 0.0380 | 0.550 | 0.544 | 0.167 | 0.165 |
| Treat_school+Interaction (p-value) | | 0.155 | | 0.170 | | 0.433 |
| Panel B: With lasso-selected controls | | | | | | |
| Treatment school | 0.08* | 0.10 | -0.07 | 0.03 | 0.03 | -0.02 |
| | (0.04) | (0.10) | (0.08) | (0.14) | (0.06) | (0.10) |
| Treatment locality | | -0.04* | | 0.02 | | 0.01 |
| | | (0.02) | | (0.10) | | (0.07) |
| Interaction | | -0.06 | | -0.18 | | 0.09 |
| | | (0.10) | | (0.18) | | (0.13) |
| Constant | -0.10* | 0.04* | 0.26 | 0.54*** | 0.15 | 0.16*** |
| | (0.05) | (0.02) | (0.16) | (0.06) | (0.12) | (0.04) |
| Observations | 180 | 180 | 180 | 180 | 180 | 180 |
| Comparison Group Mean | 0.0250 | 0.0380 | 0.550 | 0.544 | 0.167 | 0.165 |
| Treat_school+Interaction (p-value) | | 0.149 | | 0.164 | | 0.427 |

Note: Lasso-selected controls in Panel B include whether the school is within 100 km from the center of Resistencia, within 200 km from the center of Resistencia, and if the school offers lunch in columns 1, 3, 5, and 7. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A6: Baseline characteristics of schools that accepted and did not accept the voluntary audit

| Variable | (1) Did not accept voluntary audit N | Mean/SD | (2) Accepted voluntary audit N | Mean/SD | T-test P-value (1)-(2) |
|---|---|---|---|---|---|
| Num. students | 36 | 405.36 (199.97) | 24 | 317.42 (193.18) | 0.10* |
| Avg. attendance | 36 | 281.00 (136.34) | 24 | 217.42 (126.76) | 0.06* |
| School closings | 36 | 0.05 (0.10) | 24 | 0.12 (0.15) | 0.11 |
| Breakfast | 36 | 0.11 (0.32) | 24 | 0.17 (0.38) | 0.70 |
| Lunch | 36 | 0.17 (0.38) | 24 | 0.29 (0.46) | 0.39 |
| Snack | 36 | 0.94 (0.23) | 24 | 0.79 (0.41) | 0.10 |
| Dinner | 36 | 0.08 (0.28) | 24 | 0.12 (0.34) | 0.79 |
| NH income group | 36 | 3.14 (1.29) | 24 | 2.83 (1.24) | 0.64 |
| Pre-school | 36 | 0.33 (0.48) | 24 | 0.50 (0.51) | 0.31 |
| Radius 100 km | 36 | 0.75 (0.44) | 24 | 0.71 (0.46) | . |
| Radius 200 km | 36 | 0.92 (0.28) | 24 | 0.88 (0.34) | . |

Note: T-test are computed from a regression where the dependent variable is the baseline characteristic, and the independent variable is acceptance of the audit. Region fixed effects are included.
*** p<0.01, ** p<0.05, * p<0.1

Table A7: Baseline characteristics of schools that accepted and did not accept the voluntary audit in low density regions

| | (1) Did not accept voluntary audit | | (2) Accepted voluntary audit | | T-test P-value |
|---|---|---|---|---|---|
| Variable | N | Mean/SD | N | Mean/SD | (1)-(2) |
| Num. students | 12 | 352.08 (163.85) | 4 | 311.25 (229.23) | 0.52 |
| Avg. attendance | 12 | 238.67 (113.06) | 4 | 170.87 (112.51) | 0.51 |
| School closings | 12 | 0.04 (0.10) | 4 | 0.17 (0.15) | 0.39 |
| Breakfast | 12 | 0.08 (0.29) | 4 | 0.50 (0.58) | 0.44 |
| Lunch | 12 | 0.17 (0.39) | 4 | 0.50 (0.58) | 0.50 |
| Dinner | 12 | 0.92 (0.29) | 4 | 0.50 (0.58) | 0.44 |
| Snack | 12 | 0.08 (0.29) | 4 | 0.50 (0.58) | 0.44 |
| NH income group | 12 | 2.83 (1.40) | 4 | 3.25 (0.96) | 0.56 |
| Pre-school | 12 | 0.33 (0.49) | 4 | 0.25 (0.50) | 0.84 |
| Radius 100 km | 12 | 0.75 (0.45) | 4 | 0.25 (0.50) | . |
| Radius 200 km | 12 | 0.92 (0.29) | 4 | 0.75 (0.50) | . |

Note: T-test are computed from a regression where the dependent variable is the baseline characteristic, and the independent variable is acceptance of the audit. Region fixed effects are included. *** p<0.01, ** p<0.05, * p<0.1

Table A8: Baseline characteristics of schools that accepted and did not accept the voluntary audit in high density regions

| | (1) Did not accept voluntary audit | | (2) Accepted voluntary audit | | T-test P-value |
|---|---|---|---|---|---|
| Variable | N | Mean/SD | N | Mean/SD | (1)-(2) |
| Num. students | 24 | 432.00 (213.99) | 20 | 318.65 (192.02) | 0.13 |
| Avg. attendance | 24 | 302.16 (144.12) | 20 | 226.73 (130.01) | 0.09* |
| School closings | 24 | 0.05 (0.10) | 20 | 0.11 (0.15) | 0.18 |
| Breakfast | 24 | 0.12 (0.34) | 20 | 0.10 (0.31) | 0.98 |
| Lunch | 24 | 0.17 (0.38) | 20 | 0.25 (0.44) | 0.55 |
| Dinner | 24 | 0.96 (0.20) | 20 | 0.85 (0.37) | 0.16 |
| Snack | 24 | 0.08 (0.28) | 20 | 0.05 (0.22) | 0.79 |
| NH income group | 24 | 3.29 (1.23) | 20 | 2.75 (1.29) | 0.45 |
| Pre-school | 24 | 0.33 (0.48) | 20 | 0.55 (0.51) | 0.22 |
| Radius 100 km | 24 | 0.75 (0.44) | 20 | 0.80 (0.41) | . |
| Radius 200 km | 24 | 0.92 (0.28) | 20 | 0.90 (0.31) | . |

Note: T-test are computed from a regression where the dependent variable is the baseline characteristic, and the independent variable is acceptance of the audit. Region fixed effects are included.
*** p<0.01, ** p<0.05, * p<0.1

Table A9: Effect of voluntary audits on outcomes farther away from school principals' control

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Proportion of meals | | Proportion of meals with fruits or veg. | | Students attendance | |
| Panel A: without controls | | | | | | |
| Treatment school | 0.02 | 0.05 | -0.03 | -0.02 | 8.54 | -6.51 |
| | (0.02) | (0.03) | (0.03) | (0.05) | (24.22) | (40.30) |
| Treatment locality | | 0.01 | | 0.02 | | 25.89 |
| | | (0.04) | | (0.03) | | (30.47) |
| Interaction | | -0.05 | | -0.00 | | 27.11 |
| | | (0.05) | | (0.06) | | (51.72) |
| Constant | 0.93*** | 0.93*** | 0.13*** | 0.12*** | 248.62*** | 235.96*** |
| | (0.02) | (0.02) | (0.02) | (0.02) | (15.87) | (17.72) |
| Observations | 172 | 172 | 183 | 183 | 177 | 177 |
| R-squared | 0.12 | 0.01 | 0.11 | 0.00 | 0.07 | 0.02 |
| Comparison Group Mean | 0.928 | 0.926 | 0.124 | 0.117 | 245 | 236 |
| Treat school+Interaction (p-value) | | 0.997 | | 0.555 | | 0.526 |
| Panel B: With lasso-selected controls | | | | | | |
| Treatment school | 0.01 | 0.05 | -0.03 | -0.02 | -3.80 | -20.96 |
| | (0.02) | (0.03) | (0.03) | (0.05) | (7.28) | (15.00) |
| Treatment locality | | 0.01 | | 0.02 | | -8.57 |
| | | (0.03) | | (0.03) | | (8.65) |
| Interaction | | -0.05 | | -0.00 | | 26.19 |
| | | (0.04) | | (0.06) | | (17.78) |
| Constant | 0.93*** | 0.93*** | 0.05** | 0.12*** | 36.13*** | 30.76*** |
| | (0.05) | (0.02) | (0.02) | (0.02) | (10.18) | (7.51) |
| Observations | 172 | 172 | 183 | 183 | 177 | 177 |
| Comparison Group Mean | 0.928 | 0.926 | 0.124 | 0.117 | 245 | 236 |
| Treat school+Interaction (p-value) | | 0.997 | | 0.549 | | 0.576 |

Note: Lasso-selected controls in Panel B include whether the school is within 100 km and 200 km from the center of Resistencia, and if the school offers lunch in columns 1, 3, 5, and 7. Lasso also selected baseline attendance and number of registered students as controls in columns 5 and 6. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**ADDITIONAL APPENDICES**

**Appendix A: Voluntary Audit Invitation (Spanish)**

TRIBUNAL DE CUENTAS

PROVINCIA DEL CHACO – REPÚBLICA ARGENTINA

Estimado/a Director/a:

En mi carácter de Fiscal del Tribunal de Cuentas de la Provincia de Chaco me comunico con Ud. a fin de comunicarle que parte de nuestras funciones es auditar la administración del Plan Nacional de Seguridad Alimentaría (PNSA) en los comedores escolares de la Provincia del Chaco.

El programa de comedores escolares es extremadamente relevante al proporcionar alimentación gratuita y nutritiva a los niños de la provincia. En función de ello, teniendo presente el control de gestión que se realiza en virtud a lo que establece el art. 6º inc. 1 a) de la Ley Nº 831-A (Ley orgánica del Tribunal de Cuentas) y con el objetivo de mejorar la eficiencia en el uso de los recursos del Programa, algunas escuelas fueron seleccionadas para recibir voluntariamente a un equipo del Tribunal de Cuentas, que brindará conocimientos sobre organización contable, que ayudará a su institución a mejorar y optimizar su trabajo.

**Su escuela ha sido una de las seleccionadas al azar para participar del proceso en el año inaugural de esta iniciativa.**

Si usted acepta participar, el proceso consistirá en que su escuela recibirá una visita del equipo de auditores del Tribunal de Cuentas en el transcurso de los meses venideros, en fecha y horario previamente pautado con usted.

Durante esta visita, el equipo de auditores solicitará los comprobantes de compras de alimentos con los fondos recibidos y relevará el funcionamiento general del programa en la escuela a su cargo. Después de la visita, el Tribunal elaborará un acta de cierre de auditoria con la confirmación de participación de la escuela y colaborará con sugerencias y metodologías de trabajo adecuadas para llevar adelante la administración del comedor.

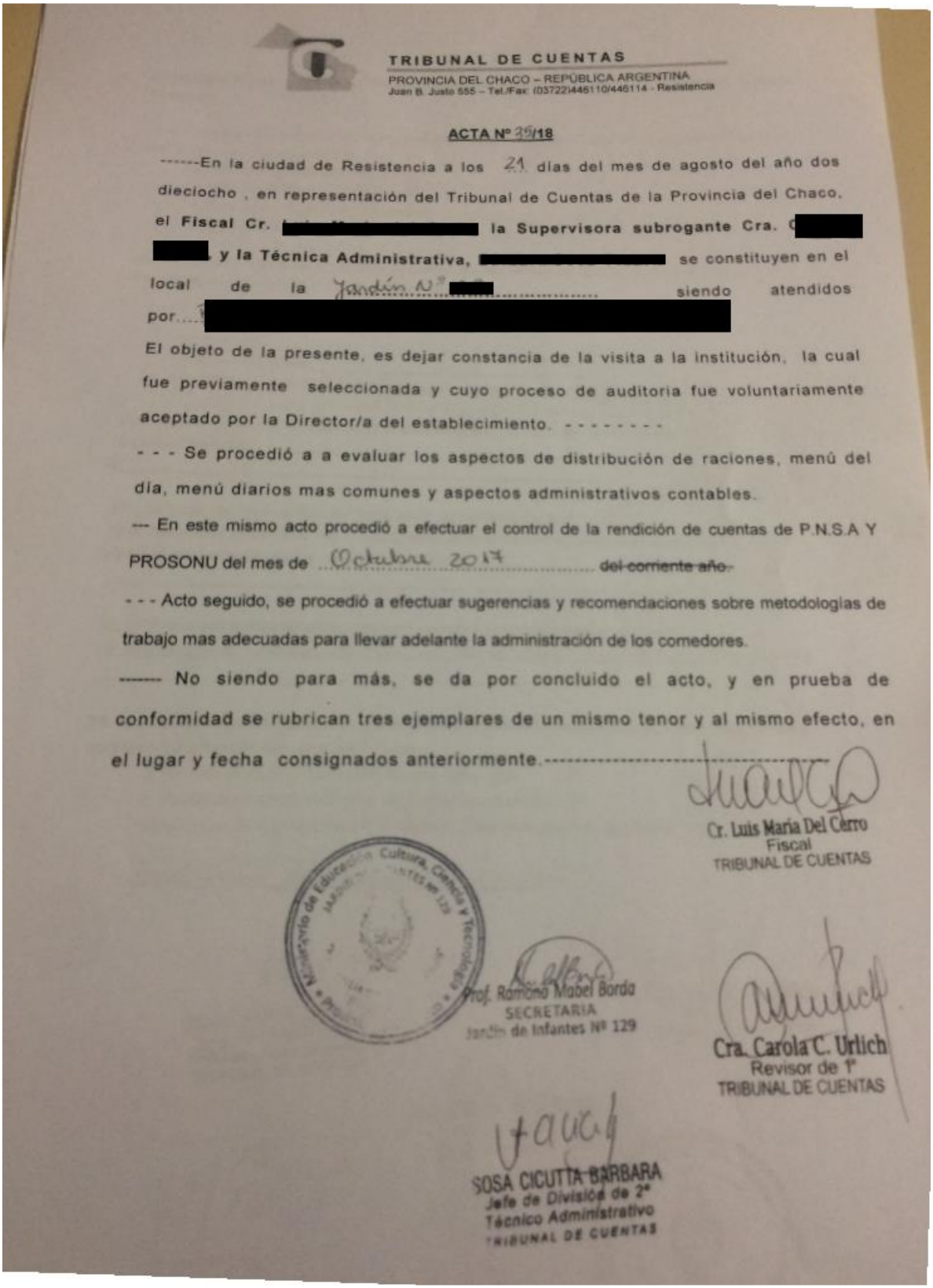
Tenga en cuenta que su participación en esta iniciativa es totalmente voluntaria. Por los beneficios que ofrece la colaboración, esperamos un alto nivel de participación.

Por favor, confirmar su decisión al mail: *email redacted*, mencionando nombre y apellido, cargo, nombre de la escuela; o al teléfono Nº *phone number redacted* dentro de los diez (10) días de recibido la presente. Si no se recepciona respuesta dentro del término previsto, nuestros asistentes del Tribunal de Cuentas lo contactarán para confirmar su decisión.

Atentamente.

*Signature redacted*

**Appendix B: Letter presented to schools at voluntary audits**



TRIBUNAL DE CUENTAS
PROVINCIA DEL CHACO – REPÚBLICA ARGENTINA
Juan B. Justo 555 – Tel./Fax: (03722)446110/446114 - Resistencia

**ACTA Nº 35/18**

------En la ciudad de Resistencia a los 21 días del mes de agosto del año dos dieciocho , en representación del Tribunal de Cuentas de la Provincia del Chaco, el **Fiscal Cr.** [redacted] **la Supervisora subrogante Cra.** [redacted] **y la Técnica Administrativa,** [redacted] se constituyen en el local de la Jardín Nº [redacted] siendo atendidos por [redacted]

El objeto de la presente, es dejar constancia de la visita a la institución, la cual fue previamente seleccionada y cuyo proceso de auditoria fue voluntariamente aceptado por la Director/a del establecimiento. - - - - - - - -

- - - Se procedió a a evaluar los aspectos de distribución de raciones, menú del día, menú diarios mas comunes y aspectos administrativos contables.

--- En este mismo acto procedió a efectuar el control de la rendición de cuentas de P.N.S.A Y PROSONU del mes de Octubre 2017 ~~del corriente año.~~

- - - Acto seguido, se procedió a efectuar sugerencias y recomendaciones sobre metodologías de trabajo mas adecuadas para llevar adelante la administración de los comedores.

------- No siendo para más, se da por concluido el acto, y en prueba de conformidad se rubrican tres ejemplares de un mismo tenor y al mismo efecto, en el lugar y fecha consignados anteriormente.----------

Cr. Luis María Del Cerro
Fiscal
TRIBUNAL DE CUENTAS

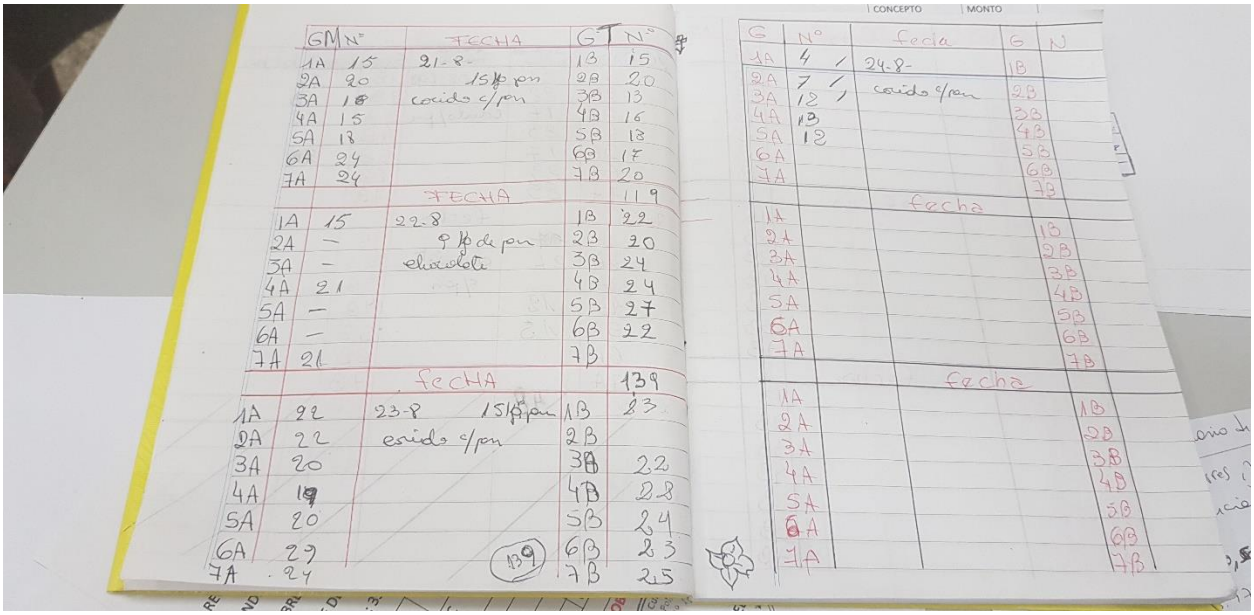
Prof. Ramona Mabel Borda
SECRETARIA
Jardín de Infantes Nº 129

Cra. Carola C. Urlich
Revisor de 1°
TRIBUNAL DE CUENTAS

SOSA CICUTTA BARBARA
Jefe de División de 2°
Técnico Administrativo
TRIBUNAL DE CUENTAS

**Appendix C: Attendance Record Keeping- Example**

*Note: This yellow notebook is the standard format schools in the province use to track attendance. Attendance for the morning session is noted on the left, while attendance in the afternoon session is noted on the right (the vast majority of elementary schools have two "shifts" and students attend only one). In this school, the principal has also noted the daily menu under the date in the center column.*



**Appendix D: Hypotheses as Detailed in the Pre-Analysis Plan**[1]

| Hypothesis | Outcome category | Measures |
|---|---|---|
| Increase in school principal effort | Number of hours worked | Number of hours worked total<br>Number of hours worked in the meal program<br>Number of extra (unpaid) hours worked |
| School principal effort in other school activities (exploratory) | Number of hours devoted to other school activities | Number of hours devoted to other school activities<br>[Sum total of hours spent supervising teachers, speaking to parents, and curricular tasks] |
| Increase in school principal motivation to work in the meal program[2] | Intrinsic motivation (meal program) | Index of intrinsic motivation |
| | Extrinsic motivation (meal program) | Index of extrinsic motivation |
| School principal motivation to work in other school activities (Placebo test; expect no effect) | Intrinsic motivation (monitor teachers) | Index of intrinsic motivation |
| | Extrinsic motivation (monitor teachers) | Index of extrinsic motivation |
| Increase in relevance of audit court as a "principal" | Ranking of potential audiences for school principal | *Tribunal de cuentas* is ranked as top 3 audience<br>*Tribunal de cuentas* is ranked as top audience |
| Increase in student attendance | Number of students attending school on selected days | Students attending school on selected days covering August 2018-Oct 2018 period[3] |
| Decrease in unanticipated closings | Number of days school closed for reasons other than official holidays or teacher training | Number of days school closed due to strikes, rain, or other school-level conditions |

[1] This table, including footnotes, appears on pages 5-6 of the pre-analysis plan.

[2] Although we expect an increase in motivation, we anticipate that the intervention could affect intrinsic and extrinsic motivation in different ways. Therefore, we think of these two types of motivations as different categories.

[3] We collect data on attendance in June as baseline data. We discuss our analysis strategy below. As a robustness check, we will also examine ratio of attendance to enrollment, but we expect higher measurement error in enrollment. If we find balance in enrollment numbers across treatment and control, we will rely on raw attendance data, but if we find imbalance in enrollment, we will rely on the ratio of enrollment/attendance as our main analysis.

| | | |
|---|---|---|
| Improvements in school meal program | Supply of snack/lunch out of 5-day reporting period | Proportion of meals school offers out of expected number of meals in 5-day reporting period[4] |
| | Composition of meals (snack/lunch) | Proportion of unique menus in 5-day reporting period |
| | | Proportion of meals offering at least 1 fresh fruit or 1 fresh vegetable[5] |

Note that we do not present results for either the nature of school meals or student attendance in the main text. As we discuss there, at the time of writing the PAP, we anticipated possible effects of Treatment on the nature and diversity of foods served in the schools, and, as a result, on student attendance. An administrative decision made immediately before our intervention (June 2018; our letters were sent to schools in July 2018) changed the system of food provision from a decentralized one, wherein principals were responsible for procurement, to a centralized one, where principals receive food purchased centrally. Although we were aware of that policy change at the time of writing our PAP, we were not aware of the extent to which this would diminish principal discretion over what foods were served in school canteens. The fact that the policy change effectively eliminated principal discretion over which foods were served became clear during informal conversations carried out during the endline survey and via qualitative interviews conducted after our endline survey.

[4] Number of expected meals in 5 day period depends on school meal program as determined by the province (does this school offer snack and/or lunch) and whether there were any official holidays during the previous 5 day reporting period.

[5] Following Britos (2015, 18), we will exclude 4 common starchy vegetables: yams, potatoes, corn, and cassava.