

OPTIMAL MONITORING AND BUREAUCRAT ADJUSTMENTS^{*}

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February 8, 2022

Abstract

This paper examines the behavior of bureaucrats, implementing India's public employment program, as their expectations of being audited change. Exploiting random assignment to audit timing, I find: the rate of deterrence (in financial misappropriation) is increasing in bureaucrats' expectations; bureaucrats evade detection by adjusting the timing and type of misappropriated expenditure. I interpret these findings with a model to analyze how information communicated about audit risk should be designed. I estimate a sufficient statistic from the model to solve for the optimal signal and analyze counterfactuals. Fully-informative signals would have persuaded bureaucrats to misappropriate USD35m less compared to uninformative signals.

^{*}I am grateful to my committee chairs, Canice Prendergast and Konstantin Sonin, and committee members, Chris Blattman and Luis Martinez, for continuous guidance. I am grateful to Ujjwal Pahurkar, Gurjeet Singh, Ganauri Vishwakarma, and the team at Jharkhand's Social Audit Unit for their invaluable support. I am grateful for feedback from Scott Ashworth, Dan Black, Oeindrila Dube, Steven Durlauf, Jeffrey Grogger, Damon Jones, Donald Moynihan, Yusuf Neggers, Guillaume Pouliot, James Robinson, Raul Sanchez de la Sierra, Shaoda Wang, Austin Wright, and participants at the Harris School's PhD Workshop and Student Working Group, and the University of Chicago's Development Economics Lunch. I am grateful to Eric Dodge, Rohini Pande, Charity Troyer Moore, and current and former colleagues at EPoD India for generously providing access and connecting me to resources in India. Any errors or expressed opinions are my own. Finally, I gratefully acknowledge financial support from the University of Chicago's Committee on Southern Asian Studies and the Becker Friedman Institute Development Economics Initiative.

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1 Introduction

Governments adopt monitoring to address failures in public compliance with the law and in the provision of public services (Wilson 1991; Rose-Ackerman and Palifka 2016; Finan et al. 2017). Examples range from monitoring of tax evasion and theft to financial misappropriation in public assistance programs. The existing empirical literature shows that more extensive monitoring, such as conducting more frequent or more credible audits, leads to more compliance.¹ This, however, offers limited guidance in evaluating the tradeoffs for designing the best monitoring policy, especially as commonplace constraints on government resources prohibit extensive monitoring. In addition, agents may adapt their behavior to evade detection when extensive monitoring is not possible, causing a displacement in misconduct.² Monitoring policies designed to maximize the deterrence of misconduct must account for such constraints and unintended consequences.

This paper determines the optimal monitoring policy in a setting where the monitor has a choice over the information communicated to agents about their audit risk and estimates the value of information design. Given that agents adjust in response to monitoring, should governments inform them when the auditor is coming or make it hard to predict? Audits and auditing guidelines are often implemented to maintain unpredictability among audit subjects.³ Yet, theory suggests that the answer depends on the relationship between deterrence and agents' expectations of being audited (Lazear 2006; Eeckhout et al. 2010). For example, at one extreme, if agents are only deterred when they are certain an auditor is coming, then it is best to provide this information in advance. In contrast, if they are deterred at the slightest probability of an audit, then withholding information may be better.

To advance our understanding on this question empirically, this paper examines strate-

1. See for e.g., Finan et al. (2017), for a review.

2. For e.g., Yang (2008), Carrillo et al. (2017), and Gonzalez-Lira and Mobarak (2021).

3. See as an example Section 5(c): U.S. auditing standards, adopted and approved by the Public Company Accounting Oversight Board and the U.S. Securities and Exchange Commission, advocate to withhold information from audit subjects to maintain unpredictability of when/how audits may occur.

gic responses of bureaucrats to changing expectations of being audited. I study the monitoring policy of India's Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS), the largest public employment program in the world (Sukhtankar 2017). First, I show that the deterrence of bureaucrats' financial misappropriation is more marginally responsive at higher than lower expectations of being audited.⁴ With these results, I apply a model of Bayesian persuasion (Kamenica and Gentzkow 2011). I then show that a simple change in the design of information, which allows bureaucrats to predict when they will be audited or not, persuades them to be less corrupt in aggregate than when leaving them to guess—while holding the budget and rules of the audit fixed. To my knowledge, this is the first empirical implementation of this theoretical construct.

Sixty-five percent of the population in India, or about 11.5% of the world population, is eligible for NREGS.⁵ The program insures against income shocks by guaranteeing employment to rural households to work on public projects. Evidence in data and anecdotes reveal troubling issues along various dimensions of NREGS program performance.⁶ Issues from audit reports range from participant payment delays to poor workplanning to fabricated employment and material procurement. Existing literature also documents financial misappropriation.⁷

This paper leverages a monitoring policy where audits of gram panchayats were staggered over years and randomly assigned *without* replacement. Gram panchayats (GPs) are the smallest implementing unit of NREGS.⁸ The policy was implemented in the state of Jharkhand beginning in 2016. This was the first time auditing of NREGS in Jharkhand was systematically conducted by the government and done so at scale. The round of first audits across all GPs completed within three years.

4. Throughout the paper, this notion of misappropriation intentionally works against government program goals and entails utility for bureaucrats.

5. NREGS expenditures make up about 0.4% of India's GDP.

6. See Sukhtankar (2017) for a synthesis of existing research on India's workfare program.

7. See for e.g., Khera (2011); Niehaus and Sukhtankar (2013); Muralidharan et al. (2016); and Banerjee et al. (2020).

8. The GP-level government sits under the block-, then district-, then state-level administrative units. The GP comprises wards. According to the 2011 Census of India, the median population of a GP in Jharkhand is about 6,100.

The staggered implementation of audits combined with the randomization design generated random variation in bureaucrats' expectations of being audited. Bureaucrats observe who has been and is waiting to be audited. For example, the longer bureaucrats wait without being audited, the better they are able to predict their audit in advance because monitoring is without replacement. In contrast, those who were audited first anticipate their second audit will not occur until everyone's first audits have been completed. Bureaucrats respond to these incentives because auditors verify the previous fiscal year's work and document issues with ongoing work. I estimate these anticipatory responses of bureaucrats by employing an event study specification around the timing of audit announcements. I do so with detailed administrative data at the GP-month level.

I use program expenditures as the main outcome to proxy for changes in misappropriated spending. Program expenditures are measured monthly, under the direct control of bureaucrats, and hard to manipulate ex-post. They focus the study of misconduct on financial misappropriation. However, they comprise both honest and misappropriated expenditures.⁹ I take two approaches to disentangle whether incentives from the monitoring policy are driving changes in honest or misappropriated expenditures. Using other administrative data, I test hypotheses to check whether the results are consistent with potential confounding mechanisms that could affect honest expenditures. Bandiera et al. (2009) and Londoño-Vélez and Ávila-Mahecha (2021) use a similar approach to study waste in government procurement and tax evasion. In addition, I use data from audit reports as a source of verification for whether the estimated changes in expenditures were misappropriated.

First, I find that the anticipatory effects of the audit on spending are substantial; total expenditures decline more on the margin at higher than lower expectations of being audited. When expectations of being audited are high to certain, there is a 15% decline in expenditures. This is driven by a significant decline in wage expenditures. When expectations of being audited are lower (zero to medium), expenditures are statistically

9. "Honest" program expenditures are those intended to contribute to program goals and do not entail personal financial gain to bureaucrats. They can include spending that is wasteful.

indistinguishable between these periods.

Second, I find that as expectations increase bureaucrats substitute misappropriation from wages to materials. When bureaucrats have medium expectations, material procurement significantly increases. This result is verified by audit reports: during this period, auditors assessed more fines related to material misappropriation. This is consistent with interpreting the estimated changes in expenditures as deterrence. Audit reports suggest that it is easier to detect wage over material misappropriation, which explains why bureaucrats adjust along this margin.

Third, intertemporal substitution diminishes the deterrence effect estimated during the audit. In particular, bureaucrats spend 11% less while auditors are present (driven by a decline in wages) and then spend 5% more in the following months once auditors leave (driven by an increase in materials). Results to disentangle mechanisms are consistent with changes being driven by misappropriated expenditures. For instance, alternative mechanisms such as multi-tasking issues while auditors are present do not explain the results. The findings are also consistent with real output remaining unchanged even though inputs change during this period.

If audits were instead randomized *with* replacement, would there have been greater deterrence? Under such a policy, bureaucrats would never know with certainty when they are up for an audit. When bureaucrats are deterred only under high expectations of being audited (as shown by the estimated anticipation effects), it is better to inform them about their audit in advance. I show this result is possible by modeling an information design problem and deriving conditions to determine the optimal signal. In this model, the principal is concerned with maximizing deterrence and has a choice over the information provided about the likelihood of being audited. The model shows that the relationship between bureaucrat deterrence and their expectation of being audited is a sufficient statistic for characterizing the principal's optimal signal and for analyzing welfare under counterfactual signals. I estimate the sufficient statistic for this empirical setting using causally-identified moments (the estimated anticipation effects).

Estimates of the sufficient statistic tell us that perfectly informing a random subset of bureaucrats when they will be audited in advance and others that they will not be audited is the optimal design of information. Signals which communicate more information yield more deterrence in aggregate than those that maintain unpredictability. This implies that assigning audits randomly without replacement is better than randomly with replacement. This conclusion is robust to a series of sensitivity analyses.

Welfare estimates show that a policy providing advanced warning would have persuaded bureaucrats to misappropriate USD 19.8 and USD 35 million *less* in expenditures (or 9% and 16% of average annual expenditures from 2016-19 in Jharkhand) compared to the actual policy of randomizing without replacement and to randomizing with replacement, respectively. These gains are substantial given wide-prevailing audit standards to withhold information from audit subjects to maintain unpredictability. This paper makes a strong case for evaluating the value of information design in other settings.

Contributions to the Literature

This paper (1) provides empirical evidence on strategic responses by bureaucrats to monitoring; (2) shows how information disseminated about a monitoring policy, which takes into account these strategic responses, can be optimally designed with a budget-constrained policymaker in mind; and (3) provides a novel empirical measure of the value of information.

Through (1), this paper complements a literature spanning across multiple fields studying the effectiveness of monitoring. Previous studies demonstrated the importance of: knowing with certainty you will be audited or policed (Di Tella and Schargrodsky 2004; Olken 2007); having a reliable monitor that can discover and accurately report findings (Banerjee et al. 2008; Duflo et al. 2012; Duflo et al. 2013); having a reliable system for imposing penalties when infractions are found, including an informed electorate (Ferraz and Finan 2008, 2011; Afridi and Iversen 2014; Bobonis et al. 2016); and having a persistent threat from monitoring over time (Avis et al. 2018).

Monitoring can be undermined when agents adapt their behavior to evade detection. For instance, targeted monitoring can lead to a displacement in crime (Yang 2008; Gonzalez-Lira and Mobarak 2021). Capturing displacement is challenging because measures of verified performance beyond audits are limited. The tax literature has made use of third-party data to examine how tax reports change with the threat of audit (Casaburi and Troiano 2016; Carrillo et al. 2017). Other studies have made use of administrative and audit data, and find audits lead to strategic adjustments in procurement by bureaucrats (Gerardino et al. 2017; Lichand and Fernandes 2019). This paper leverages administrative and audit data to measure strategic responses in combination with rich variation on the policy parameter of interest: bureaucrats' expectations of being audited. This combination is what allows us to determine the optimal design of information.

This paper interprets the empirical findings with a model of information design, complementing the theoretical literature on optimal information design (e.g. Kamenica and Gentzkow 2011; Bergemann and Morris 2019; Kamenica 2019). The intuitions from the model in this paper draw on prior theoretical work. Lazear (2006) models the trade off between informative versus uninformative signals in monitoring, and finds that the optimal signal depends on the shape of deterrence as a function of expectations of being audited. Other studies evaluate models with similar tradeoffs, but their empirical settings either require assuming an optimal policy is being implemented rather than solving for the optimum or are restricted to laboratory environments (Eeckhout et al. 2010; Chassang et al. 2020). Banerjee et al. (2019) study the optimal monitoring of drunk-driving, but their agent can choose to avoid being monitored altogether once they learn the monitor's strategy, while this is not possible for the bureaucrats in this setting. Relative to this empirical literature, this paper is novel because it studies agency issues in government and analyzes welfare under alternative communication policies.

The estimated relationship between deterrence and bureaucrats' expectations of being audited is a sufficient statistic from the theoretical model. Without requiring additional information, the sufficient statistic allows us to determine the optimal signal and analyze

welfare under counterfactual signals. The sufficient statistic is estimated using causally-identified parameters. This approach is related to a literature in public finance that develops sufficient statistics from theoretical models (Chetty 2009). These sufficient statistics evaluate welfare from changes in tax policies as functions of reduced-form elasticities and not structural primitives. To my knowledge, the sufficient statistics approach has not been applied to studies motivated by the optimal design of information. This approach can be applied to other settings where communication on monitoring can play a role to improve governance.

2 Background

2.1 The public employment program and the audit agency

Launched in 2006, the Mahatma Ghandi National Rural Employment Guarantee Scheme (NREGS) guarantees 100 days of work per year to rural households. Participants provide manual labor on projects commissioned by the local government.¹⁰ They construct or maintain assets that are intended to improve rural livelihood. Assets include structures for water conservation and harvesting, homes, latrines, and animal shelters.

Jharkhand is a state in the eastern part of India. The population is close to 33 million with 76% of people living in rural areas and eligible for NREGS. Sixty-one percent of the population relies on agricultural work, and their income is vulnerable to the volatility of seasonal agricultural output.¹¹ This makes NREGS an important source of reliable income. NREGS in Jharkhand has served around 7.7 million people and produced over 1 million projects.

In 2016, the government of Jharkhand began auditing GPs implementing the NREGS program. The Social Audit Unit (hereinafter referred to as the audit agency) is a sepa-

10. Wages set by the state government are generally below the minimum wage for manual work in agriculture and other industries.

11. Sources: Jharkhand Economic Survey 2017-18; Department of Agriculture of the Government of Jharkhand. Jharkhand has close to 40% of the mineral reserves found in India. The mining and manufacturing sectors contribute to over 33% of the state's economy.

rate government agency that conducts the audits.¹² The audit agency is funded independently of NREGS and managed by a steering committee of various stakeholders across the state government and civil society. Competitive compensation for auditors and quality assurance mechanisms suggest it is likely the audit agency conducted audits at-scale with credibility and integrity. More details on the audit agency and their processes are in Appendix A.2.

The goal of the audit agency was to audit all GPs for the first time before selecting GPs for audit for the second time. This was consistent with one of the goals of the NREGS national act to ensure regular auditing of all implementing bodies. To do so, the audit agency randomly selected GPs *without* replacement for audit from 2016-2019 until all GPs were audited. This paper focuses on the effect this monitoring policy had on bureaucrat behavior during these 3 fiscal years (FY) when GPs were receiving their first audit.¹³ During this period, 4,180 GPs were audited and informed that the previous FY's work would be a part of the audit.

2.2 The process when auditors arrive

Auditors spend a week at the GP to verify administrative reports on labor and material expenditures from the previous FY, document other observed issues with ongoing work, and conduct public hearings of their findings. The average number of auditors per audit is 2.58 and the distribution ranges from 2-9 auditors. Auditors gather information from GP office records; field observations; and household interviews. Their tasks include matching receipts with materials reported to be procured, verifying output at project sites (e.g. measuring dimensions of a dug pond), and interviewing households to verify past employment and document complaints.

The audit process is not designed to be disruptive of normal program operations. Ac-

12. "Social audits" incorporate community and participant feedback, hence the name. As noted in Section 2.2, the audit gathers information through household interviews. Public hearings are also held to announce audit findings and adjudicate issues found during the audit in a public forum.

13. Fiscal years in the Indian government go from the beginning of April to the end of March the following year.

cording to audit guidelines, providing paperwork for auditors in advance and participating in a couple days of public hearings are the only tasks required of GP bureaucrats during the audit process. About 64 percent of GPs fail to comply with parts of the audit process, but auditors can still proceed with verification of administrative reports using information gathered from field visits of project sites and household interviews.

Notably, information gathered during the audit process is also reflective of concurrent program performance. The auditors' scope of evaluation is not limited to performance from the previous FY. For example, auditors evaluate quality of record-keeping and proper advertising of available jobs, and they conduct household interviews. During these interviews, households raise ongoing issues even though auditors are investigating employment from the previous year. Problems of recall may make it hard for households to speak only to last year's work. Based on the audit reports, 50-60% of the identified issues reflect ongoing problems.

The threat of punishment to bureaucrats from the audit is credible. Issues identified from the audit are presented and adjudicated in a public hearing with auditors, bureaucrats, and participants in attendance.¹⁴ Issues are resolved when evidence is provided to show the issue is unfounded or when those culpable agree to take corrective action. Bureaucrats charged with financial misappropriation face paying a penalty commensurate with the amount misappropriated or risk losing their job.¹⁵ Unresolved issues are escalated to be adjudicated at a higher-level public hearing. The audit agency reports around \$1.5 million USD (11 crore INR) have been recovered through the audits, which is around 0.8% of Jharkhand's NREGS expenditures in FY2018-19.

14. Among all audit reports, there were 68,231 documented issues. The mean number of issues per audit is 21. Nineteen percent of issues are related to concerns about competency in implementation; 18% of issues are related to issues obtaining work and payments, not obviously related to misappropriation; 16% of issues are related to misappropriation of wages and allocated employment; 6% of issues are related to misappropriation in material procurement; 11% of issues are related to discrepancies with the observed and recorded features of the constructed project, including the project being non-existent; and 12% of issues are related to officials refusing to cooperate in some way with the audit process.

15. While the audit reports provide information on the identified issues, they currently do not contain information about the resolution or follow-up on the issue. Anecdotally, elected and appointed bureaucrats at the GP have lost their jobs (or were potentially transferred) as a result of issues uncovered during the audit.

2.3 The information environment and bureaucrat incentives

Bureaucrats can anticipate their next audit when selection for audit is predictable. In this setting, selection rotates across GPs, i.e. GPs will not be selected for their second audit until all GPs receive their first audit (via randomization without replacement). In response, bureaucrats may adjust opportunistic behavior to influence the outcome of an anticipated audit. These strategic adjustments are possible because current performance may be part of an anticipated future audit (given audits are of last FY's work). This section summarizes what bureaucrats knew and what we can infer about their incentives in response to the monitoring policy.

Every year from FY2016-2019 (the study period), all GPs received an announcement stating who has been selected for audit this year, when those audits will happen, and that last FY's work will be audited.¹⁶ The announcement in Year 1 stated that the audit agency plans to eventually target 50% of GPs for audit per year and that all GPs be audited regularly.¹⁷ It also stated that this fell short of the benchmark in the 2006 NREGS Act Section 17 requiring all GPs be audited twice a year. With this information, it would have been reasonable to expect that GPs take turns being audited.¹⁸

One's selection for audit could be anticipated, but was not perfectly predictable. Current audit capacity was observed from the announcements, but future audit capacity was unknown. So, the number of years it would take to complete the round of first audits was not known ex-ante. In Year 1 (FY 2016-17), 548 GPs were audited; 1,495 GPs in Year 2 (FY 2017-18); and 2,137 GPs in Year 3 (FY 2018-19). By the end of Year 3, the audit agency completed the round of first audits. In Year 4 (FY2019-20), they began the round of second audits.¹⁹ Predictability of a future audit is driven by observed changes in audit

16. The timing of the announcements varied across years, where the Year 1 announcement occurred during the last third of FY and during the beginning of the FY for Years 2 and 3 (see Table A.1 for details).

17. This announcement included a press and video conference with all district officials to disseminate the announcement.

18. Results from a small-sample survey (see Appendix Section A.4) along with the differences in anticipating an audit prior to Waves 1 and 2 (see results from Appendix Section C.2) provide evidence supporting that GP bureaucrats on average anticipated audits consistent with the turn-taking implemented via the randomization without replacement.

19. Around 300 GPs were not selected for audit under this policy for various reasons. They are described

capacity (to infer future capacity) and the remaining number of GPs waiting to receive their first audit.

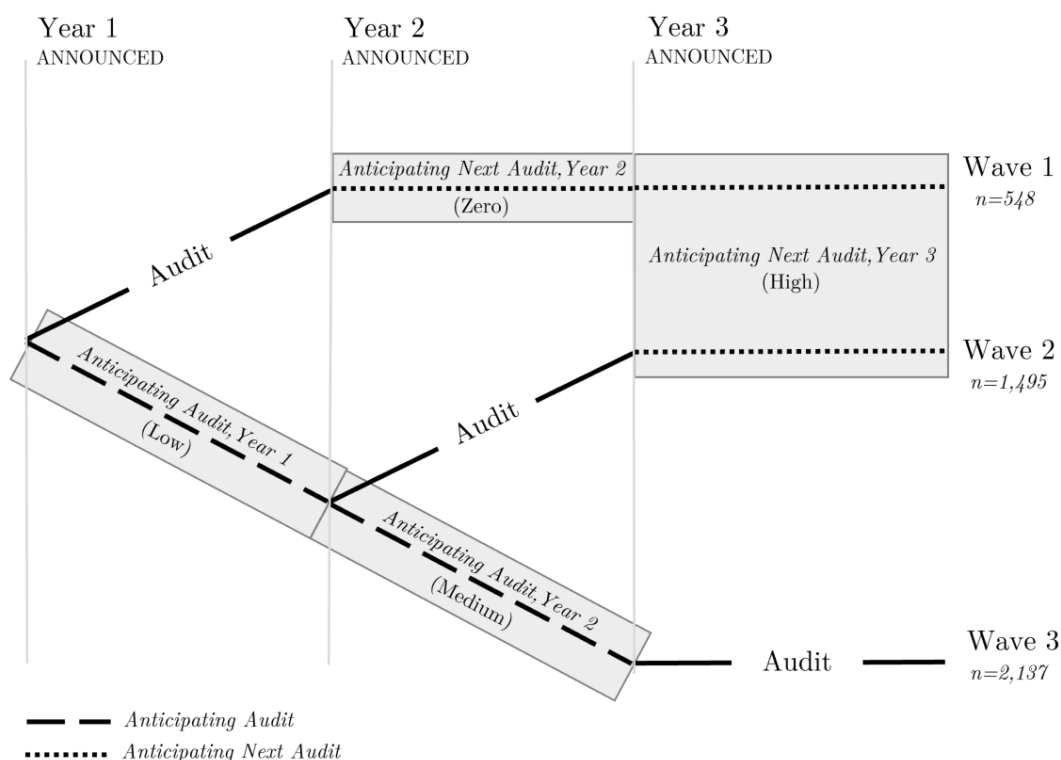


Figure 1: Roll-out of the round of first audits and evolution of beliefs for each wave. Dashed and dotted lines denote horizons of anticipating the first and second audits, respectively. Wave 1 follows the path on the top; Wave 2 follows the path in the middle; and Wave 3 follows the path on the bottom. Anticipation horizons in shaded grey areas are crudely referred to as the ‘Zero’, ‘Low’, ‘Medium’, ‘High’ probability groups for ease of interpretation in later results. Timing of the announcements is not drawn to scale.

With predictability over when one’s audit might occur, we can expect bureaucrats to act in anticipation of future audits. Figure 1 illustrates how expectations of a future audit for each wave may evolve over the three years of roll-out. Dashed and dotted lines denote horizons of anticipating the first and second audits, respectively. Wave 1 follows the path on the top; Wave 2 follows the path in the middle; and Wave 3 follows the path on the bottom.

As we move from Year 1 to Years 2 and 3, bureaucrats who have not had their first in detail in Section 4.2 and dropped from analysis. More details on notices that were publicly disseminated can be found in Appendix A.3.

audit have increasing expectations that this year's performance will be audited next year. E.g. Wave 3's expectations that current performance will be audited increases over time (i.e. horizon denoted by the dashed lines, crudely referred to as the 'Low' and 'Medium' probability horizons in Year 1 and 2, respectively, for ease of interpretation of later results). While Wave 1 bureaucrats believe the likelihood they will be audited a second time in Year 2 is very low since there are a sufficient number of other GPs waiting for their first audit (i.e. horizon denoted by the dotted line during Year 2, crudely referred to as the 'Zero' probability horizon). On the other hand, as bureaucrats observe the completion of the round of first audits in Year 3, Waves 1 and 2 bureaucrats have a very high expectation of receiving a second audit in Year 4 (i.e. horizons denoted by the dotted lines during Year 3, crudely referred to as the 'High' probability horizon). About 84% of Waves 1 and 2 GPs compared to 18% of Wave 3 GPs were audited in Year 4.

Additionally, bureaucrats also have an incentive to adjust behavior while their audit is occurring. During the audit, auditors work and sleep at the GP office where NREGS administrative matters take place. And part of the information gathered during the audit process is reflective of concurrent program performance, as discussed in Section 2.2. We will crudely refer to the Month of Audit as the probability 'One' horizon.

3 A Model of Information Design and Deterrence

This section presents a model of information design used to study the NREGS monitoring policy in Jharkhand. In the model, the Principal (e.g. audit agency) communicates information about the monitoring policy allowing the Bureaucrat to learn about the likelihood of being audited. The Bureaucrat then decides on expenditures to misappropriate. The Bureaucrat's choice affects payoffs to both the Principal and the Bureaucrat. Section 3.2 solves for the optimal signal and provides conditions for when the Principal would prefer to send informative signals, i.e. information that helps the Bureaucrat better predict when the audit will occur. The shape of the relationship between the Bureaucrat's choice with

respect to their expectations of being audited is the object of interest. This relationship is a sufficient statistic for determining the Principal's optimal signal and for evaluating how alternative signals affect the Principal's welfare. This sufficient statistic is estimated for this empirical setting in Section 6.

The following example illustrates the main intuitions of the model and provides conditions under which selecting bureaucrats for audit randomly with or without replacement is better.

Example (*To randomize with or without replacement?*). Consider a Principal who is deciding between randomly selecting N Bureaucrats for audit with or without replacement over two periods. There is greater predictability of when an audit will occur when selection is without replacement because Bureaucrats observe who is audited and waiting-to-be-audited. The Principal is interested in deterring misappropriated expenditures by Bureaucrats. In every period, the Principal only has the capacity to conduct $M \ll N$ audits or share $q_0 = \frac{M}{N}$ of Bureaucrats. Let $U(q)$ be the amount of deterrence of misappropriated expenditures for some likelihood of audit q . In the first period, a likelihood q_0 of audit for all N Bureaucrats leads to a deterrence of $U(q_0)$. In the second period, deterrence is $U(q_0)$ under randomization with replacement. Under randomization without replacement, the deterrence in the second period is $q_0 U(0) + (1 - q_0) U\left(\frac{q_0}{1 - q_0}\right)$. The share q_0 audited in the first period will not be audited in the second period, they know this, and behave accordingly (first term). While those waiting to be audited (share $1 - q_0$) believe they will be audited with probability $\frac{q_0}{1 - q_0}$ and adjust their behavior accordingly (second term). This gives us $U(q_0) \leq q_0 U(0) + (1 - q_0) U\left(\frac{q_0}{1 - q_0}\right)$. Jensen's inequality tells us that the convexity of $U(\cdot)$ is a necessary and sufficient condition for determining when auditing without replacement yields more deterrence than with replacement.²⁰ The shape (or convexity) of $U(\cdot)$ is determined by the relative benefit to bureaucrats from misappropriating additional expenditures considering the costs from being caught.

20. This simple 2-period example can be easily extended to match our 3-period empirical setting and would yield the same conclusions.

3.1 Setup

There is a Principal who oversees implementation of a government program by N Bureaucrats. Consider the Principal's interaction with a single, arbitrary Bureaucrat. This model is static and two-player but accommodates settings, such as this empirical setting, with N Bureaucrats and T periods under reasonable assumptions.²¹ The Bureaucrat privately benefits from misappropriated expenditures, while the Principal is made worse off.

The Principal chooses a communication policy, π , that conveys to the Bureaucrat the likelihood of an audit. The Bureaucrat is uncertain whether current performance will be audited. Their expectations of being audited inform their choice on the amount of expenditures to misappropriate. Formally, the Principal announces π , a chosen probability distribution over likelihoods of audit. I assume that the Principal commits to the communication policy: once π is announced, the signal received by the Bureaucrat is obtained from the distribution π .

The Principal is budget-constrained and can only audit M Bureaucrats, where $M < N$. The Bureaucrat knows this. If the Principal provides no information, each Bureaucrat is expected to be monitored with equal likelihood, given the Principal's capacity constraint. That is, the Bureaucrat has a prior about the likelihood of an audit, $q_0 = \frac{M}{N}$. Upon receiving a signal, the Bureaucrat forms the posterior belief about the likelihood of an audit, q , according to Bayes' rule. Using updated information, the Bureaucrat chooses an action $a \in [0, 1]$, which is the share of expenditures to misappropriate.

The Bureaucrat's expected utility, $V(a, q)$, is a function of the benefits from the misappropriated expenditures a and the expected punishment from audit that happens with probability q . Assume that $V(a, q)$ satisfies the *single-crossing property*, i.e., for all $q' > q$, $a' > a$, if $V(a', q) - V(a, q) \leq 0$ then $V(a', q') - V(a, q') \leq 0$. This assumption means that if a Bureaucrat who expects the audit with probability q prefers a (where $a < a'$), he would make the same choice if the probability of audit was q' (where $q' > q$). For ex-

21. See Appendix B.1 for the assumptions. The application of these assumptions to the empirical setting is discussed in Section 6.1.

ample, $V(a, q) = v(a) - qc(a)$ where $v(a)$ is the net benefit from misappropriating a and $c(a)$ is the punishment for choosing a . We abstract away from modeling heterogeneity in the Bureaucrat's propensity to misappropriate (e.g. ability to misappropriate finances) because assignment to a signal from π is random. We can think of the Bureaucrat's response, $a(q)$, given belief q , as holding all other factors affecting $a(\cdot)$ equal.

The Bureaucrat chooses an action that maximizes expected utility:

$$a^*(q) = \arg \max_{a \in A} V(a, q)$$

The Principal's utility is $u(a(q)) = -ka(q) + b$, where $k \in \mathbb{R}^+ \setminus \{0\}$ and $b \in \mathbb{R}$ are constants. The Principal's utility is net of the costs for conducting M audits. Naturally, the Principal's utility is decreasing in a , i.e. $u'_a = -k < 0$. The change in the Principal's utility with respect to q , is negatively proportional to the change in $a(q)$ with respect to q . This means that the convexity of $u(a(q))$ is determined by the (negative) convexity of $a(q)$. This is important because we can interpret $u(a(q))$ as the deterrence of misappropriated expenditures (scaled and shifted by constants k and b , respectively).

The Principal's expected utility, $\mathbb{E}_{q \sim \pi(\cdot)}[u(a(q))]$, sums over the likelihood of posterior beliefs q induced by the set of signals that can be drawn from π . The Principal will choose a communication policy, π^* , in order to maximize this expected utility given the Bureaucrat's best response, $a^*(q)$.

Example (*A fully informative signal*). For example, the Principal may consider a π that provides or does not provide additional information about the audit over the prior. The prior, q_0 , is the unconditional likelihood of being audited and determined by the capacity for audits announced every year. To implement a π that perfectly informs Bureaucrats about an audit or not, the Principal can choose a signal space where the Bureaucrat could draw signal $s \in \{H, L\}$: being selected for audit with high (H) or low (L) likelihood. The

Principal can design π so that:

$$\pi(H|\text{Audit}) = 1 \quad \pi(H|\text{No Audit}) = 0$$

$$\pi(L|\text{Audit}) = 0 \quad \pi(L|\text{No Audit}) = 1$$

This design of π perfectly informs Bureaucrats whether they will be audited or not. By Bayes' Rule, the share q_0 of Bureaucrats who drew signal H would have posterior beliefs of the likelihood of an audit of $q_H = 1$ and the remaining $1 - q_0$ who drew signal L would have posterior belief $q_L = 0$.

The timing of this game is as follows:

- (1) Principal commits to communication policy, π , about the likelihood of an audit.
- (2) Nature draws a signal for Bureaucrat from distribution π .
- (3) Bureaucrat observes the signal and forms posterior beliefs q about the likelihood of the audit.
- (4) Bureaucrat chooses, $a \in A$, the share of resources to misappropriate.
- (5) Monitoring takes or does not take place. All payoffs are realized.

The equilibrium concept for this model is Bayes-Nash perfect equilibrium.

3.2 Analysis

The optimal policy depends both on the rate of change of the Bureaucrats' response to their expectations of being audited and the Principal's capacity to conduct audits.

Since $V(a, q)$ satisfies the single-crossing property, then we know that $a^*(q)$, the Bureaucrat's best response for some given posterior belief q , is weakly decreasing in q (Milgrom and Shannon 1994). With this and the fact that $u'_a < 0$, then $u(a^*(q))$ is weakly increasing in q .

For notational convenience, let $U(q) = u(a^*(q))$. $U(q)$ is the Principal's value function given $a^*(q)$. The profile of payoffs represented by $U(q)$ and weighted combinations of these payoffs are achievable using π . Recall that π can be designed to place weight on a chosen subset of posterior beliefs as long as Bayes' Rule is satisfied. That is, if the Principal designed π to place weight on posterior beliefs q' and q'' , then their payoff is a weighted combination of $U(q')$ and $U(q'')$, so long as the mean of posterior beliefs is equal to the prior q_0 .²²

Recall that the Principal will choose a communication policy, π^* , that solves:

$$\max_{\pi \in \Pi} \mathbb{E}_{q \sim \pi(\cdot)} U(q)$$

For a given q_0 , define the set C to be all convex combinations of the values of $U(q)$ such that the mean of the posterior beliefs in the convex combination is equal to q_0 . In particular, let $\lambda_1, \dots, \lambda_n$ be a vector such that $\lambda_1 + \dots + \lambda_n = 1$ and q_1, \dots, q_n is the vector of posterior beliefs, then each convex combination in the set C must be such that $q_0 = \lambda_1 q_1 + \dots + \lambda_n q_n$.²³ Define the largest payoff possible to the Principal as $\hat{U}(q_0) = \max\{C\}$. The Principal's optimal signal, π^* , achieves the payoff $\hat{U}(q_0)$.

Proposition 1 (Persuasion works). *The Principal benefits from sending an informative signal if and only if $U(q_0) < \hat{U}(q_0)$.*

All proofs are in Appendix B.3. This condition for sending an informative signal in Proposition 1 was derived in Kamenica and Gentzkow (2011). The model uses this result to provide a more general approach to solving for the models in Lazear (2006) and Eeckhout et al. (2010); our theoretical findings are consistent.

22. Bayes' Rule tells us that the mean of the posterior beliefs of the likelihood of an audit is equal to q_0 . Using the Example on signals above: $q_0 = Pr(\text{Audit}) = \pi(\text{Audit}|H)Pr(H) + \pi(\text{Audit}|L)Pr(L) = \pi(H|\text{Audit})Pr(\text{Audit}) + \pi(L|\text{Audit})Pr(\text{Audit})$. The second equality follows from the law of total probability and the third equality follows from Bayes' Rule.

23. We could have alternatively defined the set of attainable payoffs using convex hulls. Define $co(U(q))$ to be the convex hull of the function $U(q)$, i.e. the set of all convex combinations of the values of $U(q)$. $\{u | (q_0, u) \in co(U(q))\}$ is the set of attainable payoffs for the Principal, where we are restricted to $(q_0, u) \in co(U(q))$ by Bayes' Rule. The largest payoff possible to the Principal for some given prior q_0 is $\hat{U}(q_0) = \max\{u | (q_0, u) \in co(U(q))\}$.

Proposition 1 says that the optimal signal depends on the elasticity of the Bureaucrat's response with respect to their expectations of being audited. For example, if the Bureaucrat is only responsive to the incentives from monitoring when their expectations of being audited are very high, then it would be better to inform them of audits in advance than to maintain uncertainty. In contrast, if the Bureaucrat is more responsive on the margin when there is uncertainty around the likelihood of being audited compared to knowing for sure, then the Principal is better off maintaining uncertainty. The following result further develops the intuition of Proposition 1.

Proposition 2 (When providing more information is better). *If $U(q)$ is convex at q_0 , then $U(q_0) < \hat{U}(q_0)$ and an informative signal is preferred. This result implies the following about the optimal signal given the shape of $U(q)$:*

- (i) *The Principal prefers an informative over uninformative signal for any q_0 if $U(q)$ is convex for all q . The Principal prefers an uninformative signal for any q_0 when $U(q)$ is concave for all q .*
- (ii) *An informative signal is still preferred if $U(q)$ is convex at q_0 and concave elsewhere for some q .*

To construct the optimal signal, we can find the set of distributions of posterior beliefs which achieves payoff $\hat{U}(q_0)$. With the posterior distribution of interest, we can backout a signal that induced it. A geometric interpretation of this solution means that a Principal prefers to send an informative signal if $U(q)$ is convex at q_0 . The shape of $U(q)$ determines the optimal signal. Moreover, the signal that induces the payoff $\hat{U}(q_0)$ is the optimal signal.

Figure 2 provides illustrations of when informative versus uninformative signals are better. The bold dotted line represents the set C , the payoffs attainable by the Principal. The top-left panel of Figure 2 shows that $\hat{U}(q_0)$, i.e. informing some they will be selected for audit with a very high likelihood (signal H leading to posterior q_H) and others

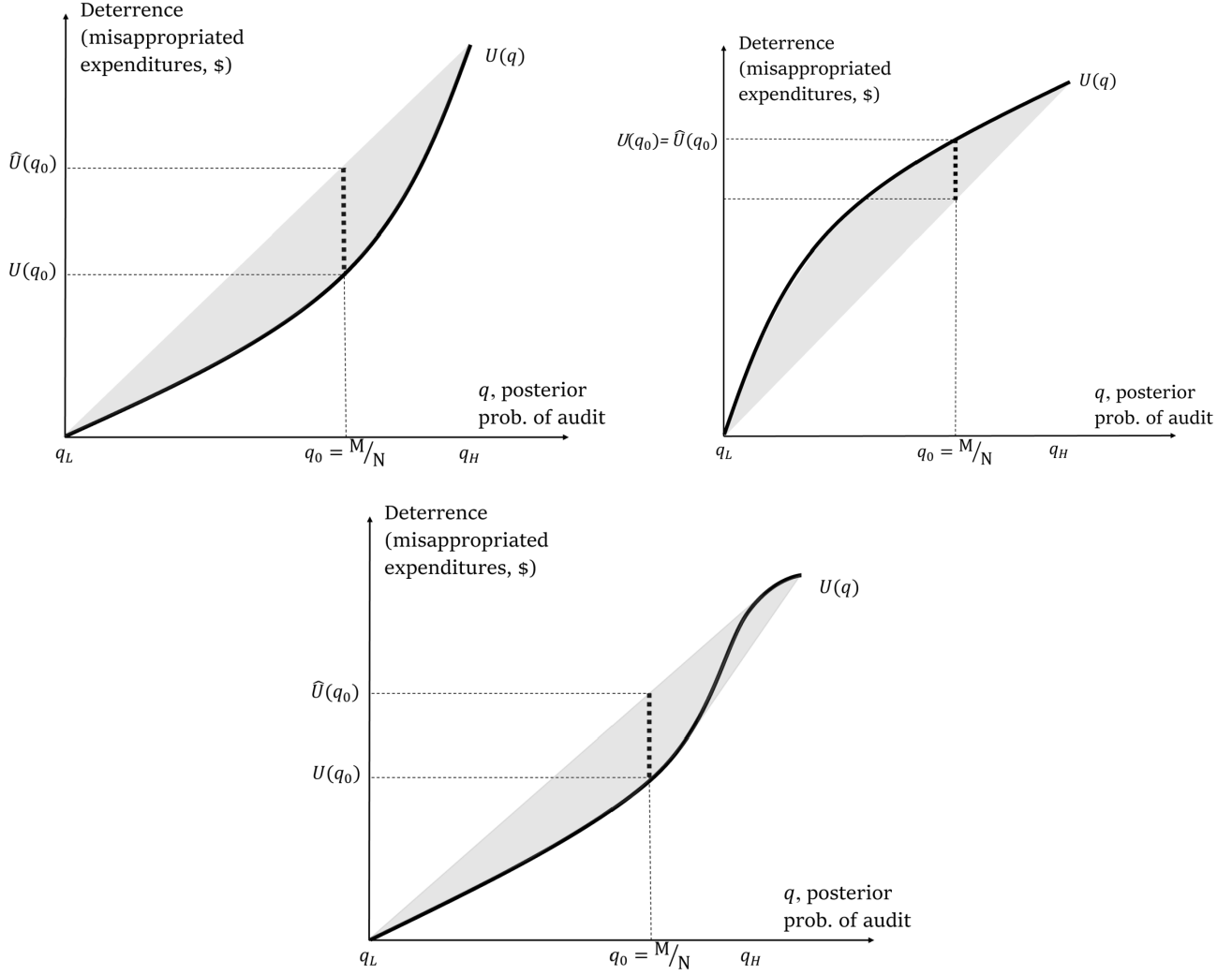


Figure 2: When does the Principal prefer an informative over uninformative signal? These figures represent the Principal's utility as a function of the Bureaucrat's posterior beliefs. When the function is convex at q_0 (e.g. top left panel), the Principal prefers to communicate additional information about the probability of audit. When the function is concave for all q (e.g. top right panel), the Principal prefers to communicate no additional information about the probability of audit. If the function is convex only for some q but concave elsewhere (e.g. bottom panel), then the Principal prefers to communicate more information if the function is also concave at q_0 .

they will not be audited (signal L leading to posterior q_L), is greater than $U(q_0)$, i.e. uninformative signal when everyone believes they will be audited with probability q_0 . To achieve utility $\hat{U}(q_0)$, our signal must place weight on posterior beliefs q_H and q_L such that $q_0 = Pr(H)q_H + (1 - Pr(H))q_L$. To solve for $Pr(H)$, we can then use Bayes' Rule to deduce the optimal signal by solving for $\pi(H|\text{Audit})$ and $\pi(L|\text{Audit})$. If $U(q)$ is concave for all q (e.g. top-right panel of Figure 2), a signal where posteriors equal the prior q_0 is optimal. Finally, if the function is convex only for some q but concave elsewhere (e.g. bottom panel of Figure 2), then the Principal prefers an informative signal if the function is also concave at q_0 .

Ultimately, the shape of $U(q)$ in a particular setting must be learned empirically. It is a sufficient statistic for determining the optimal signal and analyzing the Principal's welfare under alternative signals. If $U(q)$ is convex at prior q_0 , then communicating more information about the likelihood of being audited is better than maintaining uncertainty over when the audit will occur. With the random selection of GPs for audit without replacement in this empirical setting, I estimate this sufficient statistic with causally-identified empirical moments in the data and apply the results from Proposition 2 to determine the best policy on communicating the likelihood of being audited (Section 6). I estimate this sufficient statistic without needing to specify additional primitives underlying the Principal's choice problem.

4 Empirical Strategy

This section discusses the identification strategy and econometric specifications; the challenges of measuring corrupt behavior in response to the monitoring policy; and the approach for using changes in program expenditures as a proxy for changes in misappropriated spending in response to the monitoring policy.

4.1 Estimating equation and identification assumptions

We are interested in estimating how bureaucrats respond during time t given their expectations of the likelihood that performance in time t will be audited. I use a differences-in-differences model to estimate bureaucrat responses as expectations change during various stages of the monitoring policy. The following specification estimates the intent-to-treat from learning one has been selected for their first audit:

$$y_{it} = \alpha_i + \alpha_{dt} + \text{AnticipatingAudit}_{it}'\beta + \delta_0 \text{PostAnnounce}_{it} + \varepsilon_{it} \quad (1)$$

where α_i denotes fixed effects controlling for time-invariant unobservables for each GP i ; α_{dt} denotes fixed effects controlling for GP-invariant unobservables for each month t within a district d ; ε_{it} is the residual error term.²⁴ Standard errors are clustered by block—one administrative level above the GP and the unit of stratification for being selected for audit each year. y_{it} denotes the GP performance outcome. The main outcome is total expenditures, measured as the sum of wage and material expenditures.²⁵ Section 4.4 discusses why this outcome measure is reasonable in this particular setting for making inferences on bureaucrat changes in financial misappropriation as expectations of being audited change. Results from Sections 5.2 and 5.3 provide further evidence that estimated changes in this outcome measure reflect changes in misappropriation. Each component of bureaucrat behavior captured by the remaining variables in Equation 1 and

24. Districts are two administrative levels above the GP. The GP fixed effects also help account for stratification in the randomized roll-out at the block administrative level. In the 2016-17 audit, the randomization was stratified by block (one administrative level higher than GP). In the 2017-18 audit, the randomization was stratified by block with an additional rule and selection was among the GPs not incorporated in the 2016-17 audit. The additional rule was that all GPs within a block would be selected for audit if there were 10 or fewer GPs remaining to be audited within that block (27% of the blocks in Jharkhand have ≤ 10 GPs; 42% of blocks had ≤ 10 GPs left to be audited by 2017-18). Using the announcement data to check, an average of 98% of GPs within these blocks were audited. For the 2018-19 audit, the remainder of unaudited GPs were selected for audit. In these waves of the audit, we would expect to have independence in observed and unobserved variables between the treatment and control groups conditional on block fixed effects, which also controls for the number of GPs in a given block during this time.

25. The excluded component of expenditures is administrative expenditures because panel data are currently not available for this measure. This is of limited concern because administrative expenditures are a negligible share of total expenditures (0.4% on average).

their identification will be discussed in turn.

First, consider $PostAnnounce_{it}$, a dummy variable capturing the period after each GP learns from announcement of their selection for the first audit. This variable captures the intent-to-treat of informing bureaucrats of their audit. The effect of $PostAnnounce_{it}$ on GP performance y_{it} would be identified under the assumption of parallel trends between those announced for their first audit and not yet announced.

However, being able to anticipate one's first audit is a potential threat to the parallel trends assumption needed to identify δ_0 , the effect of $PostAnnounce_{it}$. With the random assignment to audit in Year 1, we should expect parallel trends between Wave 1 and non-Wave 1 groups prior to announcement.²⁶ But, we should not expect parallel trends for subsequent waves because random assignment without replacement under the information environment can lead to anticipation. Section 2.3 discusses this anticipatory behavior.

We can control for these horizons of anticipation with $AnticipatingAudit_{it}$ in order to identify $PostAnnounce_{it}$. $AnticipatingAudit_{it}$ is a vector of two dummy variables which capture anticipation for those waiting for their first audit separately as they observe the announcements in Years 1 (β_0) and 2 (β_1), corresponding to the 'Low' and 'Medium' probability horizons referenced in Figure 1. Importantly, the estimates β are also parameters of interest because they capture strategic responses by bureaucrats as expectations of being audited vary. Given random assignment without replacement to audit each year, bureaucrat expectations of when their first audit will occur are also randomly assigned, allowing us to identify β .

26. To illustrate, under the standard difference-in-differences estimator with two periods in the potential outcomes framework, we have that

$$\hat{\delta}^{DiD} = (\bar{Y}_{1,D=1,t_0} - \bar{Y}_{0,D=1,t_0}) + [(\bar{Y}_{0,D=1,t_0} - \bar{Y}_{0,D=1,t_0-1}) - (\bar{Y}_{0,D=0,t_0} - \bar{Y}_{0,D=0,t_0-1})]$$

where the second bracketed term is the difference between the counterfactual trend of the treatment group and the trend of the control group; D denotes treatment status; t_0 is the period of treatment; and (Y_0, Y_1) denote potential outcomes of receiving treatment or not. From randomization of the audit, the following must hold:

$$(Y_{0t}, Y_{1t}) \perp\!\!\!\perp D$$

which implies that $\mathbb{E}[Y_{0,D=0,t_0}] = \mathbb{E}[Y_{0,D=1,t_0}]$ and $\mathbb{E}[Y_{0,D=0,t_0-1}] = \mathbb{E}[Y_{0,D=1,t_0-1}]$, and is sufficient for $\hat{\delta}^{DiD}$ to be an unbiased estimate of the treatment effect.

The following is the main specification of this paper, which disaggregates behavior during $PostAnnounce_{it}$ and estimates the profile of bureaucrat responses as expectations of being audited change during the roll-out. Estimating the effect of these periods of changing expectations is effectively an event study specification of Equation 1. It groups months around the announcement into their respective horizons of anticipation and periods within an audit year.

$$y_{it} = \alpha_i + \alpha_{dt} + AnticipatingAudit_{it}'\beta + \underbrace{\delta_1 BeforeAudit_{it} + \delta_2 MonthofAudit_{it}}_{PostAnnounce, \text{disaggregated}} \quad (2)$$

$$+ \underbrace{\delta_3 AfterAudit_{it} + AnticipatingNextAudit_{it}'\gamma}_{PostAnnounce, \text{disaggregated (continued)}} + \varepsilon_{it}$$

$AnticipatingAudit_{it}$ is as described above. The remaining variables are mutually exclusive groups and are defined such that $PostAnnounce_{it} = BeforeAudit_{it} + MonthofAudit_{it} + AfterAudit_{it} + AnticipatingNextAudit_{it}$. The anticipation variables in Equation 2 correspond to the horizons of anticipation in Figure 1. The variable $BeforeAudit_{it}$ captures the period once one learns they will be receiving their first audit but before the first audit occurs. $MonthofAudit_{it}$ captures the period during the month of audit, which we also crudely refer to as the probability ‘One’ horizon. $AfterAudit_{it}$ captures the months following the first audit, but prior to learning information from the next announcement. The estimated coefficients for these variables capture the effect of an active audit on bureaucrat behavior as well as any persistent effects of experiencing an audit. While GPs were randomly assigned for audit each year, the order in which audits occur within each year is non-random. According to the audit agency, they designed the schedule to complete audits within a district in time for higher-level hearings (for unresolved issues) and to be logistically practical. I assume that district-specific time fixed effects accounts for the non-random timing of audits.

Finally, consider $AnticipatingNextAudit_{it}$, a vector of two dummy variables which capture the anticipatory horizons in Years 2 (γ_0) and 3 (γ_1) for those anticipating their

second audit, corresponding to the ‘Zero’ and ‘High’ probability horizons referenced in Figure 1. $AnticipatingNextAudit_{it}$ is mutually exclusive from the behavior captured by $Audit_{it}$. The coefficient estimates of $AnticipatingNextAudit_{it}$, γ , are parameters of interest because they capture strategic responses by bureaucrats anticipating second audits as the round of first audits progress. γ is identified if it is not confounded with persistent effects of the audit. We can test for persistent effects with $Audit_{it}$. We can also estimate an event study and test whether behavior after the audit (controlling for variation from $AnticipatingNextAudit_{it}$) is statistically different from behavior before the audit (result reported in Section 5.3).

All regressions use ‘*AnticipatingAudit* - Year 1’ as the reference group; otherwise, $AnticipatingAudit + PostAnnounce$ would be collinear with a linear combination of time fixed effects. The reason is periods captured by *AnticipatingAudit* and *PostAnnounce* are defined by when annual announcements for audit are released, and this affects all GPs within a wave simultaneously. Where for every announcement, a GP is either anticipating their first audit or has received an announcement for their first audit.

4.2 Data sources and sample restrictions

2011 Village Census of India The 2011 Village Census of India provides data on GP demographics, local economy, household and village amenities, and natural resources (Ministry of Home Affairs 2018). Census data are at the ward level, one administrative unit below the GP. Ward-level data aggregate to provide GP-level data. These data help us check the integrity of the random assignment in tests of balance on observable characteristics.

Annual audit announcements The audit agency provided documentation on the announcements from 2016-2020. Announcements detail GPs selected for audit and audit dates. Together with the announcement dates, this information helps us capture the effect during periods of anticipation discussed in Section 2.3; and the effect of learning of an upcoming audit and experiencing an audit.

NREGS administrative data NREGS management information system (MIS) provides the data for all bureaucrat performance outcomes (Ministry of Rural Development 2019). MIS is a national government data portal that tracks detailed information on program implementation in each GP.²⁷

Outcomes on employment include wage expenditures; person-days of employment; and days of delayed payment across all households. Data on projects include details on material procurement, and expenditures on labor and materials. Data on expenditures correspond to program outlays and are an upper bound of actual employment and materials provided through the program. Anecdotal evidence from interviews with government officials suggest that once expenditures have been paid, they cannot be manipulated in administrative data by bureaucrats. Panel datasets constructed with these outcomes are by GP-month from April 2014 to March 2019.

Audit characteristics and outcomes from audit reports include share of portfolio audited, number of auditors, documented issues, and fines assessed for these issues. The audit reports are from MIS and used to construct a GP-level dataset. This is used to compare audit performance across waves with differing anticipatory behavior. Currently, only a subset of audit reports are available for analysis, and are only from Waves 2 and 3 of the audit.²⁸

Sample restrictions By the end of Year 3, audits were conducted in 4,180 GPs or 93% of all GPs in Jharkhand. Around 300 GPs were not selected in the audit calendar for the following reasons: 1) 220 GPs had special audits at the request of upper-level government officials²⁹; 2) 49 GPs were audited during the pilot; and 3) an even smaller

27. Access MGNREGA MIS [here](#) and an MGNREGA Village View Dashboard [MGNREGA MIS Dashboard here](#).

28. Among audits conducted in Waves 2 and 3, 77% of audit reports are available for analysis. In tests comparing pre-audit GP characteristics of Waves 2 and 3 with available audit reports, there is balance on GP population characteristics, number of auditors, and total expenditures under audit. There are statistically significant differences in wage expenditures and the number of projects under audit, which will be included as controls in the regressions with audit reports data.

29. Requests for audits can be submitted to the audit agency by higher-level government officials; they are referred to as special audits. A majority of the special audits that took place during the study period were initiated when Chief Secretary of the Government of Jharkhand requested special audits in two districts in 2017-18 upon observation of suspicious behavior during a statewide progress report meeting. These special

number of remaining GPs did not have any NREGS expenditures or were undergoing an administrative boundary change. Furthermore, 112 GPs were selected for audit twice over the audit roll-out. A subset of these 112 GPs were audited twice because they were also selected for special audit; for the remainder, they were selected for audit twice by mistake based on conversations with technical specialists at the audit agency. The sample for analysis omits observations from GPs that meet the following criteria: (1) were ever selected for a special audit; (2) were audited during the pilot; or (3) were audited more than once. This leaves 4,052 GPs in the sample for analysis in the unbalanced panel, and 3,897 GPs in the sample for analysis in the balanced panel. All analyses use the balanced panel.

4.3 Tests for violations of identifying assumptions

Using census and administrative data, statistical tests show balance on observable characteristics across waves (Appendix Table C.1). Except there are statistical differences between Waves 2 and 3 Scheduled Tribes population, and share of person-days of labor allocated to Scheduled Caste. And differences across waves in percent of expenditures in labor. The number and extent of these differences are consistent with arising by chance. Overall, the differences are small (5% difference in Scheduled Tribe population; 6% difference in percentage of person-days Scheduled Caste; and less than or around a percentage point difference in percent of expenditures in labor), and differences in variation in demographic parameters that tend to be stable over time can be accounted for with GP fixed effects in our main specification.

To test for parallel trends, Figure 3 shows an event study of total expenditures with lags and leads around the month of announcement and including only ‘*AnticipatingAudit* - Year 2’ as a control for anticipatory behavior.³⁰ There is no statistically distinguishable trend during the months before the announcement (p -value = 0.45). This lends credibility

audits were also publicly announced. While GPs receiving special audits are not included in the sample for analysis, I account for the information learned by bureaucrats from the announcement on special audits.

30. Recall that ‘*AnticipatingAudit* - Year 1’ is the reference group. See discussion in Section 4.1.

to our difference-in-differences approach. During the months following the announcement, expenditures decline (more on this is Section 5). Additionally, there is no evidence of pre-trends for wage and material expenditures (p -value = 0.19 and 0.99, respectively; see Appendix Figure C.2).

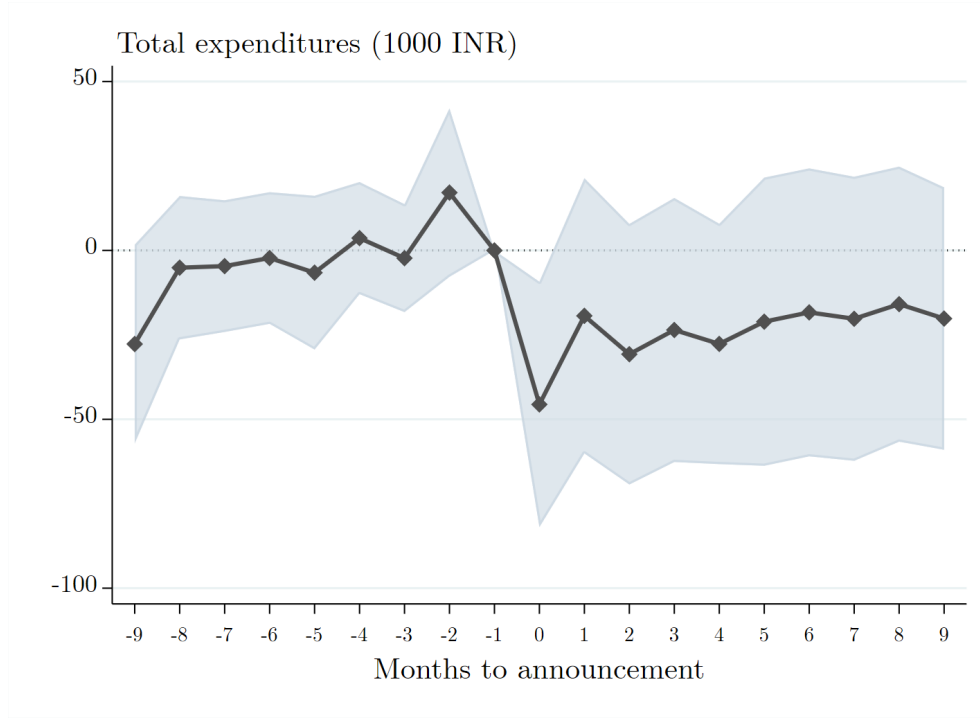


Figure 3: Announcement event study for total expenditures (1000 INR). The omitted category in this regression is the one month lead before the announcement. The regression includes GP and district month-year fixed effects. Standard errors are clustered by block.

For robustness, I estimate the same event study excluding variables capturing potential anticipatory behavior of the first audit and also cannot reject the null (Appendix C.2). However, due to potential anticipatory behavior in subsequent waves, we should not expect parallel trends between GPs in Wave 2 and Wave 3 without controlling for their horizons of anticipation. Analyzing the event studies by wave, there is evidence of pre-trends leading up to the Year 2 announcement (Appendix C.2). This supports our primary difference-in-differences specification in Equation 1 which accounts for horizons of anticipating the first audit. These findings support our interests in estimating anticipatory behavior. Finally, the results in Appendix C.2 are without GPs *already-treated*

with an announcement, which addresses concerns documented in the recent literature on implementing two-way fixed effects estimators with staggered adoption and heterogeneous treatment effects (Goodman-Bacon 2018; Sun and Abraham 2020; Callaway and Sant’Anna 2021).

4.4 Inferring deterrence from administrative data

The main analysis of this paper estimates the effect of the monitoring policy on total expenditures. Not misappropriated expenditures, a measure we cannot observe. I argue that changes in expenditures driven by the monitoring policy are a reasonable proxy for *changes* in misappropriated expenditures (or the deterrence effect), and thus an appropriate measure for the Principal to optimize over given their specified utility (Equation 3.1).

Bureaucrats misappropriate NREGS public funds by over-reporting employment (Niehaus and Sukhtankar 2013; Muralidharan et al. 2016; Banerjee et al. 2020). Audit reports suggest expenditures can be misappropriated through material procurement. Example issues from audit reports include fake receipts, higher-than-expected prices for low-price goods, and procured materials missing from worksites. Interviews and media outlets provide anecdotal evidence that corrupt bureaucrats will expense overreported labor for manual-labor-intensive projects and use machines to complete the public projects instead.³¹ Auditors have also documented the use of machines to complete otherwise non-existent but billed projects.

An independent verification of employment and material procurement over time would be the ideal measure of the effect of the monitoring policy. It would allow us to compare reported versus actual outcomes, and measure overreporting. But, this information is ex-post unavailable, as are third-party sources of verification over time. In addition, Appendix C.4 emphasizes the importance of using monthly data to study the response of bureaucrats to the monitoring policy over time. It shows that measures of annual per-

31. E.g. see news article [here](#) describing how corruption prevents participants accessing benefits of the program.

formance are too coarse to draw conclusions about bureaucrats' strategic adjustments to the monitoring policy from year to year. To my knowledge, the administrative data are the only data that provides measures of GP by month performance for the duration of the study period.

Other studies construct proxies for over-reported employment by estimating the share of fake households under NREGS (Niehaus and Sukhtankar 2013; Banerjee et al. 2020). Banerjee et al. (2020) do so at scale by comparing administrative records of working households with households documented in the 2012 Socioeconomic Caste Census (SECC).³² A similar exercise in our empirical setting would not work for two reasons. First, during the beginning of our study period, almost all (99.95%) wage payments were made directly to bank accounts. This follows a program shift from paying wages in cash to direct deposits in bank accounts. With challenges to opening fake bank accounts, this suggests that payment of overreported wages is less likely due to fabricated workers and more likely due to collusion with or coercion of real workers to overreport employment. Second, population changes since 2012 would make the comparison less reliable. For these reasons, estimating the share of fake households with the SECC is not suitable here.

To address these challenges, I study changes in monthly expenditures in response to the monitoring policy. Bandiera et al. (2009) and Londoño-Vélez and Ávila-Mahecha (2021) use a similar approach to study waste in government procurement and tax evasion. Random assignment of audits isolates the effect of incentives created by the monitoring policy on expenditures. To be clear, let c_p denote the true share of total expenditures that is misappropriated under a given audit policy p . Suppose $p \in \{0, 1\}$ where 0 denotes no audit policy and 1 denotes some audit policy. Our goal is to make inferences about $c_1 - c_0$ using administrative reports on expenditures. Let y_p represent expenditures from the administrative data under policy p . y_p aggregates honest and misappropriated expenditures under the program.

32. Imbert and Papp (2011) use another approach to compare administrative reports of NREGS employment with household survey data from the NSS on reported employment on public works, but this particular survey has not been released in recent years.

We need to rule out whether changes in p , holding all else equal, also changes behavior in honest expenditures. If we can rule out other mechanisms, then using $y_1 - y_0$ is a viable strategy to make inferences about changes in misappropriated expenditures, $c_1 - c_0$, due to the policy change. I test hypotheses on whether estimated changes in expenditures in response to the audit policy are consistent with financial misappropriation or not. This allows us to refine our interpretation of the mechanisms driving the estimated effect of the audit policy.

Finally, the outcome measures are total, wage, and material expenditures. This restricts our study of corrupt behavior to financial misappropriation. I do not have measures for other activity where the bureaucrat unlawfully leverages their position for personal gain. For example, approving public projects to construct private assets for a household as part of a collusive agreement. Furthermore, since data are at the GP-level, I can only study the collective actions of GP-level bureaucrats.³³

5 The Impact of Changing Expectations

This section shows that the deterrence effect from anticipation is strongest on the margin when one has high expectations of being audit, while under less certainty there is no detectable effect. I provide evidence we can interpret the results as deterrence effects (changes in misappropriated expenditures). For insights into the bureaucrats response, I also examine the effect of the policy on total expenditures alongside its effect on wage and material expenditures. Results show that deterrence does not come without substitution of financial misappropriation across time and type of expenditure.

5.1 Bureaucrats' response to changing likelihood of audit

Table 1 presents the fixed effects regressions: Panel A shows Equation 1 and Panel B shows Equation 2. The outcomes for are total, wage, and material expenditures. As dis-

33. See Section A.1 for description of bureaucrats at the GP level.

cussed in Section 4.1, the reference group or horizon in both panels is *AnticipatingAudit* - Year 1 (β_0 , Low).

Total expenditures decline by 7% after learning of selection for audit ($\delta_0 = -17.9$ as share of baseline 269.5, Table 1 Panel A Column 1). This effect is driven by a drop in wage expenditures (Column 2).^{34,35} Total expenditures slightly increase, though statistically insignificant, when GPs have Medium expectations ($\beta_1 - \beta_0$). Notably, this estimate is driven by a drop in wage expenditures (Panel A Column 2) and an increase in material expenditures (Panel A Column 3) on average.

Turning to the event study (Panel B), estimates under Medium expectations are qualitatively consistent with Panel A. Except, there is a 24% significant increase in material expenditures suggesting a substitution from wage to material expenditures. Similarly, when expectations are Zero (the year after Wave 1 receives their audit) there is no detectable change in total expenditures and this is coupled with an average decline in wage and increase in material expenditures ($\gamma_0 - \beta_0$). This suggests bureaucrats resume business as usual overall when they have lower expectations of being audited.

Expenditures decline substantially when expectations of being audited are high. As the roll-out completes in Year 3 and the likelihood of a second audit in Year 4 is extremely likely, there is a 15% drop in total expenditures ($\gamma_1 - \beta_0$, High). This is driven by a 15% decline in wage expenditures and a 13% statistically insignificant decline in material expenditures.³⁶ Similarly, during the month of audit when concurrent performance

34. Recall that identification of *PostAnnounce* comes from accounting for behavior captured by *AnticipatingAudit* - Year 1.

35. Qualitatively, the estimated decline in total expenditures during *PostAnnounce* is robust to relaxing the assumption that the treatment effects across waves and time are homogeneous, which can lead to negative weights on the average treatment effect on the treated in the difference-in-differences comparisons. I compute the weights following de Chaisemartin and D'Haultfœuille (2020), and find that of 72,773 ATTs, 17% have a negative weight and the negative weights sum to -0.4. Employing the estimator derived in de Chaisemartin and D'Haultfœuille (2020) which addresses the negative weights and allows for treatment heterogeneity, I find that the coefficient on *PostAnnounce* is negative and greater: -96 with a standard error of 25.

36. We also estimate Equation 2 where anticipatory behavior in Year 3 (*AnticipatingNextAudit* - Year 3', High) is disaggregated by Wave 1 and Wave 2's responses. I find that the average decline in total expenditures is 10% and 15% for Waves 1 and 2, respectively, and their responses are not statistically distinguishable from one another.

Panel A: Difference-in-differences			
	<i>Expenditures (1,000 INR):</i>		
	(1)	(2)	(3)
	Total	Wages	Materials
<i>AnticipatingAudit</i> - Year 2 [β_1 , Medium]	3.13 (10.63)	-9.07 (5.62)	12.20 (8.19)
<i>PostAnnounce</i> [δ_0]	-17.91* (9.25)	-15.43***,† (4.90)	-2.48 (7.15)
Observations	233,760	233,760	233,760
Baseline mean	269.5	187.1	82.39
Adj. R-squared	0.40	0.47	0.19

Panel B: Event study with <i>PostAnnounce</i> disaggregated			
	<i>Expenditures (1,000 INR):</i>		
	(1)	(2)	(3)
	Total	Wages	Materials
<i>AnticipatingAudit</i> - Year 2 [β_1 , Medium]	13.87 (11.33)	-5.98 (6.05)	19.84** (8.58)
<i>PostAnnounce</i> , disaggregated:			
Before <i>Audit</i> [δ_1]	-18.83** (9.46)	-10.21** (5.04)	-8.62 (7.14)
Month of <i>Audit</i> [δ_2 , One]	-42.07***,† (11.68)	-35.33***,† (5.99)	-6.74 (9.30)
After <i>Audit</i> [δ_3]	-8.42 (11.26)	-17.54***,† (5.62)	9.12 (8.70)
<i>AnticipatingAudit</i> - Year 2 [γ_0 , Zero]	-4.78 (13.83)	-14.54* (7.67)	9.76 (9.07)
<i>AnticipatingAudit</i> - Year 3 [γ_1 , High]	-39.38***,† (13.30)	-28.06***,† (7.28)	-11.32 (9.02)
Observations	233,760	233,760	233,760
Baseline mean	269.5	187.1	82.39
Adj. R-squared	0.40	0.47	0.19

Table 1: Effect of stages of the monitoring policy on Bureaucrats' response in program expenditures. This table estimates the main differences-in-differences specification for three outcome variables: total (wages + materials), wage, and material expenditures. The regressions in Panel A estimate Equation 1 and the regressions in Panel B estimate Equation 2. First term in brackets refers to the estimated coefficient; second term in brackets refers to the probability horizons referenced in Section 2.3. All regressions include district-month-year and GP fixed effects. Standard errors are clustered by block. The omitted category is the horizon of anticipating one's first audit during Year 1 (*AnticipatingAudit* - Year 1). The baseline is the mean from the beginning of the panel (two years prior to first audits) up to and including the period captured by *AnticipatingAudit* - Year 1. This longer period is included in the baseline to average out seasonal variation in expenditures. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. With a Bonferroni correction for the family-wise error rate, we reject the null hypothesis when † $p < 0.025$ for Panel A and $p < 0.008$ for Panel B.

is subject to monitoring ('Month of *Audit*', One), there is a 16% decline in expenditures largely driven by a decline in wage expenditures ($\delta_2 - \beta_0$). These results are robust to tests for spillover effects during the month your neighbors receive an audit (Appendix D.1). The estimates for γ_1 (High) and δ_2 (One) are both statistically different from behavior when expectations are lower captured by β_1 (Medium) and γ_0 (Zero). This suggests the deterrence effect from monitoring is strongest when there is greater certainty over the likelihood of an audit, while behavior when expectations are lower are statistically indistinguishable from one another.

Of note, the effects of anticipating the second audit on total expenditures are identified when there are no persistent effects from the audit (discussed in Section 4.1). The effects on total and material expenditures during the months after the audit (δ_3) are insignificant and small. There is a significant decline in wage expenditures during this period, but this effect is being driven by the decline in wage expenditures experienced 1 month after the audit. There is no effect 2+ months after the audit on wage expenditures that is statistically distinguishable from 0 (p -value = 0.25). What happens the months following the audit is explored further in Section 5.3. These results suggests the estimates for *AnticipatingNextAudit* are not confounded with persistent effects from the audit.

5.2 Deterrence and substitution in anticipation

The following results support that the estimated changes in expenditures can be interpreted as changes in deterrence or misappropriation. They also provide evidence of substitution across types of expenditures as expectations of being audited change.

Anticipatory behavior captured by $\beta_1 - \beta_0$ (Table 1 Panel B) is effectively the difference in behavior between Wave 2 and 3 in the year prior to their respective audits. We can verify the estimated differences in their wage and material expenditures by comparing audit reports from Waves 2 and 3. Table 2 tests for differences in audit performance between these waves on wage and material misappropriation.

The difference in fines related to issues of wage misappropriation between Waves 2

	Issue fine amount (1000 INR), by issue type:							
	Wage misappropriation				Material receipts misappropriation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wave 3 - Wave 2	-25.881*** (7.499)	-12.681 (7.982)	-12.674 (11.453)	18.413 (17.436)	30.943*** (10.730)	32.701** (13.253)	56.186*** (20.181)	64.490*** (17.601)
Audit manager experience				-3.638* (1.863)				-0.972 (2.136)
Wave 2 Mean	68.75	68.75	73.48	73.48	21.58	21.58	22.08	22.08
Controls		X	X	X		X	X	X
Manager FE			X	X			X	X
Observations	2,360	2,360	1,771	1,771	2,361	2,361	1,772	1,772
Adjusted R ²	0.147	0.163	0.185	0.187	0.031	0.031	0.034	0.033

Table 2: Differences in audit issue fines across waves are consistent with interpreting differences in wage and material expenditures while anticipating the first audit as misappropriated. Unit of observation is GP. All regressions include block fixed effects. Control variables: number of employed households and works to verify; and number of auditors. Audit manager experience is measured by number of audits conducted to date. Standard errors are clustered by block. *p<0.1; **p<0.05; ***p<0.01.

and 3 is statistically indistinguishable from 0 (after controlling for potential confounders). This effect is consistent with the estimate for β_1 (Medium) that spending on wages is statistically indistinguishable from zero (Table 1 Panel B). Additionally, Wave 3 experiences a threefold increase in fines related to issues of material receipt misappropriation compared to Wave 2, and this effect is highly statistically significant. This is consistent with the results that Wave 3 spent 24% more on materials during this period of anticipation. When examining the number and share of issues identified for wage and material misappropriation, the results are qualitatively similar (Appendix Table D.2 and D.3)

These results lend credibility to our interpretation that the estimated differences in bureaucrat behavior during periods of anticipation are interpretable as a deterrence effect. Moreover, an average decrease in wage expenditures and employment misappropriation issues coupled with an average increase in material expenditures and material misappropriation issues shows that bureaucrats substitute across the type of expenditure to misappropriate.

The shift from misappropriating wages to materials is specific to this context and is perhaps explained by lower detection rates of material relative to wage misappropriation. Audit reports show that issues related to wage misappropriation are more than twice as

likely to be documented compared to material misappropriation (16% vs 6%, respectively), but materials are also a smaller share of total expenditures. Or bureaucrats may be choosing material over wage expenditures because they believe material misappropriation is easier to hide from auditors, as wage misappropriation can be discovered by auditors during household interviews. Additionally, bureaucrats have been known to refuse providing program registers and receipts to auditors for verification. With this possibility, they can hide fake receipts and prevent the verification of material procurement. However, refusal to cooperate with the audit comes at a cost and is an issue documented and fined by auditors. Indeed, Appendix Table D.4 shows that Wave 3 GPs have a greater number of issues and fines related to the refusal to provide records for auditors.

5.3 Deterrence and substitution during the audit

This section provides evidence that bureaucrat behavior during the months around the audit, estimated by ‘Month of *Audit*’ in Table 1 Panel B, can be interpreted as a deterrence effect. Bureaucrats may be deterred from misappropriating expenditures because concurrent performance is being evaluated by auditors (see discussion in Section 2.2). However, experiencing a live audit introduces potential confounders to interpreting behavior as changes in misappropriated expenditures. Two alternative mechanisms potentially confound our interpretation and are tested: a disruptive audit leads to difficulties multi-tasking where usual tasks cannot be completed (Holmstrom and Milgrom 1991); and the audit helps bureaucrats learn and improve productivity (Arrow 1962; Syverson 2011).³⁷

37. Another possible explanation is that the decline in employment is demand (participant) driven and may have been caused by the audit. If this were the case, it would not reconcile with the fact that employment began to decline a month before auditors arrived where it seems unlikely for participants to adjust behavior in advance of the audit. If they did anticipate the occurrence of the audit, then we might also expect, but do not observe, behavioral adjustments once the announcements were made and prior to the audit. Furthermore, there is ample documentation of citizen complaints in the audit reports which make it unlikely that the audit deterred households from seeking employment or the benefits they are entitled, or reduced their need to be employed. The audit is likely to affect the behavior of bureaucrats over the behavior of citizens, as punishment from audits sought to punish deviations in implementation and to improve access to resources for intended participants.

There are two aspects to the multi-tasking issue while auditors are present: a disruptive audit keeps bureaucrats from (1) getting honest work done; and (2) misappropriating expenditures as they try to minimize detection while auditors are present.³⁸ The former could be a potential confounder, even though the audit process is designed to be minimally disruptive to program implementation (see discussion in Section 2.2). The latter is not a potential confounder because it contributes to changes in misappropriated expenditures.

To disentangle among potential mechanisms, I estimate event studies to examine month-to-month responses around the time of audit.³⁹ Figure 4 presents the event studies for total, wage, and material expenditures. Relative to 10 and more months prior to audit (the omitted category), total expenditures decrease by 9-12% during the month of the audit, proceed to increase by 3-6% 2-4 months after the audit though not statistically significant, and revert to pre-audit levels afterward. This effect is driven by a 17% decline in wage expenditures during the month the audit and an 24-30% increase in material expenditures 2-4 months after the audit.

We can rule out improvements in productivity by learning from the audit as a confounder. Anything learned should persist, but the observed changes are specific to the months around the time of audit. *F*-tests for each outcome cannot rule out the hypothesis of equal trends during the months before and after the period where bureaucrats respond to the audit ($p\text{-value} \in [0.14, 0.95]$).

There are several reasons why multi-tasking issues do not explain the decline in expenditures during the month of the audit.

First, while the presence of auditors affects employment and material procurement, it does not affect efficiency in making wage payments on time (Appendix Section D.4). This matters because if multi-tasking explains the decline in employment during an audit, then it may also affect other tasks, especially if these tasks are low-effort and low-consequence

Another potential, but unlikely, explanation is that the bureaucrat increased their effort in the presence of auditors. We should expect to see employment increase because more honest employment is reflective of better performance.

38. Field interviews with participants suggest that this is potentially a widespread problem.

39. See details on the specification in Appendix Section D.3.

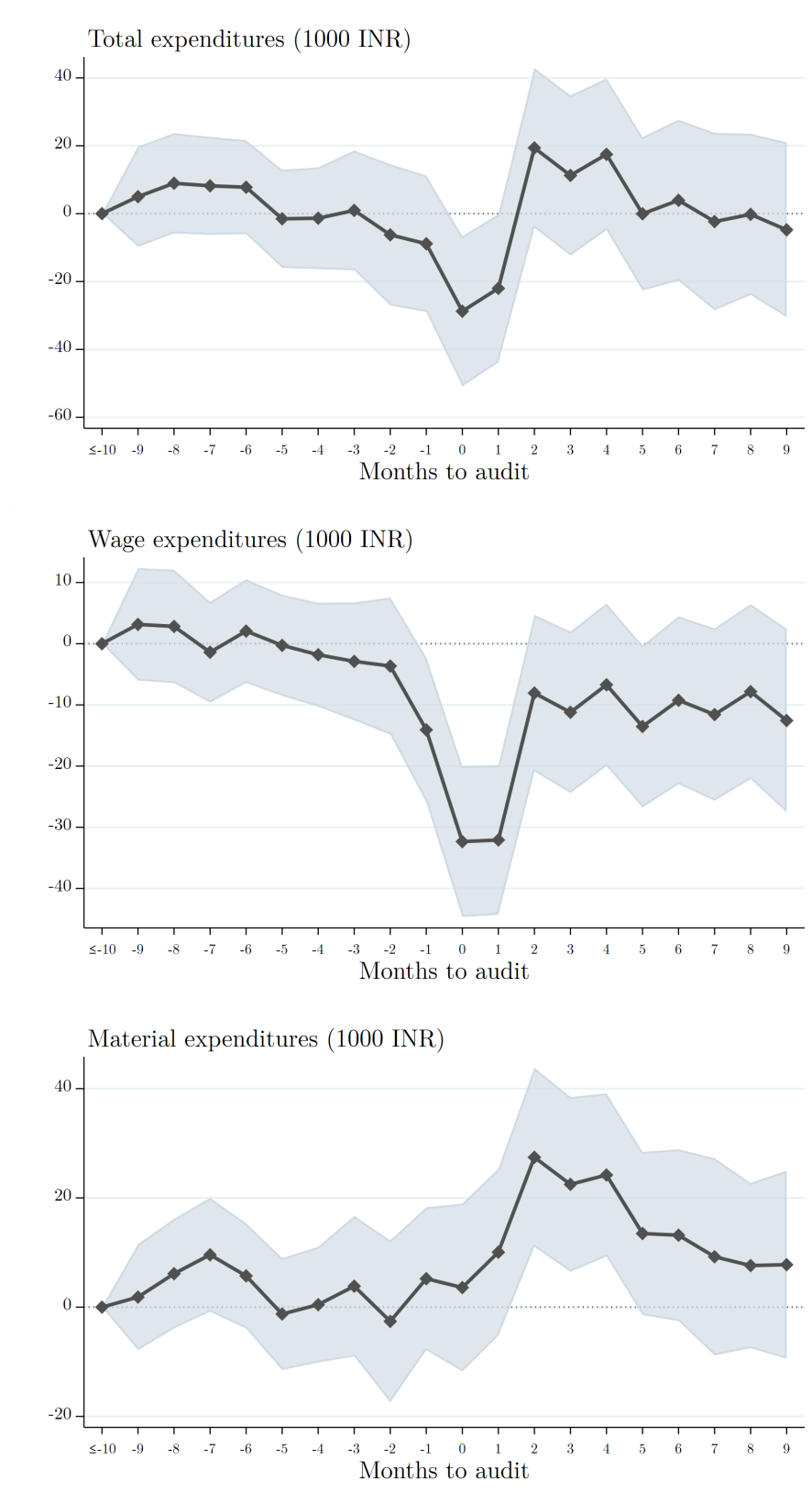


Figure 4: Changes in expenditures around the time of audit. The omitted category is 10 or more months before the audit. The raw mean of the omitted category is 272, 187, and 85 (1,000 INR) for total, wage, and material expenditures, respectively. The regressions include GP and district month-year fixed effects. 95% confidence intervals are captured by the shaded region. Standard errors are clustered by block.

like making timely payments.

Second, bureaucrats' responses in the months around the audit are unlikely to have impacted program output, providing further evidence that multi-tasking is not a concern. If we were worried that the decline in labor input during the month of the audit was a decline in real employment and thus would lead to a decline in real output, then we would also expect a corresponding decline in material input.⁴⁰ During the period prior to the first wave of audits, a 1% increase in material expenditures is associated with a 0.06% increase in person-days of work provided (t -stat = 8.12). If material and labor inputs are positively correlated, then real output will tend to depend on both inputs.

Figure 4 shows no decline in materials procured around the time of audit corresponding to the observed decline in employment. The findings are consistent when examining employment by whether the project worked on required materials or only required labor (see Appendix Section D.3). If procurement of materials were not in sync with employment, we would expect a corresponding decline in materials procured some months before or after the decline in employment (if this were a decline in real employment). However, a increase in procured materials is the only observed response occurring the month after the decline in employment. This suggests that real output did not decline during the month of audit and multi-tasking while auditors were present was not an issue.

These results are also unlikely to be a result of second order effects from the audit, e.g. bureaucrats are transferred or fired as a punishment, and the GP's workflow disrupted. Punishments from audits are enforced in higher-level hearings and these hearings can happen within the same month or months after the audit. If we disaggregate the event study by whether the higher-level hearing occurred within the month or months after the audit, we observe no differences in trends between these groups.

Taken together, these results show that the changes in expenditures in the months

40. The different projects carried out under NREGS have guidelines on the ratio of material to labor expenditures. This ratio is used as a performance indicator for bureaucrats to ensure that manual labor is used to execute projects, which is the intention of the program. It is not uncommon for bureaucrats to fake the works funded on the payroll by paying wages for fabricated work and having machines complete the work.

around the audit left program output unaffected and can be interpreted as deterrence (or changes in misappropriated expenditures).⁴¹ Bureaucrat adjustments around the time of audit were short-lived at best; lost rents were recovered later as bureaucrats substitute their behavior across time and type of expenditure (from wage to material misappropriation). These adjustments are consistent with the observed behavior of bureaucrats anticipating their first audit (Section 5.2).

6 Information Design and Counterfactuals

Section 3 tells us that the shape of bureaucrats' deterrence given their expectations of being audited is a sufficient statistic for determining the optimal design of information and evaluating the Principal's welfare under alternative signals. The changing expectations of bureaucrats generated by the random assignment to audit without replacement allows us to estimate this relationship with results on total expenditures from Table 1 Panel B, under certain assumptions (described in Section 6.1). We can do so without needing to specify underlying primitives of the choice problem, like parameters for preferences and constraints.

The estimated sufficient statistic is largely convex and shows that, for this particular setting, policies which communicate full information about audit risk will lead to more deterrence than policies which maintain uncertainty (Section 6.2). Appendix E.2 provides robustness checks and analyzes the sensitivity of the conclusion by relaxing assumptions. Section 6.3 uses the sufficient statistic to estimate welfare consequences to the Principal under alternative signals.

41. These results suggest that false payments equivalent to 18% of the level of baseline employment were averted during the month of audit. This decline is consistent in magnitude with what Imbert and Papp (2011) measure in Khera (2011). They estimate overreported employment in person-days spent on public works using household survey data to be between 20-29% of the reported person-days of NREGS employment across the states in their sample.

6.1 Assumptions for estimating the sufficient statistic

Two main assumptions are required for estimating the sufficient statistic. Assumption 1 rules out that dynamics of changing expectations or other environmental factors over time are driving the bureaucrat's response. It carries implications for: using the estimates of anticipatory behavior to estimate the sufficient statistic; and interpreting the estimated sufficient statistic.

Assumption 1. *The deterrence response of bureaucrats depends only on their current expectations of being audited.*

First, Assumption 1 implies that the estimated differences in anticipatory behavior (results from Table 1 Panel B) are only explained by differences in bureaucrats' expectations of being audited. This assumes the results are not confounded with other changes to the environment over time which may also affect deterrence, like bureaucrats' changing perceptions of audit quality or credibility.⁴² This implication has some testable predictions. Appendix E.1 shows that there are no significant differences across years in audit quality, measured as difference in audit inputs. There are also no significant differences across waves in their responses when they experience the same stages of anticipation and the audit. Furthermore, Appendix D.1 tests for spillover effects. It shows that the anticipatory and direct effects of the audit on total expenditures are unaffected after accounting for concentration of audits within one's local peer network. This provides additional evidence that the anticipatory effects are driven by changes in expectations of being audited rather than perceptions about the audit (to the extent perceptions are informed by peer experiences). For robustness, this assumption is later relaxed in a sensitivity analysis (Appendix E.2).

Second, Assumption 1 implies that the only payoff-relevant parameter in the bureaucrat's decision is their expectation of being audited for this year's work. This means

42. We can think of the perceived cost of corruption from the audit is a product of the likelihood of incurring a penalty and the penalty itself: $\text{Cost to corruption via audit} = Pr(\text{Penalty incurred}) \times \text{Penalty}$. The probability of incurring a penalty is a function of expectations of whether one will be audited and expectations of whether an audit is credible: $Pr(\text{Penalty incurred}) = f(\text{whether Audited, whether audit Credible}, \epsilon)$

that the bureaucrat's response depends only on their current beliefs and not the policy that generated those beliefs. That is, conditional on their expectations of being audited, the bureaucrat's response is policy-invariant. For example, if a Bureaucrat's beliefs are that they may be audited with probability $\frac{1}{2}$, then their best response would be invariant to whether the underlying audit policy inducing those beliefs was one where units were selected for audit randomly with or without replacement. This also means that the bureaucrat's response only depends on their current beliefs and not the history of actions or beliefs leading up to the current period. That is, the Bureaucrat's best response has a Markov property and dynamics of the Bureaucrat's response are completely explained by their current beliefs.

Furthermore, the random assignment of expectations of being audited in the empirical setting parallels the model: the signal structure chosen by the Principal randomly assigns signals to each receiver, so the differences in their responses can only be explained by differences in posterior beliefs (holding all else equal).

Assumption 2. *Bureaucrats beliefs during the horizons of anticipation are defined by their beliefs about next year's audit capacity.*

Bureaucrats expectations that this year's work gets audited can be thought of as the number of audits to be conducted in the next year (because last fiscal year's work is subject to evaluation) as a share of the remaining GPs to be audited (because of the randomization without replacement). To be agnostic, Table 3 makes a flexible range of assumptions on bureaucrats' beliefs about next year's audit capacity. Drawing on the information received from the announcements, we can pin down bureaucrats' expectations under each assumption.

Let $K_{\tau+1}$ denote bureaucrats beliefs about next year's audit capacity where time τ is the current year. Assume that $K_{\tau+1}$ could be equal to: (i) $K_{\tau-1}$, last year's audit capacity; (ii) $\frac{1}{2}(K_{\tau-1} + K_{\tau})$, the average of last year's and this year's audit capacity; (iii) K_{τ} , this year's audit capacity; (iv) $\text{Trend}_{\tau} \times K_{\tau}$, this year's audit capacity multiplied by recent growth in capacity; and (v) $K_{\tau+1}$, beliefs about next year's audit capacity are ex-post

consistent.

	Probability this year's work will be audited, q				
	<i>Assumptions on next year's audit capacity, $K_{\tau+1} =$</i>				
	$K_{\tau-1}$	$\frac{1}{2}(K_{\tau-1} + K_{\tau})$	K_{τ}	$Trend_{\tau} \times K_{\tau}$	$K_{\tau+1}$
<i>AnticipatingAudit</i> - Year 1 (Low)	0.00	0.07	0.14	0.14	0.39
<i>AnticipatingAudit</i> - Year 2 (Medium)	0.24	0.44	0.65	1.00	0.92
Month of <i>Audit</i> (One)	1.00	1.00	1.00	1.00	1.00
<i>AnticipatingNextAudit</i> - Year 2 (Zero)	0.00	0.00	0.00	0.00	0.04
<i>AnticipatingNextAudit</i> - Year 3 (High)	0.67	0.82	0.96	1.00	1.00

Table 3: Assumptions to determine bureaucrats' beliefs over likelihood this year's work will be audited.

Probabilities for *AnticipatingAudit* - Year 1, *AnticipatingAudit* - Year 2, *AnticipatingNextAudit* - Year 3 are calculated as follows: The denominator is the remaining number of GPs yet to be audited after learning who is selected for audit that year. The numerator is based on the assumption of future audit capacity ($K_{\tau+1}$) using information on past audit capacity. E.g. when $K_{\tau+1} = K_{\tau}$, expectations for *AnticipatingAudit* - Year 1 are $\frac{548}{3807} = 0.14$, where 548 is the observed number of audits conducted in Year 1 and 3807 is the remaining number of GPs to be audited after Year 1 audits.⁴³ $Trend_{\tau} \times K_{\tau}$ assumes next year's capacity is based on the growth rate in audit capacity from last year to this year.

Beliefs do not vary for 'Month of *Audit*' because concurrent performance is susceptible to detection by auditors. So, while an audit is happening, I assume bureaucrats believe today's performance will be audited with probability 1. Beliefs largely do not vary for '*AnticipatingNextAudit* - Year 2' because Wave 1 GPs in the year following their audits (Year 2) believe they will not be audited a second time until the round of first audits has

43. The special audit in FY2017-18 audited an additional 175 GPs for the first time. Likewise, 21 GPs from Wave 1 were selected for their second audit in Year 3, reportedly by accident. Under assumption $K_{\tau+1} = K_{\tau+1}$, Wave 1 would have anticipated this, hence their beliefs of 0.04. The special and duplicate audit GPs are accounted for when constructing expectations the denominator, but not included in the regressions as described in sample restrictions in Section 2.

completed.

6.2 The optimal design of information

The sufficient statistic is simply a graph that maps the various assumptions on beliefs from Table 3 (“x” values) to their associated estimates of deterrence in total expenditures from Table 1 Panel B (“y” values). Based on the model in Section 3, we can interpret this graph as the Principal’s expected utility as a function of the Bureaucrats’ beliefs about being audited. Greater declines in expenditure means more deterrence and higher expected utility for the Principal. Figure 5 plots the sufficient statistic under each beliefs assumption.

To determine the optimal policy, we need to ask whether a convex combination between any two points on the curve could achieve a higher expected utility for the Principal. We evaluate this at the audit agency’s capacity, q_0 , which ranged from 14-53% during the study period. That is, rather than leaving bureaucrats with their prior expectations of being audited (better when concavities present), could the Principal do better by informing those selected for audit they are high risk and those not selected that they are low risk (better when convexities present)?

If we consider the average deterrence response (black curves in Figure 5), then under all belief assumptions the curves are convex at q_0 (the observed range in capacity to conduct audits each year, 14-53%).⁴⁴ In other words, the Principal’s optimal policy is to provide information on whether one is at high risk of being audited or not. With a constrained budget, the optimal signal communicates full information about audit risk when $K_{\tau+1} = \{K_{\tau}, Trend_{\tau}K_{\tau}, K_{\tau+1}\}$, and communicates to those selected for audit that they are high risk when $K_{\tau+1} = \{K_{\tau-1}, \frac{1}{2}(K_{\tau-1} + K_{\tau})\}$. Sensitivity analyses in Appendix E.2 show this result, that the sufficient statistic is convex at q_0 , is robust to statistical noise and relaxing Assumptions 1 and 2.

44. The average elasticity under each assumption ranges from 0.07-0.14, where the elasticity for the increasing part of the graphs with more than 3 points of support range from 0.64-5.7.

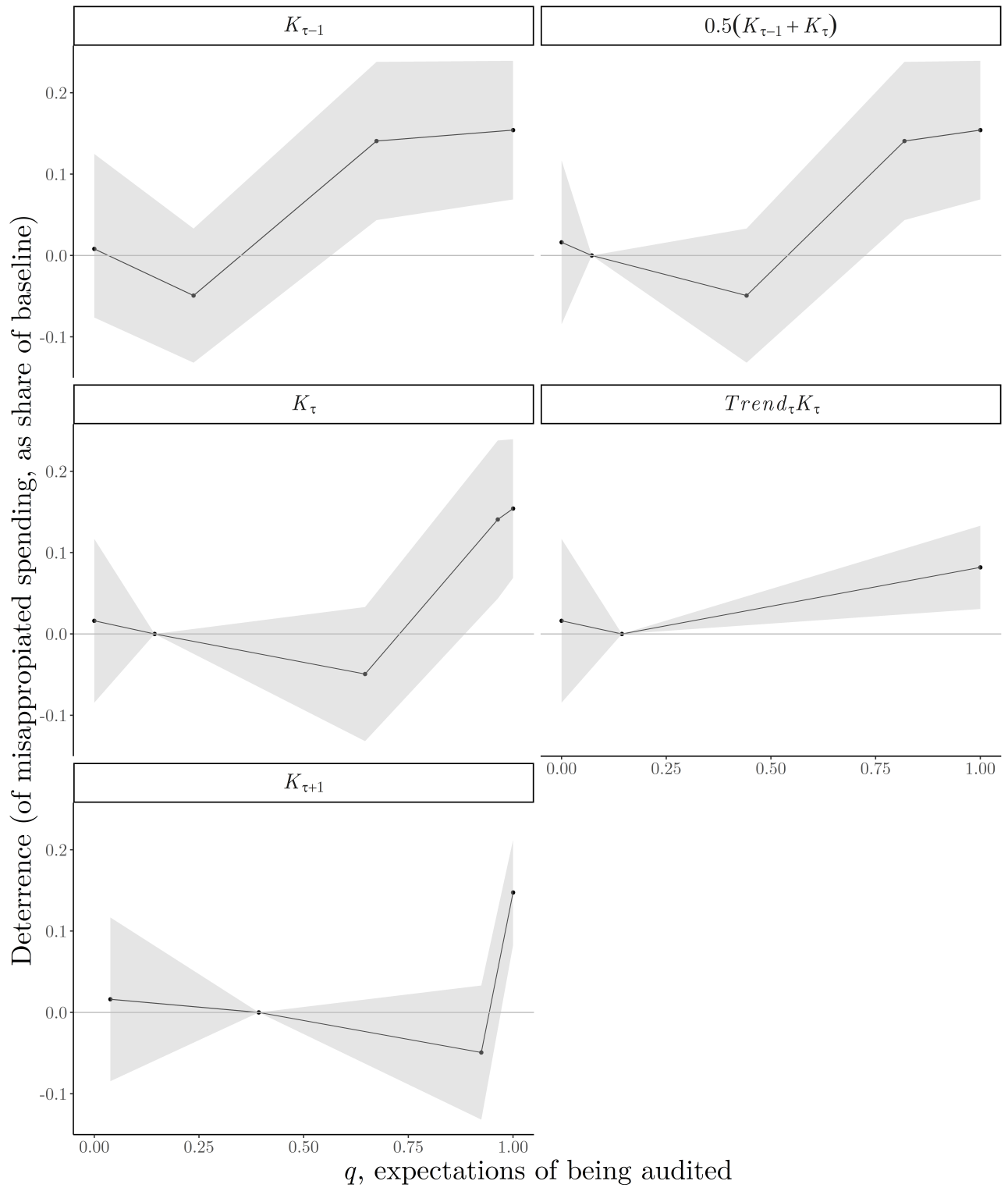


Figure 5: Principal's expected utility as a function of Bureaucrats' beliefs, under different beliefs assumptions. Black dotted line plots the mean of regression estimates with grey shaded area representing the 95% confidence interval for each parameter estimate. The deterrence estimates are relative to the reference group, 'AnticipatingAudit - Year 1', and reported as a share of the baseline mean. Under assumptions $K_{\tau+1} = \{K_{\tau-1}, Trend_{\tau}K_{\tau}, K_{\tau+1}\}$, some anticipatory groups are assumed to have the same beliefs. So, the mean of those groups' deterrence estimates and associated standard errors are plotted.

We have at maximum five points of support to interpolate the shape of the sufficient statistic using a piece-wise linear function. A limitation of this approach is that this interpolation may smooth out any local concavities that make an uninformative signal optimal. If we are correct about our theoretical assumption that bureaucrat deterrence is monotone in expectations of being audited, then this is of limited concern.

6.3 Counterfactual signals and welfare consequences

This section estimates the welfare consequences to the Principal under an alternative signal or design of information. Welfare under each policy is calculated over the course of the 27 months that the round of first audits took place. Welfare is estimated using the joint sampling distribution of the deterrence estimates produced with a block bootstrap,⁴⁵ and estimating the sufficient statistic for each bootstrapped sample. This approach allows us to report uncertainty around the welfare estimates. Appendix E.3 provides further details.

Assuming $K_{\tau+1} = K_{\tau}$, the results show that spending under the full information signal during this period would have been USD 226 million (s.e. = 12). Compared to the some information signal (modeled as the actual policy implemented randomizing without replacement), the full information signal would have deterred 10% more in misappropriated expenditures on average. Compared to the uninformative signal (randomization with replacement), the full information signal would have deterred 16% more in misappropriated expenditures. This corresponds to a decline in misappropriated expenditures of about USD 22 million (p -value<0.001) when comparing the full information to some information signal; and USD 37 million (p -value<0.001) when comparing the full information to uninformative signal—all without changing the Principal’s budget for audits.

If we worried about using the estimate of deterrence during the month of audit in the sufficient statistic and excluded it from the analysis, these conservative estimates show that the full information signal (total expenditures of USD 228, s.e. = 12.3) would on

45. This is the same block bootstrap implemented for the sensitivity analysis (Appendix E.2). The marginal sampling distribution of each estimated coefficient converges after bootstrapping about 10,000 draws. I bootstrap 100,000 samples to report results with 0.001% confidence.

average have deterred 8.7% and 15.4% more in misappropriated expenditures compared to the some information and uninformative signal, respectively. This is equivalent to a corresponding decline in misappropriated expenditures of USD 19.8 million and USD 35 million (p -values <0.001). The results under other beliefs assumptions are similar in magnitude (see Appendix Table E.4). Except, when $K_{\tau+1} = Trend_{\tau}K_{\tau}$, the differences in misappropriated spending are small, but still least under the full information signal.

These potential gains are substantial, especially given wide-prevailing audit standards that it is best to not inform clients of the auditing strategy to maintain unpredictability. The exercise conducted in this empirical setting makes a strong case for evaluating the possibility that atypical audit strategies, like implementing and informing of an audit in advance, may yield significant returns at no additional cost.

7 Conclusion

The monitor's resource constraints imply that a subset of bureaucrat activity will go unchecked, potentially allowing bureaucrats to adapt. Monitoring policies designed to maximize deterrence must account for bureaucrat attempts to evade detection. This paper provides empirical evidence on strategic responses by bureaucrats to monitoring. Taking into account these strategic responses, this paper also shows how information disseminated about the likelihood of audit can be optimally designed with a budget-constrained policymaker in mind.

Results show that the deterrence of misappropriated expenditures is strongest when one is almost certain of an audit, while the response under less certainty is statistically indistinguishable from zero. In addition, the unintended consequences of monitoring policies matter. When expectations of being audited increase, bureaucrats substitute across time and type of expenditure to misappropriate.

Interpreting these findings with a model of Bayesian persuasion, I find that designing monitoring policies which inform bureaucrats in advance would yield the most deterrence.

Signals which provide more information are better. This implies randomization without replacement is better than with replacement. I arrive at this result by estimating a sufficient statistic from the model that allows us to solve for the optimal signal and analyze welfare under counterfactuals. I estimate the sufficient statistic using causally-identified moments and a set of flexible assumptions on bureaucrats' beliefs.

This paper provides a novel empirical measure of the value of information. In this setting, up to USD 35m (16% of average annual program expenditures) could have been saved. This case study contradicts auditing standards that advocate maintaining unpredictability among audit subjects. The findings of this paper emphasize that the best practice depends on the relationship between deterrence and bureaucrats' expectations of being audited (or another policy parameter of interest in a different setting).

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Appendix (For Online Publication)

A Background

A.1 Details on implementation of NREGS in Jharkhand at the GP

At the gram panchayat (GP), there are several bureaucrats responsible for operating NREGS. The president of a GP (*mukhiya*) is an elected official. The president facilitates the process for selecting projects to fund and oversees the allocation of job cards. The secretary of the GP (*panchayat sachiv*) provides job cards, manages employment allocation and wage payments, and uploads administrative data to the NREGS database. Outside of their NREGS responsibilities, the president and secretary also manage other programs and matters at the GP office. The NREGS employment assistant (*gram rozgar sewak*) provides project work to workers, pays wages, and manages NREGS projects. Engineers at a higher administrative level ensure the quality of public projects across the GPs they oversee. The state government appoints the secretary, NREGS employment assistant, and engineer. Finally, the GP hires direct supervisors, often participants, of project sites. These supervisors take attendance for the payroll and help participants apply for work.⁴⁶

A.2 Details on the audit agency

The Social Audit Unit was founded and funded in May 2016. Audits were piloted in 49 GPs in June 2016; then the first wave of audits took place from December 2016 until March 2017 (the end of the fiscal year). The national act describes audits as an important component of NREGS. But, since the program began in 2006, limited resources kept state governments like Jharkhand from implementing a credible audit program. Before the creation of the audit agency, audits were conducted by civil society organizations on an ad-hoc basis or conducted by the bureaucrats who themselves were the object of audit interest. It was not until the creation of this audit agency that a credible and systematic audit process was in place for public welfare programs, like NREGS, in the state of

46. See the MGNREGA Operational Guidelines 2013 for details on roles and responsibilities.

Jharkhand.

The audit agency is funded independently of NREGS and managed by a steering committee of various stakeholders across the state government and civil society. There are around 7 stakeholders on the steering committee. The steering committee is largely removed from NREGS implementation. One of the members is the state commissioner responsible for implementing NREGS in Jharkhand; however, there are no reasons to suspect that his participation in the steering committee would compromise quality of audits. As the highest manager accountable for the state's performance in NREGS, he has an incentive to root out corruption with monitoring.

Due to hiring practices and quality assurance mechanisms, it is likely that the audits were conducted at-scale by the audit agency with credibility and integrity. First, audit managers and auditors are hired at competitive salaries where compensation for the lowest-ranked auditor is at least 2 times the minimum wage (from 550 INR per day to a salary of 35,000 INR per month for district-level managers). These salaries are comparable to what research agencies in India pay their surveyors and field research managers. These rates are also competitive compared to auditor rates in other government agencies. While we cannot assume there were no auditors corrupted by bureaucrats, we know that the potential loss of the job from being fired is not insignificant. There is the obvious loss of salary, but also potential hardship in finding another comparable job. This is especially salient for positions requiring higher education levels because those with a high school education or higher made up about 60% of the unemployed working age population in 2018 in India (Centre for Sustainable Employment 2019).

Furthermore, the audit agency has multiple mechanisms to ensure audit quality. First, they audit at least 5% of the audits they conduct for quality assurance.⁴⁷ Second, auditors cannot be assigned to audit their home region to prevent potential conflicts of interest. Third, strict guidelines are in place for seeking accommodations and provisions during

47. Five-percent is based on set intentions, but there is no data available at this point of the back-checked audits. GPs selected for back-checked audits are determined by field reports of collusion and a random sampling.

the week of their stay to audit the GP. In particular, they do not rely on local bureaucrats to facilitate the logistical aspects of their stay. The auditors setup a home-base during the period of audit at the GP government office, organize their own transportation, and even rent cookware to cook their own food. When they are not conducting audit verification fieldwork, they use the local government office to work, eat, and sleep. This is an intentional feature of the audit process emphasized by the audit agency. Some of the GPs for audit are in very remote areas where it may be hard to find options for lodging and meals. This helps minimize any leverage a local bureaucrat may have by offering their resources and currying favor with auditors. Just as importantly, these guidelines are put in place for fear of tarnishing the integrity of the audit especially as it may be perceived by local participants, whose incentives to report during the audit can be affected.

A.3 Details on the roll-out schedule and public notices about the audits

Table A.1 summarizes details of the audit schedule for each year of the roll-out of audits.

Table A.1: Audit Schedule, 2016-2019

Fiscal year	Announcement date	# Audits	Duration of audit calendar	
2016-17	29-Dec-16	548	17-Dec-16 –	29-Mar-17
2017-18	2-May-17	1,495	9-May-17 –	21-Mar-18
2018-19	23-Mar-18	2,137	13-Apr-18 –	14-Mar-19

The following describe the formal notices that were publicly disseminated and what could be learned by all bureaucrats:

- Audit agency created, May 2, 2016 - Notice on creation of the audit agency and personnel to be hired to staff the agency.
- Year 1 announcement, December 29, 2016 - Commencement of 548 audits for Wave 1. Official notice was disseminated on guidelines for conducting the audits and announcement of GPs in Wave 1 of audits to be conducted in the remainder of Year 1. The notice

also states that the goal of the audit agency is to eventually be able to audit 50% of GPs every year. This is against the benchmark stated in the 2006 NREGS Act Section 17 that requires all GPs to be regularly audited twice a year.⁴⁸

The notice does not mention that GPs are being randomly selected. Furthermore, given the notice and 2006 NREGS Act, it is reasonable to expect that audits will be rolled out to all GPs before one can expect to be audited again. Roll-out without replacement is discussed in steering committee meeting minutes which are made public. It is *not* known that the roll-out would take 3 years to complete. In fact, from meeting minutes of the steering committee, there is considerable uncertainty even among the committee about future audit capacity driven by annual budget approval processes contingent on current performance and the audit agency's future capacity to recruit and train a workforce of auditors.

- Year 2 announcement, May 2, 2017 - Commencement of 1,495 audits for Wave 2.
- Year 3 announcement, March 23, 2018 - Commencement of 2,137 audits for Wave 3.

Remaining GPs that have not been audited are being completed in Year 3. It is highly likely that those not selected for Wave 3 audits will be selected for audit in Year 4. About 80% of GPs audited in Year 4 were not audited in Year 3.

Every announcement across the three years states that part of the audit will involve a verification of administrative reports on employment and public projects from the previous FY.

A.4 Survey of bureaucrats' knowledge on audit policy

The rotation of audits across GPs is implied by the information from the Year 1 announcement and 2006 NREGS Act that all GPs be audited regularly. But, it may not have been

48. The commencement of Wave 1 audits started with this notice, along with a press conference and video-conference with all district officials to discuss and disseminate the notice.

explicitly recognized by GP bureaucrats. This could pose challenges to inferences we make on bureaucrats' expectations of being audited across the horizons of anticipation.

From September-November 2020, a remote survey was conducted by phone interview and SurveyMonkey to understand bureaucrats' knowledge of the audit policy from 2016-19. Piloting of the survey was conducted from February-March 2020, and continued rollout of the survey was halted from April-August 2020 during restrictions to address the surge in COVID-19 in India.

We randomly sampled 83 GPs, stratified by wave and whether the share of scheduled caste or scheduled tribe population was above average. In each GP, we attempted to reach multiple bureaucrats and most often received responses from the head bureaucrat (*mukhiya*) and the NREGS-dedicated bureaucrat (*panchayat rozgar sewak*). Response rates were low during the pandemic. Among the sampled GPs, we received 40 responses across 27 GPs (33% of sampled GPs).

In a multiple choice question, bureaucrats were asked how GPs were selected for audit of NREGS each year during 2016-2020. They could respond: GPs were selected randomly (regardless of whether they were recently audited); GPs were selected at the discretion of state government officials (regardless of whether they were recently audited); GPs took turns being audited (E.g. All GPs received their 1st audit before anyone was selected for their 2nd audit); or 'I'm not sure'. We consider the responses of the most senior bureaucrats in each GP (the *mukhiya* or NREGS-dedicated bureaucrat, if the *mukhiya* was not available). Other lower-ranked officials may not be aware of state-directed policies and are less likely to be directing the GP implementation of the NREGS program. Among them, 41% responded 'I'm not sure',⁴⁹ 30% responded that GPs took turns being audited, 22% responded that GPs were selected at the discretion of state government officials, and 7% responded that GPs were selected randomly. Among respondents who had beliefs about how GPs were selected for audit, the modal belief was that GPs took turns being

49. Respondents who were interviewed by phone were much more likely to report 'I'm not sure', which could be indicative systematic differences in responding when speaking with an enumerator over the phone. For instance, differences in the way questions may have been prompted like emphasis on certain parts of the question or perceptions that the enumerator was working on behalf of state government officials.

audited. Those responding that GPs were selected at the discretion of state government officials were not wrong, as special audits occurred for some GPs (see discussion in Section 4.2). The least common response was that GPs were selected for audit randomly, regardless of whether they were recently audited.

Although a small sample of respondents, these survey findings along with the differences in anticipating an audit prior to Waves 1 and 2 (see results from Appendix Section C.2) provide evidence supporting that GP bureaucrats on average anticipated audits consistent with the turn-taking implemented via the randomization without replacement.

B Model of Information Design and Deterrence

B.1 Using the static model to evaluate dynamic settings

Using features of the empirical setting and making reasonable assumptions, we can use the single-period model to evaluate dynamic settings and treat each bilateral sender-receiver game independently. I discuss these assumptions here. Appendix B.2 sets up the dynamic setting that can be evaluated with the static model.

First, in the empirical setting, the announcement is public and all bureaucrats receive the same information. So, the Principal sends a public signal which allows us to treat each bilateral sender-receiver game independently (Kamenica 2019). In this case, the Principal considers the vector of Bureaucrats' best responses when determining the optimal signal

Furthermore, when analyzing the model, we will examine the game between the Principal and an arbitrary Bureaucrat. In particular, we will not enumerate model specifications that capture heterogeneity among Bureaucrats, like ability or propensity to be corrupt; the only parameter that matters is the Bureaucrat's posterior beliefs. In other words, the difference in Bureaucrat response can only be explained by differences in posterior beliefs (holding all else equal) because the signal structure chosen by the Principal randomly assigns signals to each receiver. This maps well to our empirical setting where

assignment to audit and anticipatory beliefs were also randomly assigned. Section 6.1 provides further discussion of mapping the model to the empirical context.

Second, the model assumes that the state space (whether audited or not) and action space (expenditures misappropriated by Bureaucrat) are invariant across time, which allows us to consider a static model (Kamenica 2019). It is reasonable to assume a time-invariant action space because the scope of the Bureacrats' authority and responsibilities are unlikely to change over time. In the context of a monitoring policy, we can also expect the state space to remain unchanged as the relevant output of a monitoring policy is to monitor or not.

B.2 Setup: Dynamic model with multiple receivers

There are N receivers (or Bureaucrats) responsible for the implementation of the workfare program over the course of some finite length of time T , where the set of time periods is $\mathcal{T} = \{1, \dots, T\}$ and indexed by t . The set of receivers is $\mathcal{I} = \{1, \dots, N\}$, indexed by i , and $|\mathcal{I}| = N$. Every period each receiver oversees some amount of expenditures to be allocated denoted by x_{it} and has the technology to extract private rents from x_{it} .

The sender (or Principal) always wants as much of $X_t = \sum_i x_{it}$, the total program expenditures, to go towards realizing the goals of the workfare program as possible. The Principal uses monitoring as a policy to discipline Bureaucrats' behavior where significant penalties are imposed as punishment for being caught extracting private rents. However, the Principal is budget constrained and can only conduct $M < N$ audits every period.

There are two states of the world for Bureaucrat i in period t : x_{it} will be audited (1) or not (0), where $\omega_{it} \in \{0, 1\}$ denotes an element of the state space for i in t . The state space of the game is $\Omega = \{0, 1\}^{NT}$, where we can think of an element of the state space, $\omega \in \Omega$, as $\omega = \{\omega_{1t}, \dots, \omega_{Nt}\}_{\forall t \in \mathcal{T}}$.

Every period, the Principal decides and commits to a signal structure or policy, $\pi : \Omega \rightarrow \Delta(S)$ where $S = \Delta(\Omega)$ is the set of signal realizations over the state space. Each Bureaucrat takes an action $a_{it} \in A = [0, 1]$, which is the share of x_{it} to misappropriate for

private gains, conditional on posterior beliefs induced by their signals.

B.3 Proofs

Proposition 1 (Persuasion Works). *The Principal benefits from sending an informative signal if and only if $U(q_0) < \hat{U}(q_0)$.*

Proof. Let's start with the first statement of the proposition. Suppose that the Principal benefits from sending information, then there exists a signal structure π that induces a distribution of posteriors which yields payoff $\mathbb{E}_{q \sim \pi(\cdot)}[U(q)] = \hat{U}(q_0)$. Since an informative signal is better for the Principal than an uninformative one, with payoff $U(q_0)$, we can construct π such that $\hat{U}(q_0) > U(q_0)$.

Now, suppose that $U(q_0) < \hat{U}(q_0)$, then there is a distribution of posteriors induced by a signal π that achieves payoff $\hat{U}(q_0)$ different from an uninformative signal which achieves $U(q_0)$. Since $U(q_0) < \hat{U}(q_0)$, the Principal prefers to send an informative signal. \square

Proposition 2. *If $U(q)$ is convex at q_0 , then $U(q_0) < \hat{U}(q_0)$ and an informative signal is preferred. This result implies the following about the optimal signal given the shape of $U(q)$:*

- (i) *The Principal prefers an informative over uninformative signal for any q_0 if $U(q)$ is convex for all q . The Principal prefers an uninformative signal for any q_0 when $U(q)$ is concave for all q .*
- (ii) *An informative signal is still preferred if $U(q)$ is convex at q_0 and concave elsewhere for some q .*

Proof. Let's start with the first statement of the proposition. Suppose that $U(q)$ is convex when evaluated at q_0 . Let $q_0 = \lambda q_1 + (1 - \lambda)q_2$ where $\lambda \in (0, 1)$ and q_1, q_2 are in the domain of $U(\cdot)$ where $U(\cdot)$ is convex around q_0 . By Jensen's inequality, $U(q_0) = U(\lambda q_1 + (1 - \lambda)q_2) < \lambda U(q_1) + (1 - \lambda)U(q_2)$. Let $\tilde{q} = \lambda U(q_1) + (1 - \lambda)U(q_2)$ where

$\tilde{q} \in C$ by definition of the set C . \tilde{q} is an achievable payoff for the Principal. We've established that $\tilde{q} > U(q_0)$ and we know that $\hat{U}(q_0) = \max\{C\}$. So, $\hat{U}(q_0) \geq \tilde{q} > U(q_0)$. We've established that whenever $U(q_0)$ is convex at q_0 , we have that $\hat{U}(q_0) > U(q_0)$. This also shows that statement (ii) holds. However, it is not always true that when $U(q)$ is concave at $U(q_0) > \hat{U}(q_0)$ and an uninformative signal is preferred. A counterexample to consider is if there is a local concavity at q_0 , but convexity in the function $U(q)$ elsewhere.

Let's turn to (i) of the proposition. Apply Jensen's inequality as in the above proof. Let $q_0 = \lambda q_1 + (1 - \lambda)q_2$ where $\lambda \in (0, 1)$ and q_1, q_2 are in the domain of $U(\cdot)$. By Jensen's inequality, $U(q_0) = U(\lambda q_1 + (1 - \lambda)q_2) < \lambda U(q_1) + (1 - \lambda)U(q_2)$. Since $\lambda U(q_1) + (1 - \lambda)U(q_2)$ is in the set C it is an achievable payoff for the Principal. Now, pick q_1, q_2 such that $\lambda U(q_1) + (1 - \lambda)U(q_2) = \hat{U}(q_0) > U(q_0)$. So the Principal prefers an informative signal when $U(q)$ is convex for all q by sending signals that induce posteriors q_1 and q_2 , rather than leaving everyone with the prior q_0 where everyone has the same likelihood of audit (uninformative signal). If $U(q)$ is concave for all q , then Jensen's inequality tells us that $U(q_0) = U(\lambda q_1 + (1 - \lambda)q_2) > \lambda U(q_1) + (1 - \lambda)U(q_2)$. By Proposition 1, the Principal prefers an uninformative signal and will send an uninformative signal where posterior beliefs equal the prior belief q_0 . \square

B.4 Motivating example in detail

The Principal is deciding between randomly selecting N Bureaucrats for audit without replacement over two periods (equivalent to uninformative signal) or randomly selecting Bureaucrats for audit with replacement over two periods (an informative signal). In every period, the Principal only has the capacity to conduct $M < N$ audits or share $p = \frac{M}{N} \in (0, 1)$ of Bureaucrats. Suppose Bureaucrats have perfect information on the Principal's audit capacity (M) every period.

Let $U(q)$ be deterred financial misappropriation in a given period (reflecting Bureaucrats' optimal adjustments in behavior) as a function of Bureaucrats' expectations of being audited (q). The more likely they are to be audited (when q is higher), the more

Bureaucrats adjust. Consider $U(q)$ to be the Principal's value function as a function of Bureaucrats' best response. Assume for simplicity that the Principal is patient. Under randomization without replacement, the Principal's expected utility given Bureaucrats' beliefs and best response over the two periods is:

$$U(p) + \underbrace{pU(0)}_{(a)} + \underbrace{(1-p)U\left(\frac{p}{1-p}\right)}_{(b)}$$

where the first term reflects Bureaucrat behavior in the first period and the last two terms are based on Bureaucrat behavior in the last period. The share p audited in the first period will not be audited in the second period, they know this, and behave accordingly (term (a)). While those waiting to be audited (share $1-p$) believe they will be audited with probability $\frac{p}{1-p}$ and adjust their behavior accordingly (term (b)). On the other hand, if randomizing with replacement, the Principal's expected utility is:

$$2U(p)$$

This gives us:

$$U(p) \leq \underbrace{pU(0) + (1-p)U\left(\frac{p}{1-p}\right)}_{=\tilde{U}}$$

Whether the Principal prefers to randomize with or without replacement depends on this inequality.

Corollary 1. *When deciding between randomizing with (informative signal) or without replacement (uninformative signal), the Principal prefers randomization with replacement if $U(q)$ is concave for all q . Conversely, the Principal prefers randomization without replacement if $U(q)$ is convex for all q .*

Proof. If $U(q)$ is concave for all q , then

$$U(p) > pU(0) + (1-p)U\left(\frac{p}{1-p}\right)$$

by definition of a concave function and randomization with replacement is preferred. If $U(q)$ convex for all q , then the reverse is true. \square

B.5 Sufficiency of $U(q)$ for analyzing welfare changes and determining the optimal signal

Let U_1, \dots, U_n be a sample from the probability distribution of the Principal's welfare (as measured by levels of bureaucrat financial misappropriation), $f(u|\theta)$, where θ is a vector of parameters that determine the bureaucrat's decision to misappropriate finances. The sample $\left((U_1, q_1), \dots, (U_n, q_n)\right)$ from the joint distribution $f(u, q)$ where q is bureaucrat expectations of being audited and is randomly assigned. The distribution $f(u|q)$ is sufficient for θ . That is, given information on q , θ provides no additional information on the Principal's welfare and consequently, the optimal signal. This is because random assignment of q from the signal structure π holds all other pay-off relevant parameters in θ equal.

In practice, I estimate statistics for $f(u|q)$ for observed q . This will be used to estimate $U(q)$. With this and the assumption that $U(\cdot)$ is monotonic in q , we can assess changes in welfare as q changes and we can also construct the optimal signal.

C Empirical Strategy

C.1 Balance Across Waves

	Wave1	Waves2 – 1	p-value	Waves3 – 1	p-value	Waves2 – 3	p-value	Observations
<i>Panel A: 2011 Village Census</i>								
Number of households	1153.85	13.46	0.35	13.12	0.35	0.34	0.98	3,684
Total population	6072.11	52.01	0.50	64.54	0.38	-12.53	0.86	3,684
Scheduled castes population	760.63	-5.42	0.82	-3.45	0.87	-1.97	0.93	3,684
Scheduled tribes population	2035.97	88.00	0.13	-8.13	0.88	96.13	0.02**	3,684
Literate population	3026.02	52.80	0.20	50.83	0.23	1.97	0.96	3,684
Total working population	2662.77	-6.30	0.86	-2.41	0.94	-3.89	0.90	3,684
Main working population	1210.52	-10.43	0.73	12.65	0.62	-23.07	0.33	3,684
Main working population, cultivation	515.71	-14.67	0.35	-8.79	0.60	-5.88	0.62	3,684
Main working population, agriculture	297.51	0.06	1.00	6.74	0.58	-6.68	0.51	3,684
Main working population, household industries	49.73	-3.69	0.49	1.61	0.75	-5.30	0.23	3,684
Marginal working population	1452.25	4.13	0.89	-15.06	0.55	19.18	0.46	3,684
Marginal working population, cultivation	430.35	13.48	0.42	11.88	0.49	1.60	0.91	3,684
Marginal working population, agriculture	791.96	-7.07	0.73	-23.08	0.22	16.00	0.34	3,684
Marginal working population, household industries	48.38	-2.78	0.43	-1.53	0.65	-1.25	0.63	3,684
Total geographical area (sq. km.)	1914.76	-1.39	0.98	-28.25	0.58	26.87	0.53	3,694
Forest area (hectares)	508.05	-21.29	0.56	-29.52	0.37	8.23	0.76	3,694
Barren, uncultivable land area (hectares)	85.03	-1.17	0.84	-4.48	0.46	3.31	0.45	3,694
Permanent pastures/grazing land area (hectares)	30.66	2.62	0.41	1.78	0.46	0.85	0.73	3,694
Total unirrigated land area (hectares)	587.54	15.86	0.57	20.44	0.48	-4.59	0.87	3,694
Wells and tubewells area (hectares)	32.93	-3.25	0.15	5.58	0.36	-8.83	0.14	3,694
Tanks and lakes area (hectares)	33.64	6.79	0.09*	14.41	0.07*	-7.62	0.29	3,694
<i>p-value of F-test of joint orthogonality</i>			0.61		0.72		0.97	
<i>Panel B: MGNREGA MIS 2014-16, annual average</i>								
Number of households provided employment	1003.86	-23.93	0.41	-43.15	0.12	19.23	0.43	3,854
Labor expenditures (1000 INR) per household	2.09	-0.07	0.27	-0.09	0.21	0.02	0.54	3,852
Material expenditures (1000 INR) per household	0.91	0.00	0.93	-0.01	0.79	0.02	0.59	3,852
% Labor expenditures of Total	71.62	-1.08	0.04**	-0.28	0.56	-0.80	0.09*	3,854
Days of delayed payment per household	27.78	2.24	0.23	0.62	0.70	1.61	0.37	3,852
% SC	12.52	-0.15	0.79	0.65	0.20	-0.80	0.07*	3,845
% ST	35.64	0.25	0.79	-0.32	0.74	0.57	0.46	3,845
% Women	30.93	0.50	0.26	-0.06	0.90	0.56	0.15	3,845
% Work completed	64.96	-0.22	0.70	-0.40	0.48	0.18	0.68	3,845
<i>p-value of F-test of joint orthogonality</i>			0.36		0.15		0.21	
<i>p-value of F-test of joint orthogonality on all covariates</i>			0.57		0.84		0.93	
<i>p-value of Likelihood ratio test on multinomial logit with and without all covariates</i>	0.59							

Table C.1: Tests of balance in observables across waves

C.2 Parallel Trends and Anticipation By Year

Figure C.1 plots a time series of the average person-days of employment for each wave.

It suggests parallel pre-trends between the treatment and control groups leading up to the

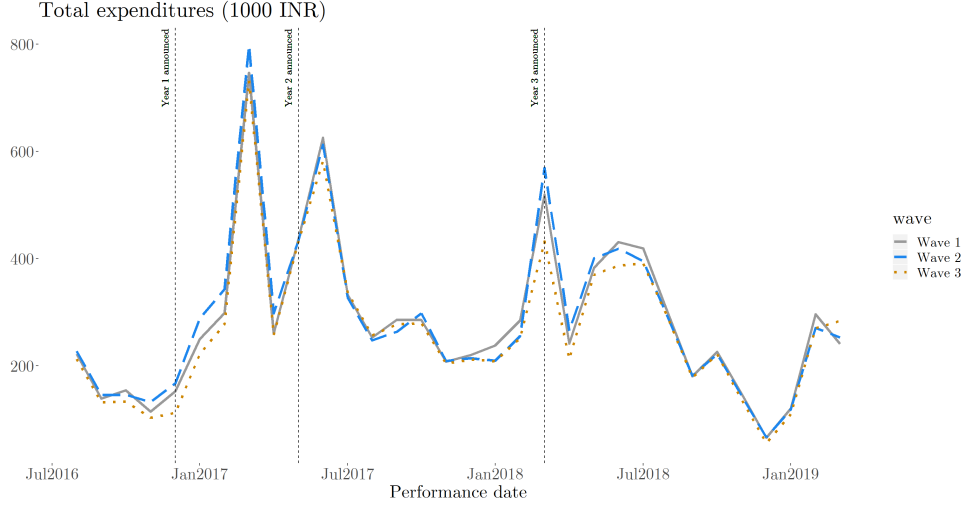


Figure C.1: Raw means of monthly total expenditures, by wave

first announcement (denoted by the first dashed line).⁵⁰ Furthermore, those being audited experience slight declines in employment as audits are implemented throughout the year.

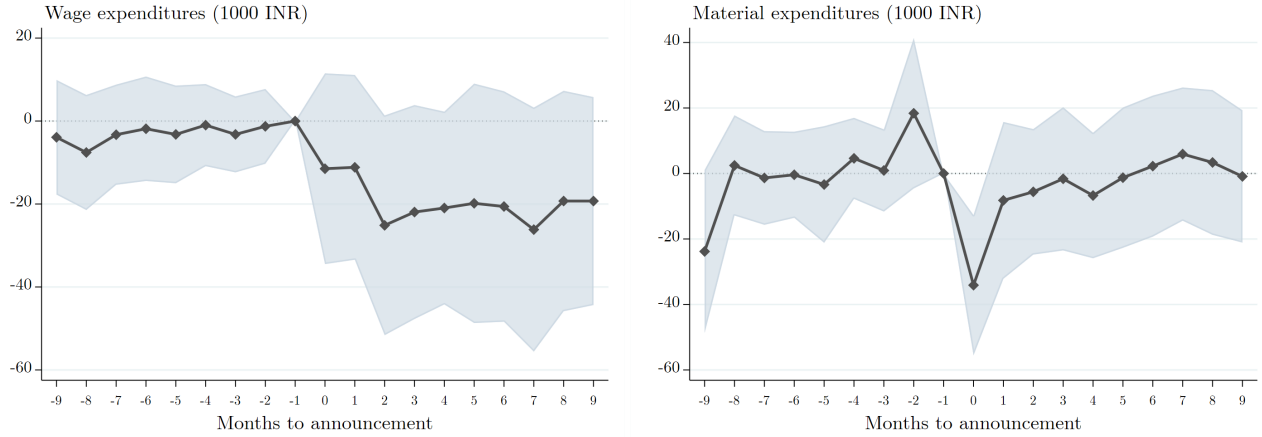


Figure C.2: Announcement event study for wage and material expenditures. The omitted category is the month before the announcement. The raw mean of the omitted category is 179 and 69 (1000 INR) and the p -values of no pre-trends are 0.99 and 0.11 for wage and material expenditures, respectively. The regression includes GP and district month-year fixed effects. Standard errors are clustered by block.

There are two events for every wave of the audit where the GPs potentially have an incentive to change their behavior as a response: 1) the announcement of the audit schedule

50. The spike in employment from March-June 2016 is likely attributed to severe droughts earlier in the year that led to increased demand for employment during a weak harvest. Since this event occurs prior to the roll-out of audits, I do not consider it a threat to identification.

which is simultaneously distributed across all GPs; and 2) the audit itself. Once informed of the audit, bureaucrats in their respective audit group may respond differentially in anticipation of the audit. When estimating the effect of the audit itself, we should not expect parallel pre-trends in the treatment and control group if one group reacts in anticipation of the audit while the other is operating business as usual.

As discussed in Section 4.1, we should expect parallel pre-trends between the groups prior to the Year 1 announcement of Wave 1 audits and can expect parallel pre-trends prior to the announcement in subsequent years after accounting for the horizons of pre-audit anticipation. We can test for violations of parallel trends by estimating the following event study around the time of announcement:

$$y_{it} = \alpha_i + \alpha_{dt} + AnticipatingAudit'_{it}\beta + \sum_{k \in \tau} \delta^k Announce^k_{it} + \varepsilon_{it} \quad (C.2)$$

where $Announce^k_{it}$ is a dummy variable taking a 1 if a GP is k months from learning when they will be audited at time t . This vector of dummy variables, indexed by k , comprise our lags and leads to the announcement.

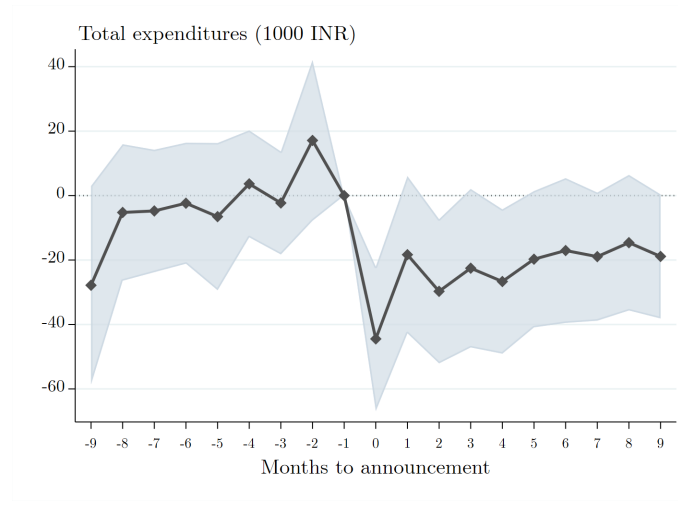


Figure C.3: Announcement event study for total expenditures without controlling for anticipatory behavior. The omitted category in this regression is the one month lead before the announcement. The raw mean of the omitted category is 248 (1000 INR) and the p -values of no pre-trends is 0.34. The regression includes GP and district month-year fixed effects. Standard errors are clustered by block.

In the months before the announcement, the total expenditures do not seem to be following a trend and are statistically indistinguishable from behavior the month before the announcement (p -value = 0.45, Figure 3). This lends credibility to our difference-in-differences approach. Results look similar for wage and material expenditures (p -value = 0.19 and 0.99, respectively) captured in Figure C.2.

The same event study, excluding variables capturing potential pre-audit anticipatory behavior, lead to the same conclusion—that we cannot reject that there are no pre-trends. However, breaking up the estimation by wave, there is evidence of pre-trends leading up to the Year 2 announcement for Wave 2 audits. This supports our primary difference-in-differences specification in Equation 1. It accounts for horizons of anticipatory behavior to not only provide credible estimates of the effect of the announcement and the audit, but also validates our interest in estimating the parameters of anticipatory behavior.

Figure C.3 estimates Equation C.2 but excludes the variables capturing the horizons where GPs potentially anticipate their first audit. We are led to the same conclusion that we cannot reject there is no pre-trend prior to each wave’s announcement (p -value = 0.41).

But, if we examine the same event study on total expenditures by comparing wave 1 to those not audited in Wave 1, and Wave 2 to those not yet audited in Wave 2, we observe that we cannot reject a test of pre-trends in the Wave 1 comparison (p -value = 0.35) but we reject pre-trends in the Wave 2 comparison (p -value = 0.01). Since our balance checks support that the GPs selected for audit in each wave were randomly selected, then it is more likely the case that the observed pre-trends in the Wave 2 comparison are attributed to differences in the beliefs over being audited in the next fiscal year leading up to each wave’s respective announcement of audit. For this reason, our preferred specification through this paper is to account for horizon of anticipation of one’s first audit as specified in Equations 1- D.3.

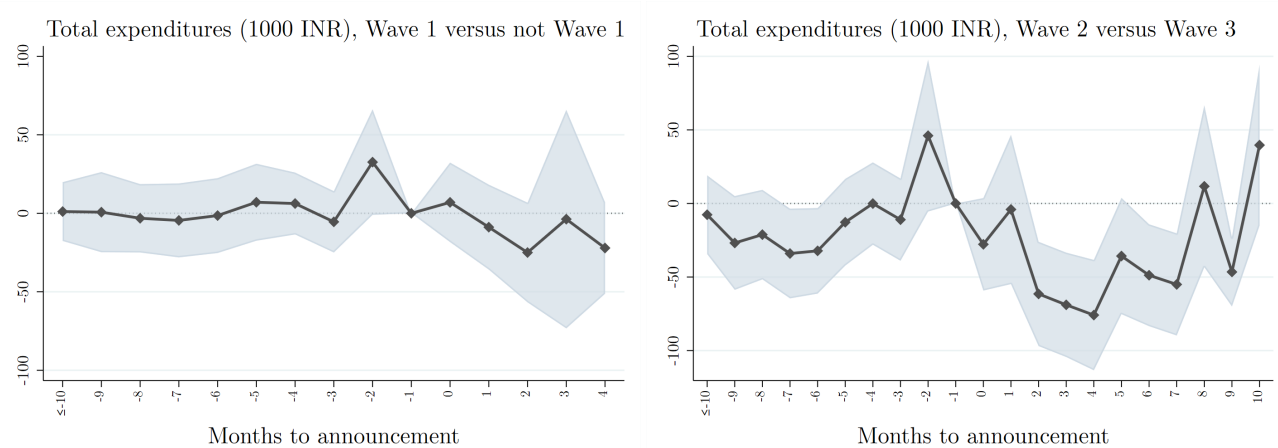


Figure C.4: Announcement event study by wave for total expenditures. The omitted category in this regression is the one month lead before the announcement. It includes GP and district month-year fixed effects as specified under Equations C.2 and D.3, and observations are weighted by the inverse of number of households in the GP. The figure on the left compares those selected for audit in Wave 1 to those not audited and the panel of data is truncated at the month prior to the announcement of the second wave audit. The figure on the right compares those selected for audit in Wave 2 to those not yet audited (and will be audited in Wave 3) and the panel of data is truncated at the month prior to the announcement of the third wave audit. So, for instance, the announcement pre-periods in the figure on the left include data from both Waves 2 and 3 while the announcement post-periods include data only from Wave 2.

C.3 Alternatives to the baseline specification

This section presents alternatives to the baseline specification and shows that results from our preferred specification are robust.

Table C.2 shows the results for the specification in Equation 1 using only month-year fixed effects and not district-month-year fixed effects in the first column. The remaining columns show the specification in Equation 1 for additional outcome variables. Results are qualitatively similar for other measures of employment.

Figure C.5 shows the specification in Equation C.2 using month-year and block-month-year fixed effects. Qualitatively, the results are similar and we cannot reject that there are no pre-trends (p -value = 0.36 and 0.53, respectively). I use district \times month FE over block \times month-year fixed effects because for Wave 2 of the audit, all GPs within a block were assigned to be audited if they were considered to be small blocks. In this case, incorporating block month-year fixed effects would absorb variation being generated by

	(1) Total expenditures (1000 INR)	(2) # HHs provided employment	(3) Person-days of work generated	(4) Delay in days
<i>Anticipating1stAudit</i> - Year 2	14.34 (11.81)	-0.540 (2.539)	-26.64 (39.95)	-0.554 (0.766)
<i>Post1stAnnounce</i> , disaggregated:				
Before <i>1stAudit</i>	-11.31 (10.46)	-3.472 (2.273)	-64.97* (35.68)	0.0817 (0.654)
Month of <i>1stAudit</i>	-42.48*** (12.61)	-16.32*** (2.410)	-273.1*** (37.42)	0.543 (0.724)
After <i>1stAudit</i>	-17.43 (11.67)	-4.350* (2.466)	-84.82** (38.67)	0.295 (0.751)
<i>Anticipating2ndAudit</i> - Year 2	-2.338 (15.64)	-4.080 (3.708)	-78.75 (57.25)	-0.0239 (1.278)
<i>Anticipating2ndAudit</i> - Year 3	-39.07*** (13.51)	-10.50*** (3.172)	-169.4*** (47.91)	0.787 (1.264)
Observations	233,760	233,760	233,760	206,795
Time FE	monyr	district-monyr	district-monyr	district-monyr
Baseline mean	269.5	87.48	1197	21.12
Adj. R-squared	0.34	0.55	0.50	0.10

Table C.2: Effect of stages of the monitoring policy on additional outcomes and specifications. The first regression uses month-year FE for the main outcome variable, total expenditures; the remaining regressions show the effect of the policy on other performance outcomes using district-month-year FE. Standard errors are clustered by block. The omitted category is the horizon of anticipating one's first audit during Year 1. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

the announcement and audit for blocks that met this criteria.

Finally, the preferred specification for the outcome variables on expenditures is levels of expenditures, rather than log-transformed for example. Several reasons inform this decision. First, the results are qualitatively robust under a log transformation of the outcome variable. Second, zero expenditures in wages and materials by month are not uncommon. Taking the log transformation of total, wage, and material expenditures would drop different observations in regressions across each outcome variable. Using levels of expenditures ensures that regressions are performed on the same sample for each outcome variable. Finally, a Box-Cox test, which tests goodness of fit of transformations of the outcome variable, was also performed. Residuals from regressions with the transformation of best fit (including an inverse hyperbolic sine transformation) were bimodal, and not normally distributed. This would violate our fixed effects regression assumption that

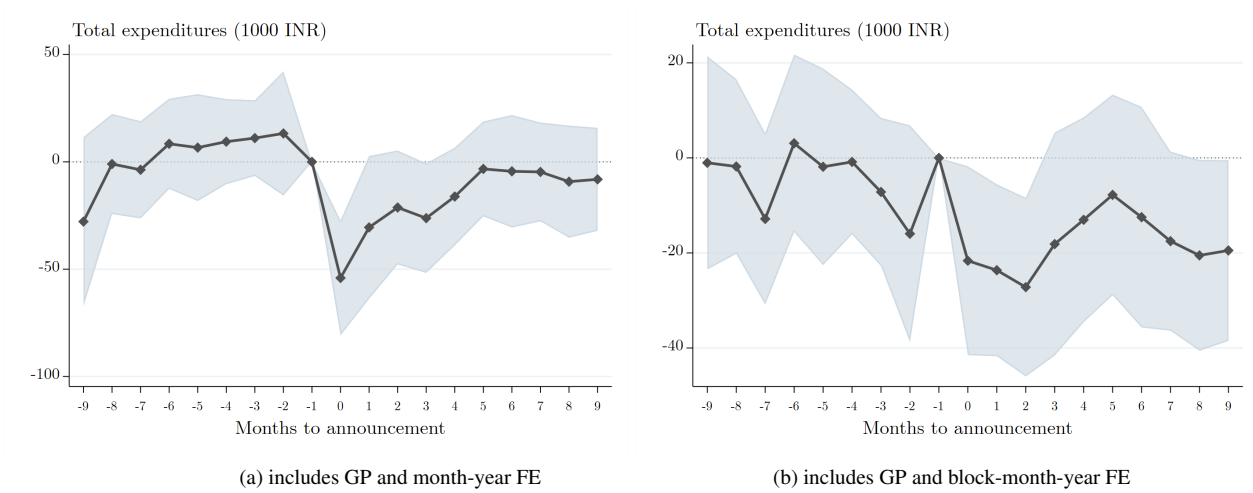


Figure C.5: Announcement event study with month-year and block-month-year fixed effects. Standard errors are clustered by block.

the residuals be normally distributed.

C.4 The importance of using monthly performance data

To show the importance of using more-frequent, monthly data on bureaucrat performance for our main analysis, I compare performance across waves with annual data. I construct annual means by treatment group, where treatment groups are defined by wave of audit.

We should expect that the difference in means by treatment group should be statistically indistinguishable from zero during the pre-audit periods which include FY 2014-15 and FY 2015-16. This is the true for all comparisons except when we compare total expenditures in FY 2014-15 for those in Wave 1 to those in Wave 3, which could be due to chance occurrence.

From Year 1 onward, performance is statistically distinguishable performance across waves. Using data at the annual level is too coarse to detect any changes in behavior in the various stages of the response to the audit policy, especially since horizons of anticipation do not overlap perfectly with fiscal years. This further supports our approach to using available data on frequent measures of bureaucrat performance to infer bureaucrat adjustments to the audit policy especially during periods of performance outside the scope of

	Total Expenditures (1000 INR)				
	(1)	(2)	(3)	(4)	(5)
	FY1415	FY1516	FY1617	FY1718	FY1819
			<i>Year1</i>	<i>Year2</i>	<i>Year3</i>
Treatment group mean by fiscal year					
<i>Wave1</i>	2,465	3,154	4,382	3,906	3,024
<i>Wave2</i>	2,405	3,196	4,301	3,768	2,868
<i>Wave3</i>	2,296	3,046	4,224	3,866	3,025
t-tests of differences in means across treatment groups					
<i>Wave2 – Wave1</i>	-60.13 [0.503]	41.95 [0.693]	-81.09 [0.195]	-138.5 [0.382]	-156 [0.219]
<i>Wave3 – Wave1</i>	-169.3 [0.0441]**	-108 [0.257]	-157.9 [0.574]	-40.54 [0.768]	0.0873 [0.114]
<i>Wave2 – Wave3</i>	109.2 [0.154]	149.9 [0.257]	76.80 [0.479]	-97.98 [0.338]	-156.1 [0.999]
Adj. R-squared	0.37	0.38	0.40	0.29	0.28
<i>N</i>	3,896	3,896	3,896	3,896	3,896

Table C.3: Annual difference in means in total expenditures (1000 INR). Regressions include block fixed effects to account for the randomization design. *p*-values are in brackets and reflect tests of difference in estimated coefficients for each Wave. Standard errors are clustered by block. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

audit. Frequent measures of verification of bureaucrat performance are infeasible during periods outside the scope of audit, and in this particular context third-party sources of verification are either unavailable or not possible for checking measures correlated with expenditures on materials and labor.

D The Impact of Changing Expectations

D.1 Testing for Spillover Effects of the Audit

This section estimates whether the direct effects from the audit are confounded with spillover effects from GPs within your own block. Random assignment of GPs to audit means that the concentration of audits within a block is also random. So, we can estimate the spillover effects of the audit from GPs within the same block. Results show

spillover effects from being audited are not a concern for identification of the direct and anticipatory effects of the audit policy.

Spillover effects can be a concern because communication among peers in other GPs within your block may effect your own performance. Block managers describe how performance and administrative matters at lower administrative units are often discussed as a group. Group texting applications like *WhatsApp* are often used to communicate information. This suggests a free flow of information across GPs within the same block, where group performance and administrative matters like the audit are discussed.

When treatment is randomized, spillover effects can be estimated in a reduced-form linear-in-means specification by including a control for share of GPs being audited within one's block (Manski 1993; Bobonis and Finan 2009; Lalive and Cattaneo 2009; Dieye et al. 2014):

$$y_{it} = \alpha_i + \alpha_{dt} + \text{AnticipatingAudit}_{it}'\beta + \delta_1 \text{BeforeAudit}_{it} + \delta_2 \text{MonthofAudit}_{it} + \delta_3 \text{AfterAudit}_{it} + \text{AnticipatingNextAudit}_{it}'\gamma + \eta \text{ShareBlockAudited}_{it} + \varepsilon_{it} \quad (\text{D.2})$$

where *ShareBlockAudited* denotes the share of GPs being audited in *i*'s block at time *t*. The spillover effect through the linear-in-means specification is identified when (i) spillover effects are equal across audited and not audited groups, and (ii) the spillover effects are linear in share of group being audited (Vazquez-Bare 2017).

First, an event study around time to announcement including *ShareBlockAudited* as a control shows that the results do not change (Figure D.1). Furthermore, an *F*-test ($p = 0.84$) shows support for parallel pre-trends.

Table D.1 Column 1 shows the main specification from Equation 1 without spillover effects. Column 2 includes *ShareBlockAudited* as a control and shows that the estimates on both anticipatory and direct audit effects are unaffected. Column 3 interacts *ShareBlockAudited* with an indicator for the month of audit to provide evidence for assumption (i) that we cannot reject the spillover effects, if any, are equal across audited and not audited groups during the month of audit. Column 4 includes a quadratic term

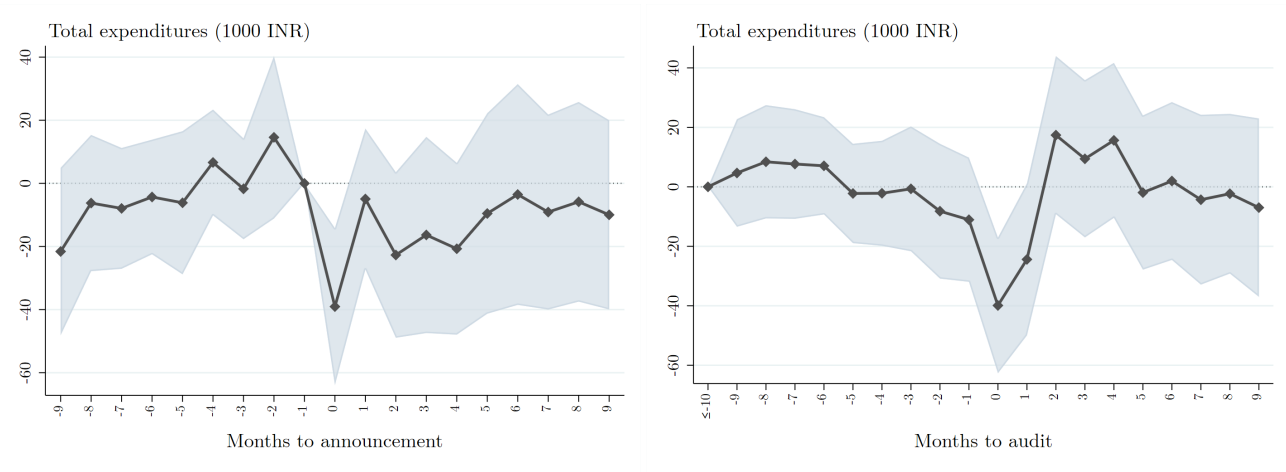


Figure D.1: Event studies around time to announcement and audit are unaffected by controlling for spillovers. The omitted category is 10 or more months before the announcement and audit, respectively. The raw mean of the omitted category is 250 and 272 (1,000 INR) for the announcement and audit event studies, respectively. The regressions include GP and district-month-year fixed effects. Standard errors are clustered by block.

for *ShareBlockAudited* and provides evidence for assumption (ii) where the coefficient on the quadratic terms is insignificant. This tells us we cannot reject the spillover effects are linear in the share of group receiving treatment.

Furthermore, Table D.1 shows overall that the estimated anticipatory and direct effects of the audit are largely unchanged after accounting for concentration of audits within one's block. This provides additional evidence that the anticipatory effects are driven by changes in expectations of being audited rather than perceptions about the audit driven by peer experiences.

Figure D.1 shows an event study around month of audit (controlling for '*AnticipatingAudit* - Year 2' as in the main specification) and the results remain unchanged when controlling for spillovers. As before, we cannot reject the hypothesis of no pre-trends prior to one's announcement (p -value = 0.5). Altogether, the estimates in the main analysis are robust to spillover concerns.

	Total expenditures (1,000 INR)			
	(1)	(2)	(3)	(4)
<i>Anticipating 1st Audit</i> - Year 2	13.87 (11.33)	15.52 (11.27)	14.54 (11.28)	15.42 (11.28)
<i>Post 1st Announce, disaggregated:</i>				
<i>Before 1st Audit</i>	-18.83** (9.46)	-17.00* (9.43)	-17.93* (9.47)	-16.97* (9.44)
<i>Month of 1st Audit</i>	-42.07*** (11.68)	-49.06*** (11.59)	-42.42** (17.25)	-49.17*** (11.61)
<i>After 1st Audit</i>	-8.42 (11.26)	-6.44 (11.25)	-7.47 (11.17)	-6.40 (11.27)
<i>Anticipating 2nd Audit</i> - Year 2	-4.78 (13.83)	-3.47 (13.85)	-4.47 (13.65)	-3.50 (13.83)
<i>Anticipating 2nd Audit</i> - Year 3	-39.38*** (13.30)	-37.76*** (13.30)	-38.97*** (13.33)	-37.69*** (13.31)
<i>ShareBlockAudited</i>		25.64 (18.96)		40.35 (65.87)
<i>ShareBlockAudited</i> ²				-21.10 (87.61)
<i>Month of 1st Audit</i> = $0 \times \text{ShareBlockAudited}$			32.71 (22.70)	
<i>Month of 1st Audit</i> = $1 \times \text{ShareBlockAudited}$			10.85 (31.92)	
Observations	233,760	233,760	233,760	233,760
Baseline mean	269.5	269.5	269.5	269.5
Adj. R-squared	0.40	0.40	0.40	0.40
<i>H</i> ₀ : (Month of 1st Audit = 0 × ShareBlockAud) – (Month of 1st Audit = 1 × ShareBlockAud), p-val = 0.57				

Table D.1: Tests for spillover effects of the audit. All regressions include district-month-year and GP fixed effects. Standard errors are clustered by block. The omitted category is the horizon of anticipating one's first audit during Year 1 (*Anticipating Audit* - Year 1). The baseline is the mean from the beginning of the panel (two years prior to first audits) up to and including the period captured by '*Anticipating Audit* - Year 1'. *** p<0.01, ** p<0.05, * p<0.1.

D.2 Additional results with audit report data

	Number of issues, by type:							
	Wage misappropriation				Material receipts misappropriation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wave 3 - Wave 2	-0.495 (0.330)	-0.344 (0.299)	-0.049 (0.357)	-0.246 (0.547)	1.023*** (0.133)	1.015*** (0.157)	0.887*** (0.164)	1.047*** (0.256)
Audit manager experience				0.023 (0.044)				-0.019 (0.019)
Wave 2 Mean	3.67	3.67	3.79	3.79	0.42	0.42	0.40	0.40
Controls		X	X	X		X	X	X
Manager FE			X	X			X	X
Observations	2,361	2,361	1,772	1,772	2,361	2,361	1,772	1,772
Adjusted R ²	0.392	0.417	0.535	0.535	0.266	0.267	0.289	0.289

Table D.2: Differences in audit issue counts across waves is consistent with differences in wage and material expenditures while anticipating the first audit. Unit of observation is GP. All regressions include block fixed effects. Control variables: number of employed households and works to verify; and number of auditors. Audit manager experience is measured by number of audits conducted to date. Standard errors are clustered by block. *p<0.1; **p<0.05; ***p<0.01.

	Share of total issues, by type:								
	Wage misappropriation			Material receipts misappropriation			Project non-existent		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Wave 3 - Wave 2	-0.055*** (0.010)	-0.043*** (0.010)	-0.032* (0.017)	0.036*** (0.005)	0.037*** (0.006)	0.037*** (0.011)	-0.005 (0.011)	-0.007 (0.011)	-0.026 (0.021)
Audit manager experience			-0.0002 (0.001)			-0.001 (0.001)			0.002 (0.002)
Wave 2 Mean	0.17	0.17	0.17	0.04	0.04	0.04	0.09	0.09	0.09
Controls		X	X		X	X		X	X
Manager FE			X			X			X
Observations	2,361	2,361	1,772	2,361	2,361	1,772	2,361	2,361	1,772
Adjusted R ²	0.247	0.265	0.356	0.224	0.224	0.233	0.238	0.242	0.301

Table D.3: Audit performance across waves is consistent with differences in behavior while anticipating the first audit. Unit of observation is GP. All regressions include block fixed effects and the following control variables: number of employed households and works to verify; and number of auditors. Audit manager experience is measured by number of audits conducted to date. Standard errors are clustered by block. *p<0.1; **p<0.05; ***p<0.01.

	Issue fine amount (1000 INR), by type:							
	Records not provided to auditors				Bills/Vouchers not provided to auditors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wave 3 - Wave 2	154.967* (81.643)	252.967*** (60.431)	273.056*** (87.711)	434.863** (172.601)	23.038*** (7.428)	22.106** (9.337)	23.039* (12.766)	36.734*** (10.527)
Audit manager experience				-18.943 (12.720)				-1.603 (1.406)
Wave 2 Mean	147.43	147.43	150.67	150.67	9.93	9.93	9.62	9.62
Controls		X	X	X		X	X	X
Manager FE			X	X			X	X
Observations	2,361	2,361	1,772	1,772	2,361	2,361	1,772	1,772
Adjusted R ²	0.127	0.174	0.210	0.211	0.051	0.055	0.142	0.143

Table D.4: Differences in audit issue fine amounts across waves for issues related to not providing any records to auditors for verification. The first outcome variable pools issues where no registers, bills, or vouchers are provided. The second outcome presents only issues related to bills or vouchers not being provided. Control variables: number of employed households and works to verify; and number of auditors. Audit manager experience is measured by number of audits conducted to date. Standard errors are clustered by block. *p<0.1; **p<0.05; ***p<0.01.

	Number of issues, by type:							
	Records not provided to auditors				Bills/Vouchers not provided to auditors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wave 3 - Wave 2	1.336*** (0.192)	1.119*** (0.190)	1.309*** (0.261)	0.634** (0.314)	0.112*** (0.033)	0.103*** (0.034)	0.144*** (0.045)	0.121** (0.052)
Audit manager experience				0.079*** (0.030)				0.003 (0.005)
Wave 2 Mean	1.82	1.82	1.83	1.83	0.13	0.13	0.13	0.13
Controls		X	X	X		X	X	X
Manager FE			X	X			X	X
Observations	2,361	2,361	1,772	1,772	2,361	2,361	1,772	1,772
Adjusted R ²	0.216	0.222	0.316	0.318	0.058	0.057	0.158	0.158

Table D.5: Differences in audit issue counts across waves for issues related to not providing any records to auditors for verification. The first outcome variable pools issues where no registers, bills, or vouchers are provided. The second outcome presents only issues related to bills or vouchers not being provided. Unit of observation is GP. All regressions include block fixed effects. Control variables: number of employed households and works to verify; and number of auditors. Audit manager experience is measured by number of audits conducted to date. Standard errors are clustered by block. *p<0.1; **p<0.05; ***p<0.01.

D.3 Deterrence and substitution during the audit

Event studies around the month of audit in Section 5.3 are estimated with the following specification:

$$y_{it} = \alpha_i + \alpha_{dt} + \text{AnticipatingAudit}_{it}'\beta + \sum_{k \in \tau} \delta^k \text{Audit}_{it}^k + \varepsilon_{it} \quad (\text{D.3})$$

where Audit_{it}^k is an indicator taking the value 1 if i is k months from audit at time t . As discussed in Section 4.1, the set of fixed effects account for potentially endogenous timing of audits due to scheduling constraints of the audit agency.

Figure D.2 disaggregates the event study by examining employment by whether the project worked on required materials or only required labor. It shows that the decline in employment is observed across projects of both types. Moreover, the observed increase in materials 2-4 months after the audit (third figure of Figure 4) does not correspond to an increase in employment on projects requiring materials as shown in Figure D.2 (a). Likewise, this suggests that the increase in material expenditures 2-4 months after the audit did not result in an increase in real output.

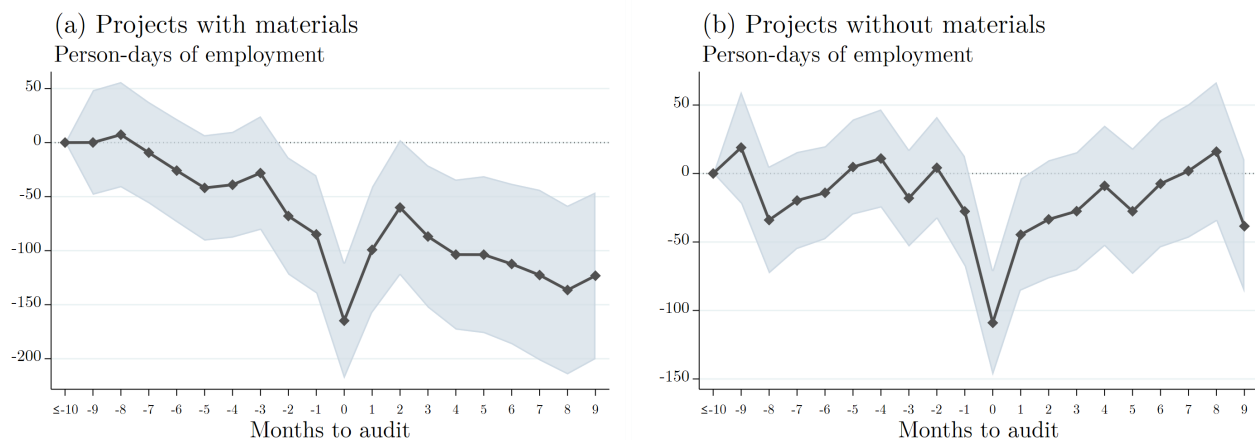


Figure D.2: Person-days of employment around the time of audit, by whether the project requires materials. The omitted category is 10 or more months before the audit was conducted. The raw mean of the omitted category is 855 and 310 work-days for projects requiring and not requiring materials, respectively. The regression includes GP and district month-year fixed effects as specified under Equation D.3. Standard errors are clustered by block.

D.4 Delayed payments as a measure of effort

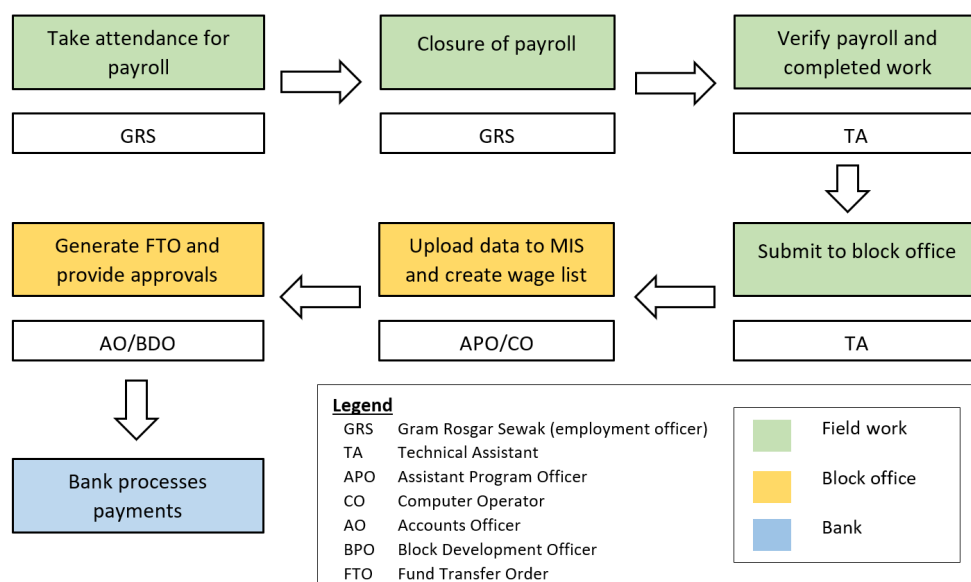


Figure D.3: Example of stages to the NREGS payment process. White boxes beneath steps present the officials responsible for implementing that step. FTO denotes fund transfer order. Gram Rozgar Sewak (GRS) is one of the key GP-level personnel and also responsible for allocating days of work. Source: Evidence for Policy Design, September 2015 presentation to the Ministry of Rural Development of the Government of India.

The problem of delayed payments is well-documented in the literature (Banerjee et al. 2020; Narayanan et al. 2019). Delays are counted as days over the 15 day maximum for processing payment from the time of the closure of the payroll (or muster roll). There are several steps in the administrative process between attendance for work and processing the wage payment through the bank as shown in Figure D.3. Delays tend to occur during the closure of the payroll to entering the data into their MIS at the GP level; between data entry to generation of the wagelist at the block level; and between the first signature of the fund transfer order to the second signature at the block level.⁵¹ According to the NREGS National Act, workers should be compensated 0.05% of unpaid wages for each day of delay in wage payment.⁵²

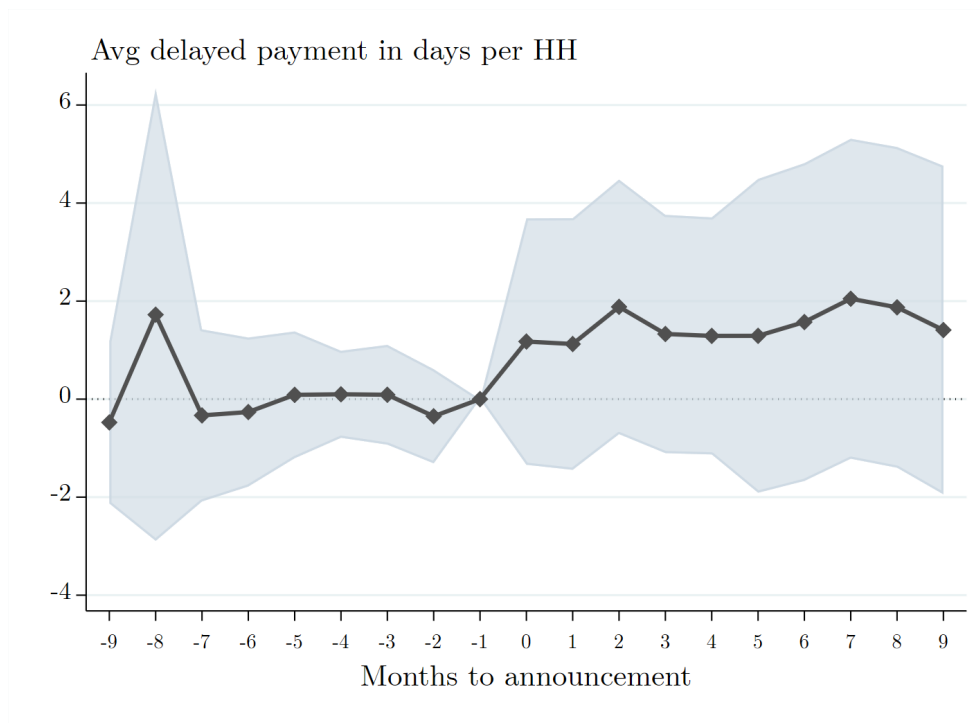


Figure D.4: Announcement event study on mean days of delay per household. This omitted category in this regression is the one month lead before the announcement and its mean is 1.9 days of delay per household. The p -value on a test of no pre-trends is 0.97. Standard errors are clustered by block.

Delayed payments are often cited to reflect a lack of a core competency of the gov-

51. According to the Evidence for Policy Design, September 2015 presentation to the Ministry of Rural Development of the Government of India.

52. The Mahatma Gandhi National Rural Employment Guarantee Act, 2005.

ernment in targeting resources⁵³, and not a known mechanism through which bureaucrats misappropriate finances. Delays are a low-cost task for bureaucrats to shirk while auditors are present. They are a lagged indicator of performance and current measures of delay are not detectable by auditors. Studying delays in making wage payments also allows us to disentangle GP bureaucrat effort from incentives to misappropriate. When examining the effect of the audit on delayed payments, I consider the possibility that these outcomes are reflective of the amount of effort exerted by the bureaucrat. Although, in theory, it is possible that withholding payments for honest work could be used by bureaucrats as leverage. But, it is an unlikely strategy since increased delays reflect negatively on performance and thus costly for bureaucrats.

This section complements the analysis in Section 5.3 on whether delays are affected around the time of audit. Figure D.4 shows that prior to a month before the announcement, mean delayed payments in days per household do not follow a statistically distinguishable trend (p -value = 0.97). Furthermore, there are also no statistically distinguishable effects following the announcement, although there is an average increase in delays post-announcement.

Figure D.5 plots the event study around month of audit and shows the audit does not affect delays in making wage payments. There is no statistically significant change in average days of delayed payment per household during the month around the time of audit; the average increase during the month of audit is less than one day of delayed payment per household. This provides a piece of evidence that multi-tasking issues during the audit are not driving the measured decline in employment and increase in material procurement.

53. Aggarwal, A. (2017). *Ten Ways MGNREGA Workers Do Not Get Paid*. Economic&Political Weekly

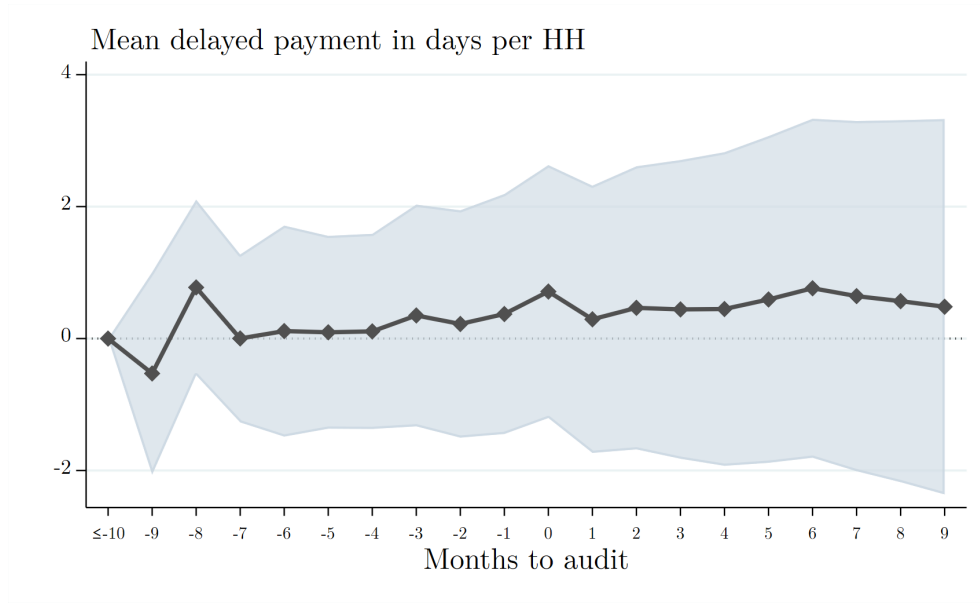


Figure D.5: Audit event study on delays in making wage payments. The omitted category is 10 or more months before the audit was conducted and its mean is 20 mean delayed payment in days per household. The regression includes GP and district month-year fixed effects as specified under Equation D.3. Standard errors are clustered by block.

E The Optimal Design of Information and Counterfactual Signals

E.1 Testing the Implication that Bureaucrat Perceptions of the Audit Remained Constant

This section tests the empirical implications of Assumption 1 that the differences in bureaucrats' response across the horizons of anticipation are explained only by their differences in expectations of being audited. These are tests for whether bureaucrats' perception of audit quality was constant across years.

	Workload per auditor		Total audit expenditures (INR)	
	(1)	(2)	(3)	(4)
Wave 3	-7.326 (10.066)	-1.897 (4.540)	-1,660.990 (1,127.538)	-1,584.749 (1,013.885)
Num. HHs to check		0.432*** (0.019)		-0.191 (2.201)
Num. works to check		0.422*** (0.032)		9.910** (4.051)
Num. Auditors		-44.329*** (3.221)		1,137.143 (738.204)
Mean of Dep Var	249.69	249.69	27,206.45	27,206.45
Observations	2,445	2,445	2,676	2,445
Adjusted R ²	0.441	0.944	0.298	0.346

Table E.1: Audit quality by year of audit. Unit of observation is the GP. Omitted group is Wave 2. Outcome variables are: workload per auditor measured as the number of households and projects to verify per auditor; and total audit expenditures. Control variables include: Number of employed households and works to verify; and number of auditors. Standard errors are clustered by block. *p<0.1; **p<0.05; ***p<0.01

Using data from audit reports on audit inputs, we can test for differences in audit implementation to see if audit quality was comparable across years. Workload per auditor and audit expenditures serve as proxy variables for audit quality. Workload per auditor is measured as the sum of households and projects to be verified divided by number of auditors. Only a subset of data from Waves 2 and 3 are available. Table E.1 shows no statistically significant differences between Wave 2 and 3 in workload per auditor and audit expenditures.

These results together with the fact that the rules of the audit were unchanged support that the quality of audit implementation did not change over time. However, despite this, bureaucrats may have responded differently over time for reasons unrelated to changing expectations, e.g. learning about the audit process. If the perceived quality or credibility of the audit is changing, then one might expect the response to vary by year; if constant, then one might expect estimates to be same across years. To test for this we can extend

the estimates from Table 1 Panel B by disaggregating the treatment effects by year for total expenditures, the main outcome of interest for estimating the sufficient statistic in Section 6.

Table E.2 shows that the treatment effects for ('Month of *Audit*', and 'After *Audit*') across Year are on average different but not statistically distinguishable. We cannot reject the hypothesis that the true difference between their responses is zero. If the null is true, then the implication from Assumption 1 that Bureaucrat perceptions of the audit are constant across years is reasonable.

On the other hand, if the parameter is *not* truly zero, then it could be that we are not powered to detect small differences given the sample size. And the estimated response of bureaucrats is confounded with changing perceptions of audit quality. Section E.2 addresses this possibility by conducting robustness checks which relax this implication from Assumption 1.

	Total expenditures (1,000 INR)	
	(1)	(2)
<i>Anticipating 1st Audit</i> - Year 2	13.87 (11.33)	14.33 (11.76)
<i>Post 1st Announce</i> , disaggregated:		
Before 1st Audit	-18.83** (9.46)	-17.72** (8.56)
Month of 1st Audit	-42.07*** (11.68)	
Month of 1st Audit - Year 1		-54.61** (23.46)
Month of 1st Audit - Year 2		-45.10*** (15.50)
Month of 1st Audit - Year 3		-33.04** (14.54)
After 1st Audit	-8.42 (11.26)	
After 1st Audit - Year 1		26.69 (30.58)
After 1st Audit - Year 2		-10.08 (13.39)
After 1st Audit - Year 3		-6.95 (12.43)
<i>Anticipating 2nd Audit</i> - Year 2	-4.78 (13.83)	-1.74 (13.68)
<i>Anticipating 2nd Audit</i> - Year 3	-39.38*** (13.30)	
<i>Anticipating 2nd Audit</i> - Year 3, Wave 1		-28.12** (13.98)
<i>Anticipating 2nd Audit</i> - Year 3, Wave 2		-41.24*** (12.63)
Observations	233,760	233,760
Baseline mean	269.5	269.5
Adj. R-squared	0.397	0.400
Month of 1st Audit, H_0 : Year 1 = Year 2 = Year 3 (p-val)		0.67
After 1st Audit, H_0 : Year 1 = Year 2 = Year 3 (p-val)		0.54
<i>Anticipating 2nd Audit</i> - Year 3, H_0 : Wave 1 = Wave 2 (p-val)		0.22

Table E.2: Effect of stages of the monitoring policy disaggregated by year. This table estimates the main differences-in-differences specification breaking down ‘Month of *Audit*’ and ‘After *Audit*’ by year and ‘*Anticipating Next Audit* - Year 3 by Wave’ for total expenditures. Regressions include district-month-year and GP fixed effects. Standard errors are clustered by block. The omitted category is the horizon of anticipating one’s first audit during Year 1 (*Anticipating Audit* - Year 1). The baseline is the mean from the beginning of the panel (two years prior to first audits) up to and including the period captured by *Anticipating Audit* - Year 1. This longer period is included in the baseline to average out seasonal variation in expenditures. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

E.2 Sensitivity analysis and robustness of conclusions

This section evaluates the sensitivity of our conclusions about the optimal monitoring policy to: inherent noise from the regression estimates, and Assumptions 1 and 2. How wrong would our assumptions have to be in order for us to conclude that an uninformative signal would have been better? Technically, we want to know under what belief and deterrence parameters would the sufficient statistic be linear (i.e. Principal indifferent) or weakly/locally concave at q_0 (i.e. Principal prefers uninformative signal)? The results from this section provide greater certainty that the sufficient statistic is a convex curve as shown in Figure 5 and that a signal informative about audit risk is optimal.

For these sensitivity checks, I produce the joint sampling distribution of the estimated coefficients using the block bootstrap. With this, we can compute the likelihood that the jointly estimated coefficients lead to a sufficient statistic with an alternative conclusion. The marginal sampling distribution of each estimated coefficient converges after bootstrapping about 10,000 draws. I bootstrap 100,000 samples to report results with 0.001% confidence.

First, I assess deviations from the mean of deterrence estimates. This exercise assesses the likelihood we observe an alternative conclusion using the bootstrapped joint sampling distribution of the estimated coefficients (Column 4 of Table 1). Under the assumptions $K_{t+1} = \{\frac{1}{2}(K_{t-1} + K_t), K_t, K_{t+1}\}$, the probability of realizing an alternative conclusion is less than 0.001%.⁵⁴ If $K_{t+1} = K_{t-1}$, there's a 0.02% chance of realizing an alternative conclusion. If $K_{t+1} = K_{Trend_t}K_t$, there is a 30% chance of being indifferent between an informative and uninformative signal. There is not enough information under this assumption to say whether this is a strict preference because there are only 3 points of support. If we exclude deterrence estimates capturing behavior during the month of audit ('Month of *Audit*'), the results are similar: under assumptions $K_{t+1} = \{\frac{1}{2}(K_{t-1} + K_t), K_t, K_{t+1}\}$ the

54. The likelihood under $K_{t+1} = K_t$ of a local concavity is 2% on average, but the local concavity occurs at 'AnticipatingNextAudit - Year 3' where the expectations of likelihood of an audit is 96%. This means that all GPs should have the same information if the prior probability of an audit is at least that high. Given the audit agency's capacity is about 50% at it's highest, an uninformative signal would still not be optimal.

likelihood is less than 0.005%; about 0.02% likelihood if $K_{t+1} = K_{t-1}$ and 34% likelihood if $K_{t+1} = Trend_t K_t$.

Second, I relax the first implication of Assumption 1 that bureaucrat perceptions of audit quality are constant across years. As discussed in Appendix E.1, bureaucrat responses are statistically indistinguishable across years for each variable that spans multiple years: ‘Month of *Audit*’, ‘After *Audit*’, and *AnticipatingNextAudit* - Year 3. This supports the assumption that perceptions of audit quality were constant across years. But, to rule out the possibility of a false negative, I relax this assumption.

If the estimated parameter across years is not truly 0, then perhaps we were not powered to detect the true effect size. Given the estimates for *PostAnnounce*, ‘Month of *Audit*’, and ‘After *Audit*’, a power analysis provides the minimal detectable difference for each variable’s estimate by year. Under standard inference rules of rejecting the null with 95% confidence and accepting the alternative with 80% confidence, the minimum detectable effect is at most 2.3 (1,000 INR) in total expenditures across all variables.

To be conservative, I model a constant bias of ± 5 (1,000 INR) where bias adjustments for each coefficient are assumed to be in the direction of simulating a concave function (the alternative conclusion). Results are presented in Table E.3. With these adjustments, it is still unlikely that an uninformative signal is optimal (1.6% under $K_{t+1} = K_{t-1}$; $< 0.1\%$ under $\frac{1}{2}(K_{t-1} + K_t)$, K_t , K_{t+1} ; 37.5% under $Trend_t K_t$). The bias would have to be at least ± 9 (1,000 INR) to find a 15% chance of an alternative conclusion under $K_{t+1} = K_{t-1}$ while likelihoods under other belief assumptions are similar in magnitude when assuming a bias of ± 5 (1,000 INR). While the likelihood of an alternative conclusion under $Trend_t K_t$ is not insubstantial, as before, there is not enough information to conclude whether there’s a strict preference (given 3 points of support).

abs(Bias)	Beliefs Assumption, $K_{\tau+1} =$	Prob. Alt. Conclusion
2	$K_{\tau-1}$	0.001
2	$\frac{1}{2}(K_{\tau-1} + K_{\tau})$	0.0001
2	K_{τ}	0.00004
2	$K_{\tau+1}$	0
2	$\text{Trend}_{\tau} \times K_{\tau}$	0.352
5	$K_{\tau-1}$	0.016
5	$\frac{1}{2}(K_{\tau-1} + K_{\tau})$	0.001
5	K_{τ}	0.0005
5	$K_{\tau+1}$	0
5	$\text{Trend}_{\tau} \times K_{\tau}$	0.375
7	$K_{\tau-1}$	0.055
7	$\frac{1}{2}(K_{\tau-1} + K_{\tau})$	0.006
7	K_{τ}	0.001
7	$K_{\tau+1}$	0
7	$\text{Trend}_{\tau} \times K_{\tau}$	0.375
9	$K_{\tau-1}$	0.148
9	$\frac{1}{2}(K_{\tau-1} + K_{\tau})$	0.021
9	K_{τ}	0.004
9	$K_{\tau+1}$	0
9	$\text{Trend}_{\tau} \times K_{\tau}$	0.375
15	$K_{\tau-1}$	0.711
15	$\frac{1}{2}(K_{\tau-1} + K_{\tau})$	0.294
15	K_{τ}	0.091
15	$K_{\tau+1}$	0.0002
15	$\text{Trend}_{\tau} \times K_{\tau}$	0.375
20	$K_{\tau-1}$	0.840
20	$\frac{1}{2}(K_{\tau-1} + K_{\tau})$	0.747
20	K_{τ}	0.476
20	$K_{\tau+1}$	0.007
20	$\text{Trend}_{\tau} \times K_{\tau}$	0.375

Table E.3: Likelihood of alternative conclusion relaxing assumptions on constant perceptions of audit quality and assuming bias in estimates. The bias incorporated purposely increased the likelihood of drawing an alternative conclusion. So, for e.g., ± 5 bias was incorporated for each anticipatory group such that the sufficient statistic was closer to being concave. The bias was modeled separately for each beliefs assumption. The probability of an alternative conclusion indicates the likelihood that the bootstrapped joint distribution of estimated coefficients formed a sufficient statistic that an uninformative signal was optimal (weakly/locally concave).

Third, I assess deviations from assumptions on beliefs. I simulate perturbations from assumed beliefs (bound between 0 and 1) and use the deterrence estimates for each bootstrapped sample. The perturbations can take any value, within the range of assumed beliefs for each anticipatory group (i.e. assumptions on beliefs from Table 3), in order to simulate a sufficient statistic that is linear or concave. Among these perturbations, only 0.5% of the sample would yield a sufficient statistic that is linear or concave. Conservative estimates that exclude behavior estimated during the month of audit (coefficient captured by ‘Month of *Audit*’) yield a 1.2% likelihood of a sufficient statistic that is linear or concave with simulated beliefs within the range of assumed beliefs.

E.3 Welfare calculations

This section describes how welfare across the information policies were computed.

I analyze welfare for three policies: (1) the actual implemented policy of randomizing without replacement; (2) an uninformative signal where all GPs have no additional information over the prior and have the same expectations of being audited (equivalent to randomizing with replacement); and (3) an informative signal where all GPs are perfectly informed in advance of when they will be audited or not.

For each policy, I examine total expenditures over the course of the 27 months that it took for the round of first audits to roll-out under the original monitoring policy. I examine how total expenditures changes as beliefs evolve under counterfactual policies. For each policy, I exclude the year during which a GP experiences an audit to exclude behavior after one learns about their audit prior to receiving the audit and behavior during the months after the audit but still within the audit year. We can think of each calculation as excluding behavior during this period which may capture other phenomena of GPs responding to the policy not related to expectations of being audited, e.g. perceived salience of audits holding expectations fixed.

The calculation of counterfactual expenditures for each policy estimates anticipatory behavior using the deterrence estimates from Year 1 through Year 3. The calculation for

each policy is discussed in detail below:

- (1) Partially informative signal (or randomization without replacement, p_1)

$$\begin{aligned} DeterredExpenditures_{p_1} = & n_{Months,Year1} \left[(n_{GPs,Wave2} + n_{GPs,Wave3}) U(q_{Year1}) \right] \\ & + n_{Months,Year2} \left[n_{GPs,Wave3} U(q_{Year2}) + n_{GPs,Wave1} U(q'_{Year2}) \right] \\ & + n_{Months,Year2} \left[(n_{GPs,Wave1} + n_{GPs,Wave2}) U(q'_{Year3}) \right] \end{aligned}$$

where $n_{GPs,Wavex}$ corresponds to the number of GPs in Wave x , $n_{Months,Year y}$ corresponds to the number of months during Year y that the monitoring policy was in place, i.e. 4 months in Year 1, 11 months in Year 2, and 12 months in Year 3. $U(q)$ corresponds to the amount of deterred misappropriated expenditures as a function of bureaucrats' posterior beliefs, q , on the likelihood of an audit. Under this signal capturing the actual policy that took place, $U(q_{Year1})$ corresponds to the 'AnticipatingAudit - Year 1'; $U(q_{Year2})$ corresponds to 'AnticipatingAudit - Year 2'; $U(q'_{Year2})$ corresponds to 'AnticipatingNextAudit - Year 2'; and $U(q'_{Year3})$ corresponds to 'AnticipatingNextAudit - Year 3'.

- (2) Uninformative signal (or randomization with replacement, p_2)

$$\begin{aligned} DeterredExpenditures_{p_2} = & n_{Months,Year1} \left[(n_{GPs,Wave2} + n_{GPs,Wave3}) U\left(\frac{K_{Year2}}{N}\right) \right] \\ & + n_{Months,Year2} \left[(n_{GPs,Wave2} + n_{GPs,Wave3}) U\left(\frac{K_{Year3}}{N}\right) \right] \\ & + n_{Months,Year3} \left[(n_{GPs,Wave1} + n_{GPs,Wave2}) U\left(\frac{K_{Year4}}{N}\right) \right] \end{aligned}$$

where N is the total number of GPs; $K_{Year y}$ is the assumption made on next year's audit capacity. When $\frac{K_{Year y}}{N}$ is equal to a probability corresponding to one of the anticipatory groups, then $U\left(\frac{K_{Year y}}{N}\right)$ is the corresponding regression estimate. When $\frac{K_{Year y}}{N}$

is not equal to a probability corresponding to one of the anticipatory groups, then $U\left(\frac{K_{Year\ y}}{N}\right)$ is estimated by linear interpolation between the two nearest anticipatory group estimates.

(3) Fully informative signal (or p_3)

$$\begin{aligned} DeterredExpenditures_{p_3} = & n_{Months, Year1} \left[n_{GPs, Wave2} U(1) + n_{GPs, Wave3} U(0) \right] \\ & + n_{Months, Year2} \left[n_{GPs, Wave1} U(0) + n_{GPs, Wave3} U(1) \right] \\ & + n_{Months, Year3} \left[n_{GPs, N-Wave4} U(0) + n_{GPs, Wave4} U(1) \right] \end{aligned}$$

In the main analysis, I assume that deterrence under $U(1)$ is equivalent to behavior when the auditors are present (estimates provided by ‘Month of *Audit*’. The reported conservative estimates assume that deterrence under $U(1)$ is equivalent to behavior in Year 3 when Waves 1 and 2 believe with very high likelihood they will be audited in Year 4 (estimates provided by ‘*AnticipatingNextAudit* - Year 3’. These are conservative estimates because first, they address concerns that some other behavioral phenomena may be driving the response when auditors are present. Second, under some beliefs assumptions about tomorrow’s audit capacity (see Table 3), the expectations of being audited captured by ‘*AnticipatingNextAudit* - Year 3’ can be less than 1. This makes the estimate conservative because the deterrence response from bureaucrats under $U(1)$ could be greater than what is estimated with ‘*AnticipatingNextAudit* - Year 3’.

Finally, when we assume $K_{\tau+1} = K_{\tau+1}$, I estimate $U(0)$ using ‘*AnticipatingNextAudit* - Year 2’ where the beliefs for that group are 0.04. I do so because this is the minimum probability under this assumption, with no other point of support to interpolate what $U(0)$ might be. This approach provides conservative estimates because we would expect the gains in welfare to be smaller with $U(0.04)$, provided that we have assumed $U(q)$ is monotonically increasing in q .

I provide standard errors for the welfare calculations using the bootstrapped estimates of the deterrence parameters.

Table E.4 shows the results from the welfare analysis comparing the full information signal to the some information and uninformative signals under each beliefs assumption.

Beliefs Assumption, $K_{\tau+1} =$	Mean Inform. (Mil. USD)	Inform.— Some Info.	Std. Error	Inform.— Uninform.	Std. Error
	(1)	(2)	(3)	(4)	(5)
$K_{\tau-1}$	226	21.17	0.038	32.3	0.038
$\frac{1}{2}(K_{\tau-1} + K_{\tau})$	226	21.75	0.038	38.1	0.038
K_{τ}	226	21.75	0.037	36.8	0.037
$\text{Trend}_{\tau} \times K_{\tau}$	240	7.87	0.037	2.8	0.037
$K_{\tau+1}$	227	20.78	0.035	32.5	0.035
<i>Estimates excluding Month of Audit:</i>					
$K_{\tau-1}$	228	19.24	0.038	30.3	0.038
$\frac{1}{2}(K_{\tau-1} + K_{\tau})$	228	19.82	0.038	36.2	0.038
K_{τ}	228	19.82	0.037	34.9	0.037
$\text{Trend}_{\tau} \times K_{\tau}$	247	0.93	0.037	2.0	0.037
$K_{\tau+1}$	228	19.82	0.036	31.6	0.036

Table E.4: Welfare analysis comparing the full information signal to the uninformative and some information signals. Welfare is measured as differences in expenditures (Columns 2 and 4), which we interpret as differences in misappropriate expenditures driven by changing expectations of being audited. The rows present the welfare calculations under each assumption on beliefs about next year's audit capacity, with conservative calculations that exclude behavior during *Month of Audit*. Standard errors for the differences in means are provided in Columns 3 and 5.