Pay for Locally Monitored teacher attendance?

A welfare analysis for Ugandan primary schools

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Abstract

Local monitoring of schools has the potential to provide policymakers with information at low cost. If accurate, this information can then serve the dual purpose of incentivizing performance and reducing policy mistakes. Received wisdom suggests that the benefit of raising performance via financial incentives should be balanced against the risk of collusion resulting in false reporting. We evaluate two different forms of local monitoring by head teachers of Ugandan primary schools, randomly varying whether reports of teacher attendance trigger financial incentive payments or not. A theoretical model allows us to make social welfare comparisons, taking into account impacts on teacher attendance, cost of monitoring, and also policy mistakes due to false reporting. Consistent with the model, we find that teacher attendance, monitoring frequency, and the number of false reports all increased with the introduction of financial incentives. More surprisingly, but again consistent with the theory, we find that the number of policy mistakes actually decreased: there were more false reports but this effect was counter-balanced by more reports in general and (hence) fewer mistakes due to a lack of information. Taken together, our results suggest that social welfare was higher with financial incentives.

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1 Introduction

To build incentives for civil servants, public sector organizations often rely on reports from interested parties that are costly to verify. These systems fail when managers or the agents themselves exert effort to game—and even to outright misreport—these performance metrics. When they go awry, such incentive schemes risk failure on two margins: they may fail to incentivize the desired behavior, and they may provide systematically biased information about outcomes that can mislead broader planning processes.

Public policy is replete with cases of delegated monitoring and incentive schemes gone wrong. Secretary of the U.S. Department of Veteran’s affairs resigned after an audit revealed evidence of fraudulent behavior in 64 percent of VA health care facilities, where employees kept veterans off of official waiting lists in order to meet targeted 14-day waiting times for appointments.¹ Teachers have been found to cheat on high-stakes student assessment in the Atlanta, Chicago, and Washington, DC school districts.² French colonial history provides a dramatic tale of incentives gone wrong, when colonial administrators’ attempts to incentivize the collection of rats resulting in widespread rat farming (Vann 2003). More recently, conditional transfers of cash or goods in kind have increasingly been used to motivate targeted behaviors, but recent work has raised questions about agents’ scope for manipulation of the metrics by which these conditions are measured. (Linden & Shastry 2012)

In this paper, we address these issues in the context of Ugandan primary education. We ask whether and how incentives for civil servants—here, primary school teachers—can be effectively designed when based on delegated monitoring. This is an important issue in Ugandan education, where teacher absenteeism levels are such that pupils in rural, northern Uganda receive only 50 effective days of instruction in the entire school year (Wane & Martin 2013).³ Beyond the direct advantage of incentivising attendance, the reported information can also be used by government for planning purposes. A parallel concern that the information is therefore the reliability of the reports.

We develop an intuitive theoretical framework to understand how the preferences of both agents and monitors affects reporting and attendance, and how this depends on the financial stakes attached to the reports. This allows us to evaluate the joint welfare consideration on both metrics: attendance and quality of information. In particular, a bureaucrat needs to make a (binary) policy decision. When there is no reporting, his best guess is that the teacher is present. It therefore makes a policy mistake whenever a teacher is absent, but the monitor either (i) reports him/her as present, or (ii) does not submit a report. The theory predicts that policy mistakes due to false reports of presence will be higher with financial mistakes, but policy mistakes due to non-reports when the teacher is present will be lower. This is because the monitor is more likely to submit a report and the teacher is more likely to be present. The net welfare impact, however, is ambiguous. We then conduct a randomized, controlled trial that tests alternative monitoring schemes, experimentally varying the existence of financial stakes attached to local

³Comparable problems exist in schooling systems across the developing world (Chaudhury et al. 2006)
reports. This experiment provides tests of the positive predictions of the theory, and our theory provides a normative lens through which to gauge the welfare impacts of alternative intervention designs. We find that most theoretical predictions of the model hold: with the introduction of bonus payments, teacher attendance is higher and policy mistakes due non-reports of absent teachers are lower. We further find that the total number of policy mistakes actually decreased with. Taking these estimates back to the model, we conclude that social welfare is higher in the case of financial incentives.

This paper builds on a related stream of literature that considers monitoring and incentive schemes to improve public-sector outcomes, in developing-country education systems and beyond. In education, Muralidharan and Sundararaman (Muralidharan & Sundararaman 2011) demonstrate the potential effects of using measures of learning outcomes to incentivize teachers; however, such schemes may be costly to administer and may lead to distortionary activities by teachers (Glewwe, Ilias & Kremer 2010). Incentivizing teacher inputs is attractive insofar as these (a) margins of effort such as presence are both important and well aligned with the production of learning, and (b) these expose teachers to substantially less risk than test-based accountability. While ‘automated’ measurement of teacher presence has been effective in NGO schools in India (Duflo, Hanna & Ryan 2012), efforts to scale up such approaches among public-sector (health) workers were met with challenges (Banerjee et al. 2007) that suggest understanding how monitoring schemes interact with the preferences of agents and monitors is crucial to designing scalable incentive schemes. This message is reinforced in recent work by Duflo and colleagues (forthcoming), who show that environmental auditors suffer from conflicts of interest that substantially affect the data they report and the subsequent behavioral responses of firms.

This paper makes three principal contributions to this literature. First, the randomized, controlled trial we conduct provides direct evidence of the efficacy of alternative policies to promote teacher presence in Uganda. Second, the theory of delegated monitoring developed and tested here has application to a variety of contexts—it may apply wherever incentives for public servants depend on reports by interested parties, with costly verification. Third, from a methodological perspective, our paper highlights three advantages of closely incorporating theory in such experimental work: theory yields several testable predictions that we take to our data; it provides a lens to guide the selection and specification of outcomes considered; and it provides a normative framework through which welfare comparisons can be made across alternative treatment regimes.

The remainder of the paper proceeds as follows. Section 2 introduces a theory of delegated monitoring, which yields both positive predictions for monitors’ and teachers’ responses to alternative treatment regimes and normative, welfare criteria for comparing outcomes under each. Section 3 outlines the experiment and data. Section 4 describes the empirical results: tests of theoretical predictions, welfare calculations, enrollment impacts, and evidence of robustness. Section 5 concludes.

2 Theory

2.1 Model

Players and actions The economy consists of: a teacher (he), a head-teacher (she), a government bureaucracy (it), and $n$ identical parents. In all arms, the teacher chooses whether to attend school, $a \in \{0,1\}$. In the control arm, the head-teacher plays no active role. In both treatment arms, the head-
teacher chooses whether to monitor $m \in \{0, 1\}$. If the head-teacher monitors, $m = 1$, she observes $a$ and chooses a public report $r \in \{0, 1\}$. We will say that the head-teacher send a truthful report iff $r = a$. If the head-teacher does not monitor, $m = 0$, she cannot send a report.\footnote{We provide evidence to support this assumption that monitoring is necessary to send a report in Section X below.} In all arms, the bureaucracy takes a policy decision $p \in \{0, 1\}$. We will say that the bureaucracy makes a policy mistake whenever $p \neq a$.

In the Info+Bonus treatment arm, the bureaucracy pays a cash bonus $\beta$ directly to the teacher iff he is reported present, $r = 1$. Parents play no active role under any treatment arm.

## Payoffs
All players are risk neutral. Net of any side-transfers, the payoffs of the strategic players are:

\[
U^T = 1_{\{m=1,r=1\}} \cdot \beta - 1_{\{a=1\}} \cdot C_T - 1_{\{r=0\}} \cdot \delta \\
U^H = 1_{\{a=1\}} \cdot \varepsilon^H - 1_{\{m=1\}} \cdot C^H \\
U^G = 1_{\{a=1\}} \cdot n \varepsilon^P - 1_{\{m=1,r=1\}} \cdot \beta - 1_{\{p \neq a\}} \cdot \kappa.
\]

If the teacher attends school, $a = 1$, he incurs a (possibly negative) cost of $C_T$. If the teacher attends, the head-teacher and a representative parent receive private benefits of $\varepsilon^H$ and $\varepsilon^P$. If the head-teacher monitors, she incurs a (possibly negative) cost of $C^H$. Reporting entails no further cost for the head-teacher but a mark of absent, $r = 0$, imposes a reputational cost of $\delta$ on the teacher. If the head-teacher is indifferent, we assume that she reports truthfully. Finally, a policy mistake entails a loss of $\kappa$ for the bureaucracy. We will use the bureaucracy’s payoff—i.e. the total parental benefit from teacher attendance less the cost of cash bonus and/or policy mistake—as our welfare criterion.

## Key Assumptions
The costs $C_T$ and $C^H$ are observed by the head-teacher and teacher but not by any other player. From the bureaucracy’s perspective, these costs are random variables. For convenience, we assume that realizations are drawn independently from uniform distributions. The lower and upper support of the distribution of $C_T$ are denoted by $C_T^\text{low}$ and $C_T^\text{high}$. To calibrate the model to the baseline absenteeism rate, we assume $-C_T^\text{low} > \overline{C} > 0$. The lower and upper support of the distribution of $C^H$ are denoted similarly, although here we simply assume $\overline{C}^H > 0 > \underline{C}^H$. Again for convenience and in the spirit of rationalizing baseline absenteeism, we assume that the bonus $\beta$ is the only source of transferable utility.\footnote{If all sources of utility were transferable, then (via Coasian logic) the players should reach a jointly-efficient outcome. The high rates of absenteeism reported above suggest this is not the case. In reality, $\delta$ might be partly transferable. We assume non-transferability to make the distinction between the Info and Info+Bonus arms as clear as possible.} Relatedly, we assume that side-contracts sharing $\beta$ are costless and enforceable, and that the head-teacher can commit to monitor. Finally, we assume that parameters satisfy

\[
\frac{n \varepsilon^P}{\varepsilon^H (C_T - C_T^\text{low})} > \varepsilon^H > \delta > 0,
\]

implying that the parental gain from teacher attendance must be sufficiently high.\footnote{As we discuss below, this is a sufficient condition for the bureaucracy to choose $\beta \geq \varepsilon^H$ in the Info+Bonus treatment.}

## Timing
To emphasize the differences across arms, it is worth spelling out the order of play. The timing in the Control arm is:

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0. Nature draws a realization of $C_T$ and reveals this cost to the teacher.

1. The teacher chooses whether to attend school, $a \in \{0, 1\}$. Simultaneously, the bureaucracy chooses a policy decision $p \in \{0, 1\}$. Payoffs are realized and the game ends.

The timing in the Info arm is:

0. The bureaucracy announces the monitoring scheme. Nature draws realizations of $C_T$ and $C_H$ and reveals both of these costs to the teacher and the head-teacher.

1. The head-teacher chooses whether to make an announcement to the teacher. An announcement $R(a)$ commits the head-teacher to monitor, $m = 1$, and specifies the report $r$ that the head-teacher will send to all players following each possible action $a$.

2. The teacher chooses whether to attend school, $a \in \{0, 1\}$.

3. If the head-teacher made the announcement at Stage 1, she monitors and sends the public report $r = R(a)$. Otherwise, the head-teacher takes no action.

4. The bureaucracy chooses a policy decision $p \in \{0, 1\}$. Payoffs are realized and the game ends.

The timing in the Info+Bonus arm:

0. The bureaucracy announces the monitoring and incentive scheme. Nature draws realizations of $C_T$ and $C_H$ and reveals both of these costs to the teacher and the head-teacher.

1. The head-teacher chooses whether to make a side-contract offer to the teacher. A side-contract $<R(a), \tau>$ commits the head-teacher to monitor, specifies the report $r$ that the head-teacher will send to all players following the action $a$ and specifies the side-transfer $\tau$ that the teacher will pay to the head-teacher in the event that $r = 1$.

If the side-contract is accepted at Stage 1, the game continues as follows.

2. The teacher chooses whether to attend school, $a \in \{0, 1\}$.

3. The head-teacher monitors and sends the public report $r = R(a)$. If $r = 1$, the bureaucracy pays $\beta$ to the teacher who then transfers $\tau$ to the head-teacher.

4. The bureaucracy chooses a policy decision $p \in \{0, 1\}$. Payoffs are realized and the game ends.

If the side-contract is not accepted at Stage 1, the game proceeds as in the Info arm except that at Stage 3 the bureaucracy transfers $\beta$ to the teacher in the event that $r = 1$.

2.2 First Best

To provide a benchmark for later analysis, we begin by considering the outcome when both the teacher’s cost $C_T$ and her action $a$ are public information. We continue to restrict the set of mechanisms available to the bureaucracy to simple bonus contracts for the teacher. Since the bureaucracy observes $a$ there will never be a policy mistake, implying that we can ignore $\kappa$. 

5
In the status quo with no bonus scheme, the teacher will attend school iff \( C^T \leq \delta \), in which case \( U^G = n \varepsilon^P \) (and otherwise \( U^G = 0 \)). Now suppose that the bureaucracy offers to pay the teacher a bonus \( \beta \) if and only if she attends school. The teacher will attend iff \( C^T \leq \delta + \beta \), in which case \( U^G = n \varepsilon^P - \beta \) (and otherwise \( U^G = 0 \)). It follows that the bureaucracy will adopt the bonus scheme iff the teacher’s cost takes an intermediate value: \( \delta < C^T < n \varepsilon^P + \delta \). Specifically, for these parameter values, the bureaucracy commits to pay a bonus of \( \beta = C^T - \delta \) iff \( a = 1 \) (and otherwise nothing). Hence, in equilibrium, the teacher attends iff \( C^T < n \varepsilon^P + \delta \).

### 2.3 Analysis

We now turn to our main theoretical analysis of the model in Section 2.1. Readers who wish to skip these derivations can proceed directly to Section 2.4 where we provide a graphical representation of the four testable predictions. Expected welfare comparisons are set out in Section 2.5.

#### 2.3.1 Control arm

The equilibrium outcome is straightforward to establish. The teacher cares only about her participation cost and so attends iff \( C^T \leq 0 \). The probability of teacher attendance is therefore

\[
\Pr[a = 1|\text{Control}] = \Pr[C^T \leq 0] = \frac{-C^T}{C^T - C^T}.
\]

The bureaucracy cares only about minimizing the probability of making a policy mistake. Given the symmetric loss function, the bureaucracy will set \( p = 1 \) iff it believes the teacher is more likely to be present than absent. Anticipating the teacher’s strategy, the bureaucracy deduces that the probability of teacher attendance is

\[
\Pr[a = 1|m = 0] = \frac{\Pr[m = 0, a = 1]}{\Pr[m = 0]} = \Pr[C^T \leq 0] = \frac{-C^T}{C^T - C^T} > 0.5,
\]

and so it chooses \( p = 1 \). The probability of a policy mistake is therefore

\[
\Pr[p \neq a|\text{Control}] = \Pr[C^T > 0] = \frac{C^T}{C^T - C^T}.
\]

It follows that the bureaucracy’s expected payoff is

\[
E[U^G_{\text{Control}}] = n \varepsilon^P \cdot \Pr[a = 1|\text{Control}] - \kappa \cdot \Pr[p \neq a|\text{Control}]
\]

\[
= n \varepsilon^P \cdot \frac{-C^T}{C^T - C^T} - \kappa \cdot \frac{C^T}{C^T - C^T}.
\]

#### 2.3.2 Info Arm

The equilibrium outcome is still fairly straightforward to establish. There are four different cases.

- **Truthful reporting of teacher presence, \( m = 1, r = a = 1 \).**
  This outcome arises if \( 0 < C^T \leq \delta \) and \( C^H \leq \varepsilon^H \). The head-teacher knows that if she commits to monitor and report truthfully then the teacher will attend school (because \( C^T \leq \delta \)) giving her a
payoff of $\varepsilon H - C^H$. This is a better outcome for the head-teacher than if she does not commit to monitor, since the teacher will not attend school (because $C^T > 0$) giving her a payoff of 0. This outcome also arises if $C^T \leq 0$ and $C^H \leq 0$. The head-teacher knows that monitoring has no impact on the teacher, as he will always attend. Since the head-teacher derives utility from monitoring she chooses to do so.

- **Truthful reporting of teacher absence, $m = 1, r = a = 0$.**
  This outcome arises if $C^T > \delta$ and $C^H < 0$. The head-teacher knows that monitoring has no impact on the teacher, as he will never attend. Since the head-teacher derives utility from monitoring she chooses to do so.

- **No monitoring of teacher absence, $m = 0, a = 0$.**
  This outcome arises if $C^T > \delta$ and $C^H > 0$. The head-teacher knows that monitoring has no impact on the teacher, as he will never attend. The head-teacher therefore refrains from costly monitoring.

- **No monitoring of teacher presence, $m = 0, a = 1$.**
  This outcome arises if $C^T \leq 0$ and $C^H > 0$. The head-teacher knows that monitoring has no impact on the teacher, as he will always attend. The head-teacher therefore refrains from costly monitoring.

The probability of teacher attendance is therefore

$$Pr[a = 1|\text{Info}] = Pr[C^T \leq 0] + Pr[0 < C^T \leq \delta, C^H \leq \varepsilon H] = \frac{-C^T}{C^T - \varepsilon H} + \frac{\delta(-C^H + \varepsilon H)}{(C^T - \varepsilon H)(C^H - \varepsilon H)}.$$  \hspace{1cm} (5)

and the probability of monitoring is

$$Pr[m = 1|\text{Info}] = Pr[C^H \leq 0] + Pr[0 < C^T \leq \delta, C^H \leq \varepsilon H] = \frac{-C^H}{C^H - \varepsilon H} + \frac{\delta\varepsilon H}{(C^T - \varepsilon H)(C^H - \varepsilon H)}.$$  \hspace{1cm} (6)

In contrast, to the Control arm, the bureaucracy now reaches three information sets, and so we must consider how it forms beliefs at each of these. The first information set is $m = 0$. Anticipating the teacher and head-teacher’s strategies, the bureaucracy deduces that the probability of teacher attendance at this information set is

$$Pr[a = 1|m = 0] = \frac{Pr[m = 0, a = 1]}{Pr[m = 0]} = \frac{Pr[C^T \leq 0, C^H > 0]}{Pr[C^T \leq 0] + Pr[C^T > 0, C^H > 0] - Pr[0 < C^T \leq \delta, 0 < C^H \leq \varepsilon H]}$$

$$= \frac{\frac{-C^T C^H}{C^T - \delta\varepsilon H - \varepsilon H} > \frac{-C^T}{C^T - \varepsilon H}}{0.5},$$

and so it chooses $p = 1$. The second information set is $r = 1$. Since the bureaucracy knows that the head-teacher reports truthfully, it chooses $p=1$. The third information set is $r = 0$. Again, since the bureaucrat knows that the head-teacher reports truthfully, it chooses $p=0$. The probability of a policy
mistake is therefore

\[
\Pr[p \neq a | \text{Info}] = \Pr[C^T > 0] - \Pr[C^T > 0, C^H \leq 0] - \Pr[0 < C^T \leq \delta, 0 < C^H \leq \varepsilon^H]
\]

\[
= \frac{\beta}{\beta + \varepsilon^H} - \frac{\delta^H \varepsilon^H - \beta C^T + \varepsilon^H \delta^H}{(C^T - C^T)(\delta^H - C^H)}.
\]

(7)

It follows that the bureaucracy’s expected payoff is

\[
E[U_{\text{Info}}^G] = n \varepsilon^P \cdot \Pr[a = 1 | \text{Info}] - \kappa \cdot \Pr[p \neq a | \text{Info}]
\]

\[
= n \varepsilon^P \cdot \left(\frac{-C^T}{C^T - C^T} + \frac{\delta^H \varepsilon^H - \beta C^T + \varepsilon^H \delta^H}{(C^T - C^T)(\delta^H - C^H)}\right) - \kappa \cdot \left(\frac{\delta^H \varepsilon^H - \beta C^T + \varepsilon^H \delta^H}{(C^T - C^T)(\delta^H - C^H)}\right).
\]

(8)

2.3.3 Info+Bonus arm

We show in Appendix A that, under the parameter assumptions in (1), if the bureaucracy chooses the Info+Bonus arm, then the optimal \( \beta \) must exceed \( \varepsilon^H \). This again gives rise to four cases, although crucially one equilibrium outcomes differs to the Info arm; there is now untruthful reporting.

- **Truthful reporting of teacher presence, \( m = 1, r = a = 1 \).**

  This outcome arises if \( 0 < C^T \leq \varepsilon^H \) and \( C^H \leq \varepsilon^H + \beta - C^T \). There are two sub-cases. If \( C^T \leq \delta \) and \( C^H \leq \varepsilon^H \), the teacher’s outside-option is a payoff of \(-C^T\) (since the head-teacher will commit to monitor and report truthfully even if she cannot extract any incentive payment from the teacher).

  It follows that a side-contract of truthful reporting and a transfer of \( \tau = \beta \) will be accepted, giving the head-teacher a payoff of \( \varepsilon^H + \beta - C^H > 0 \). This is better than the head-teacher can achieve via any other contract. Intuitively, information alone is enough to incentivise the teacher to attend, so, since the head-teacher has the bargaining power, she captures all of the bonus payment. If \( \varepsilon^H \geq C^T > \delta \) and/or \( \varepsilon^H + \beta \geq C^H > \varepsilon \), the teacher’s outside-option is a payoff of 0 (since the head-teacher will not commit to monitor and report truthfully). It follows that a side-contract of truthful reporting and a transfer of \( \tau = \beta - C^T \) will be accepted, giving the head-teacher a payoff of \( \varepsilon + \beta - C^T - C^H > 0 \). This is better than the head-teacher can achieve via any other contract. Intuitively, the head-teacher now has to share some of the bonus payment with the teacher to compensate him for his participation cost. This cost is sufficiently low relative to the head-teacher’s private benefit from teacher presence to make such an ‘incentive’ side-contract worthwhile.

  This outcome also arises if \( C^T \leq 0 \) and \( C^H \leq \beta \). The teacher’s outside option is \(-C^T > 0\). Hence a side-contract of a truthful report and a transfer of \( \tau = \beta \) will be accepted, giving the head-teacher a payoff of \( \varepsilon^H + \beta - C^H \). This is better than the head-teacher can achieve via no contract iff \( C^H \leq \beta \).

  Since her monitoring cost is sufficiently low, the head-teacher offers a ‘superfluous’ side-contract simply to collect the bonus payment.

- **Collusion, untruthful reporting of teacher presence, \( m = 1, r = 1, a = 0 \).**

  This outcome arises if \( C^T > \varepsilon^H \) and \( C^H \leq \beta \). The teacher’s outside option is 0. Hence a side-contract of untruthful report (\( r = 1 \) for any \( a \)) and a transfer of \( \tau = \beta \) will be accepted, giving the head-teacher a payoff of \( \beta - C^H > 0 \). This is better than the head-teacher can achieve via any other contract. Intuitively, the head-teacher no longer finds it worthwhile to incentivise the
Teacher to attend (his participation cost is too high). Since her monitoring cost is sufficiently low, the head-teacher offers a ‘collusive’ side-contact simply to collect the bonus payment.

- **No monitoring of teacher absence,** $m = 0, a = 0$.
  This outcome arises if $C^H > \beta$ and $C^H > \epsilon^H + \beta - C^T$. The head-teacher’s monitoring cost is now so high that neither the incentive nor the collusive side-contact is worthwhile.

- **No monitoring of teacher presence,** $m = 0, a = 1$.
  This outcome arises if $C^T \leq 0$ and $C^H > \beta$. The monitoring cost is now too high, and so the head-teacher withdraws the ‘superfluous’ side-contact.

The probability of teacher attendance is therefore

$$
Pr[a = 1|\text{Bonus}] = Pr[C^T \leq 0] + Pr[0 < C^T \leq \epsilon^H, C^H \leq \epsilon^H + \beta - C^T]
$$

$$
= \frac{-C^T}{C^T - C^T} + \frac{\epsilon^H(-C^H + \beta + (\epsilon^H)^2/2)}{(C^T - C^T)(C^H - C^H)^

and the probability of monitoring (equivalently the probability of a bonus payment) is

$$
Pr[m = 1|\text{Bonus}] = Pr[C^H \leq \beta] + Pr[\beta < C^H < \epsilon^H + \beta - C^T]
$$

$$
= \frac{\beta - C^H}{C^H - C^H} + \frac{(\epsilon^H)^2/2}{(C^T - C^T)(C^H - C^H)^

The bureaucracy now reaches just two information sets. The first information set is $m = 0$. Anticipating teacher and head-teacher strategies, the bureaucracy deduces that the probability of teacher attendance at this information set is

$$
Pr[a = 1|m = 0] = \frac{Pr[m = 0, a = 1]}{Pr[m = 0]} = \frac{Pr[C^T \leq 0, C^H > \beta]}{Pr[C^T \leq 0, C^H > \beta] + Pr[C^T \geq 0, C^H > \beta] - Pr[\beta < C^H < \epsilon^H + \beta - C^T]}
$$

$$
= \frac{-C^T(C^H - \beta)}{(C^T - \epsilon^H + \beta - C^T)(C^H - \beta)} > \frac{-C^T}{C^T - C^T} > 0.5,
$$

and so it chooses $p = 1$. The second information set is $m = 1, r = 1$. Anticipating teacher and monitor strategies (in particular that the head-teacher may now report *untruthfully*), the bureaucracy deduces that the probability of teacher attendance at this information set is

$$
Pr[a = 1|m = 1, r = 1] = \frac{Pr[m = 1, r = 1, a = 1]}{Pr[m = 1, r = 1]} = \frac{Pr[C^T \leq \epsilon^H, C^H \leq \beta] + Pr[\beta < C^H < \epsilon^H + \beta - C^T]}{Pr[C^T \leq \epsilon^H, C^H \leq \beta] + Pr[\beta < C^H < \epsilon^H + \beta - C^T] + Pr[C^T > \epsilon^H, C^H \leq \beta]}
$$

$$
= \frac{(-C^T + \epsilon^H)(\beta - C^H) + (\epsilon^H)^2/2}{(-C^T + \epsilon^H)(\beta - C^H) + (\epsilon^H)^2/2 + (C^T - \epsilon^H)(\beta - C^H)} > \frac{-C^T}{C^T - C^T} > 0.5,
$$

and so again it chooses $p=1$. The probability of a policy mistake is therefore

$$
Pr[p \neq a|\text{Bonus}] = Pr[C^T > 0] - Pr[0 < C^T \leq \epsilon^H, C^H \leq \epsilon^H + \beta - C^T]
$$

$$
= \frac{C^T}{C^T - C^T} - \frac{\epsilon^H(-C^H + \beta + (\epsilon^H)^2/2)}{(C^T - C^T)(C^H - C^H)}.
$$
It follows that the bureaucracy’s expected payoff is

\[ E[U_{\text{Bonus}}^G] = n \varepsilon^P \cdot \Pr[a = 1|\text{Bonus}] - \kappa \cdot \Pr[p \neq a|\text{Bonus}] - \beta \cdot \Pr[m = 1|\text{Bonus}] \]

\[ = n \varepsilon^P \cdot \left( \frac{-C^T}{C^T - C^H} + \frac{\varepsilon^H(-C^H + \beta + (\varepsilon^H)^2/2)}{(C^T - C^H)(C^H - C^H)} \right) - \kappa \cdot \left( \frac{\pi^T}{C^T - C^T} - \frac{\varepsilon^H(-C^H + \beta + (\varepsilon^H)^2/2)}{(C^T - C^H)(C^H - C^H)} \right) \]

\[ - \beta \cdot \left( \frac{\beta - C^H}{C^H - C^H} + \frac{(\varepsilon^H)^2/2}{(C^T - C^T)(C^H - C^H)} \right). \]  

(12)

2.4 Testable Predictions

The analysis in Section 2.3 deliver a series of testable predictions. Prediction 1 follows from a comparison of (2), (5), and (9), Predictions 2 and 3 from a comparison of (3), (7), and (11), and Prediction 4 directly from (10) and the observation that, if the head-teacher monitors, then she marks the teacher present.

Prediction 1. The probability of teacher attendance is (i) higher in the Info arm than in the Control arm, and (ii) higher in the Info+Bonus arm than in the Info arm.

Prediction 2. The probability of a policy mistake because the head-teacher does not monitor and the teacher is absent is (i) lower in the Info arm than in the Control arm, and (ii) lower in the Info+Bonus arm than in the Info arm.

Prediction 3. The probability of a policy mistake because the head-teacher falsely reports the teacher to be present is (i) equal in the Info and Control arms, and (ii) higher in the Info+Bonus arm than in the Info arm.

Prediction 4. The probability of reported teacher attendance is (i) higher in the Info arm than in the Control arm, and (ii) higher in the Info+Bonus arm than in the Info arm. Hence, (iii) the probability of a bonus payment is positive in the Info+Bonus arm.

The teacher present whenever \( C^T < 0 \); this is the baseline presence rate. Prediction 1 (i) can be seen in the first two panels of Figure 1. There are parameter values (\( 0 < C^T \leq \delta \) and \( 0 < C^H \leq \varepsilon^H \)) where the possibility of reporting changes the teacher’s behaviour from absence in the Control arm to presence in the Info arm. We refer to this as the attendance effect of local monitoring. The size of this effect is driven by the reputational cost \( \delta \) and the head-teacher’s valuation of teacher attendance \( \varepsilon^H \). Prediction 1 (ii) can be seen in the final two panels of Figure 1. There are parameter values (\( \delta < C^T \leq \varepsilon^H \) and \( \varepsilon^H < C^H \leq \beta + \varepsilon^H - C^T \)) where the additional possibility of a cash bonus changes the teacher’s behaviour from absence in the Info arm to presence in the Info+Bonus arm. Hence, the attendance effect of local monitoring and incentives is greater than the attendance effect of local monitoring alone. The difference is increasing in \( \varepsilon^H \) and \( \beta \), since it is these parameters that determine the size of the pie that is available to cover the bargaining parties’ participation costs \( C^T \) and \( C^H \).

Prediction 2 (i) is driven by two forces. The first is simply the attendance effect of local monitoring. The possibility of reporting changes behaviour from no monitoring of teacher absence in the Control arm to truthful reporting of teacher presence in the Info arm. This eliminates the policy mistake that occurs in

\footnote{Obviously, the parameter restriction in (1), specifically \( \beta > \varepsilon^H > \delta \), and the fact that \( \beta \) is a source of transferable utility are important here.}
Figure 1: Teacher and monitor preferences and outcomes under alternative treatments

(a) Probability of teacher attendance (Prediction 1)

(b) Probability of a policy mistake (Prediction 2 and 3)

(c) Probability of reported teacher attendance (Prediction 4)
the Control arm, not by changing the policy which remains $p = 1$ but by changing $a$ from 0 to 1. Second, there are parameter values ($C_T > \delta$ and $C_H < 0$) where the possibility of reporting has no impact on the teacher but changes the head-teacher’s behaviour from no monitoring in the Control arm to truthful reporting in the Info arm. This eliminates the policy mistake that occurs in the Control arm, not by changing the teacher’s action which remains $a = 0$ but by changing $p$ from 1 to 0. This second force stems from direct utility gains from monitoring, and so its magnitude depends on $C_H < 0$. Prediction 2 (ii) is driven by the difference in attendance effects. Region A in Figure 1(b) depicts parameter values where the additional possibility of a cash bonus changes behaviour from no monitoring of teacher absence in the Info arm to truthful reporting of teacher presence in the Info+Bonus arm. This eliminates the policy mistake that occurs in the Info+Bonus arm, again not by changing the policy which remains $p = 1$ but by changing $a$ from 0 to 1.

Prediction 3 (i) has a straightforward explanation: with no transferable utility on the table, the head-teacher either cannot, or has no incentive to, monitor and send a false report. Hence, the probability of a policy mistake due to false reporting is equal (to zero) in both the Control and the Info arms. Prediction 3 (ii) is also simple. Region B in Figure 1(b) depicts parameter values where the additional possibility of a cash bonus has no impact on the teacher but changes the head-teacher’s behaviour from truthful reporting in the Info arm to false reporting in the Info+Bonus arm. This introduces a policy mistake in the Info+Bonus arm, not by changing the teacher’s action which remains $a = 0$ but by changing $p$ from 0 to 1.\footnote{Note that that this occurs even though the bureaucracy anticipates false reporting; i.e. it is \textit{ex ante} optimal for the (Bayesian) bureaucracy to set $p = 1$.} Such collusive behaviour occurs because, with $C_T > \epsilon_H$, it is no longer worthwhile for the head-teacher to compensate the teacher for his participation cost; she prefers instead to lie and pocket the entire bonus payment.

Together Predictions 2 and 3 give us the overall probability of a policy mistake. As one might expect, this is predicted to be lower in the Info arm than in the Control arm. However, contrary to common intuition, the overall probability of a policy mistake is not predicted to be lower in the Info arm than in the Info+Bonus arm. The difference is ambiguous and depends on the relative magnitude of Regions A and B. Indeed it is perfectly possible (as shown in Figure 1(b)) for Region A to exceed Region B, and hence for the introduction of the cash bonus to reduce the likelihood of a policy mistake.

Prediction 4 (i) follows from the fact that the head-teacher reports truthfully in the Info arm if $C_H < 0$ and/or there is an attendance effect of local monitoring. Prediction 4 (ii) is driven by the larger attendance effect in the Info+Bonus arm, and the head-teacher’s incentive to report the teacher present and trigger the bonus payment. The shaded region in the final panel shows the parameter values (low $C_H$) where the bureaucracy makes a bonus payment to the teacher, Prediction 4 (iii).

### 2.5 Welfare Comparisons

Our goal is to establish whether the bureaucracy’s expected payoff is higher in the Control arm with no scheme, in the Info arm with a monitoring scheme, or in the Info+Bonus arm with a monitoring and
incentive scheme. Subtracting (4) from (8), we have

$$E[U_{\text{Info}}^G] - E[U_{\text{Control}}^G] = n \varepsilon^P \cdot \left( \Pr[a = 1|\text{Info}] - \Pr[a = 1|\text{Control}] \right) - \kappa \cdot \left( \Pr[p \neq a|\text{Info}] - \Pr[p \neq a|\text{Control}] \right) > 0.$$  

Prediction 1 (i)

The first term on the RHS is the parental gain from increased teacher attendance, Prediction 1 (i). The second term on the RHS is the bureaucracy’s gain from fewer policy mistakes, Predictions 2 (i). Clearly, the bureaucracy prefers the Info arm to the Control. Subtracting (8) from (12), we have

$$E[U_{\text{Bonus}}^G] - E[U_{\text{Info}}^G] = n \varepsilon^P \cdot \left( \Pr[a = 1|\text{Bonus}] - \Pr[a = 1|\text{Info}] \right) - \kappa \cdot \left( \Pr[p \neq a|\text{Bonus}] - \Pr[p \neq a|\text{Info}] \right) - \beta \cdot \left( \Pr[m = r = 1|\text{Bonus}] \right).$$

Prediction 2 (ii) vs. Prediction 3 (ii)

The first term on the RHS represents the parental gain from increased teacher attendance, Prediction 1 (ii). The second term is of ambiguous sign. Theoretically, the bureaucracy could gain from fewer, or lose from more, policy mistakes, Predictions 2 (ii) and 3 (ii). The final term is the loss from paying out the cash bonus, Prediction 4 (iii).

Our theoretical model gives structure to this welfare analysis but, ultimately, gives only a partial welfare ranking. For this reason, we now turn to the data collected via the field experiment to estimate the above probabilities. These estimated probabilities will serve as a reality check of the model by enabling us to test Predictions 1-4. Together with the experimental value of $\beta$, they will also enable us to complete this welfare ranking for specified values of $n \varepsilon^P$ and $\kappa$.

3 Experimental Design and Data Description

3.1 Intervention Description and Experimental Design

In order to test the theoretical predictions of the model, we designed two local monitoring schemes in rural primary schools in Uganda where headteachers were required to submit daily reports of teacher attendance. These interventions are identical, except that in one intervention the reports also triggered bonus payments.

Working with World Vision, we trained headteachers, assisted by the deputy, in the use of a platform which allows them to report teacher attendance on a mobile device. This information, combined with a uniqueid of the school, teacher and monitor, is sent to a central database in Makerere University. The platform can be added to any Java-enabled phone, but we provide one phone per school to be sure. In all the intervention schools we re-broadcast a summary of results of teacher attendance to school stakeholders on a monthly basis via SMS. However, in a random sub-set of these schools teachers would also receive a monthly bonus of UShs 60,000 (USD23, or per cent of their monthly salary) if they were reported as present every week that month. We refer to these schools as belonging to the $\text{Info+Bonus}$ arm and the schools where the reports were not combined with bonus payments as the $\text{Info}$ arm.
The study took place in six different districts in 85 rural government schools in Uganda. Stratifying by district, we randomly assigned 40 schools to the control, 25 schools to the Info arm and 20 schools to the Info+Bonus arm. The intervention was implemented in September 2012 at the beginning of the third school term and lasted for a year until the end of the second school term in 2013.

3.2 Data Description

Our analysis draws from three sources of data: monitor reports of teacher attendance, our own independent spot-checks of teacher attendance, and a school survey conducted prior to and after the intervention.

First, we conducted random spot-checks of teacher attendance, both before the intervention started and during every term that the intervention took place: one in July 2012, one in November 2012 (Term 3), one over April/May 2013 (Term 1), and one over August 2013 (Term 2). This data is therefore at the teacher-day level: each observation is a different spot-check for a different teacher. Furthermore, we matched this data-set of teacher attendance with the monitor reports that took place for the same teacher on the same day. This allows us test the theoretical predictions of the model, since we have days where both a monitor reported and our independently administered spot-check occurred with the same teacher.

We visited some schools more than once during these rounds of spot-checks and randomly varied which schools received additional visits. This allows us to test for Hawthorne effects. In addition, we conducted a school survey both before the intervention started (July 2012) and after it was completed (November 2013) asked the headteachers questions about basic school and teacher characteristics.

Table 1 provides descriptive statistics of key school and teacher characteristics. In our sample of 85 schools, there are on average 11.5 teachers per school, 978 in total. Average pupil enrollment is 535 and ranges between 298 and 1,611. The teachers are predominantly male (62%), government-employed (93%), with ages that range between 19 and 62. The average teacher attendance during the baseline was 72%.

Table 2 shows the baseline balance of select variables. Columns (1) to (6) reports the results from regressing the treatment dummies on key baseline characteristics, controlling for district fixed effects and clustering the standard errors at the school level. The penultimate row reports the p-statistic for the test if the coefficients on the two treatment dummies are equal. (A p value below 0.1 would thus suggest imbalance between treatment arms.) Table 2 clearly shows that we are balanced across all treatment arms for most indicators. Most importantly, there is no statistically significant difference between treatment arms in teacher attendance rate and school enrollment figures, two key outcome variables for this paper. We only observe imbalance in teacher contract between Control and the Info arm; and imbalance in number of teachers between the Info+Bonus and Control. However, the magnitude of these differences are very small.

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9An additional 95 schools were also allocated to pilots of other monitoring schemes, which are not the focus of this paper
Table 1: Descriptive Statistics

<table>
<thead>
<tr>
<th></th>
<th>Observations</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
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<td><strong>Teacher Characteristics</strong></td>
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<td></td>
<td></td>
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<td></td>
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<tr>
<td>Teacher Attendance</td>
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<td>0.72</td>
<td>0.45</td>
<td>0</td>
<td>1</td>
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<td>Proportion of Female Teachers</td>
<td>978</td>
<td>0.38</td>
<td>0.49</td>
<td>0</td>
<td>1</td>
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<tr>
<td>Average Teacher Age</td>
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<td>9.14</td>
<td>19</td>
<td>62</td>
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<tr>
<td>Proportion of Government Teachers</td>
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<td>0.93</td>
<td>0.25</td>
<td>0</td>
<td>1</td>
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<tr>
<td><strong>School Characteristics</strong></td>
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<td></td>
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<td></td>
</tr>
<tr>
<td>Pupils Enrolled per school</td>
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<td>298.80</td>
<td>74</td>
<td>1611</td>
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<tr>
<td>Teachers per school</td>
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<td>11.51</td>
<td>4.28</td>
<td>5</td>
<td>23</td>
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<tr>
<td>Teachers Attendance per School</td>
<td>85</td>
<td>0.72</td>
<td>0.24</td>
<td>0</td>
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</table>

*Note:* Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Table 2: Balance Statistics

<table>
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<tr>
<th>Teacher Characteristics</th>
<th>School Characteristics</th>
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<tr>
<td>Info</td>
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<tr>
<td></td>
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<tr>
<td>Info+Bonus</td>
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<td></td>
<td>(0.06)</td>
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<tr>
<td>Strata indicators</td>
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<tr>
<td>Obs</td>
<td>978</td>
</tr>
<tr>
<td>Info=Info+Bonus: p-value</td>
<td>0.815</td>
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<td>Control mean</td>
<td>0.76</td>
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Note: Columns (1) to (6) report the results for regressing the treatment dummies on key baseline characteristics, controlling for district fixed effects. The data is based on a school survey conducted prior to the intervention. In columns (1) to (4) the data is at the teacher level. In columns (5) and (6) the data is collapsed to the school level. The penultimate row shows the p-statistic for the test if the coefficients on the (info) and (info+bonus) treatment arms are equal. In column (7), the data is based on independent spot-checks of teacher attendance. Each observation is a different report. Column (7) regresses the number of spot-check visits on teacher attendance, controlling for district and period effects. The third-last row indicators the number of observations in each regression. Standard errors are in parentheses and clustered at the school level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.
4 Results

In this section we test for the theoretical predictions of the model, before performing welfare analysis based on these estimated values. We find that predictions 1, 2 and 4 are all supported by the data: Teacher attendance is higher, non-reports of absent teachers less frequent, and reported presence higher in the Info+Bonus schools compared to both Info and Control schools. Furthermore, we empirically estimate total policy mistakes and find, somewhat surprisingly, that the bureaucrat would commit fewer mistakes under the Info+Bonus arm, compared to Info. Increasing the financial stakes of local monitoring was thus effective at increasing teacher attendance, yet at no cost of lower quality information. We conclude that for reasonable parameter assumptions welfare is higher when local monitoring is combined with bonus payments. We also show that pupil enrollment improved substantially in the Info+Bonus arm, compared to both Info and Control.

4.1 Testable Predictions

Figures 2, 3 and 4 present the empirical tests for the theoretical predictions, as they were outlined in figure 1a, b, and c of Section 2. Although it is easier to present our results graphically, table 3 shows the regression results that support these graphs. Columns (1), (3), (5) and (6) report the results of a simple cross-sectional comparison across all treatment arms and is estimated using the following equation:

$$Y_{i,s,t} = \sum_{t=1}^{3} \delta_t + \gamma_1 (\text{Info})_s + \gamma_2 (\text{Info+Bonus})_s + \rho_d + \epsilon_{i,s,t}$$

(15)

where $Y_{i,s,t}$ is the outcome for teacher $i$ in school $s$ in post-treatment time period $t$; $\delta_t$ are time dummies for each of the three rounds of post-treatment data collection; $\rho_d$ is district fixed effects; $(\text{Info})_s$ and $(\text{Info+Bonus})_s$ refer to the two treatment dummies; $\epsilon_{i,s,t}$ is the error term clustered at the school level.\footnote{In the appendix we also allow for time-variant treatment effects. Our preferred specification pools the treatment impact, as there is no evidence of a change of the impact of the program over time.}

Furthermore, when our independent variable is teacher attendance we can also control for baseline data, since we conducted independent spot-checks prior to the intervention. Column (2) is thus estimated using:

$$Y_{i,s,t} = \theta Y_{i,s,PRE} + \sum_{t=1}^{3} \delta_t + \gamma_1 (\text{Info})_s + \gamma_2 (\text{Info+Bonus})_s + \rho_d + \epsilon_{i,s,t}$$

(16)

where $s$, $Y_{i,s,PRE}$ is baseline attendance for teacher $i$ in school $s$.

In addition, when we test for prediction 2, the only meaningful comparison is between the two treatment arms. Column (4) reports regression results from a restricted sample which excludes the control schools:

$$Y_{i,s,t} = \sum_{t=1}^{3} \delta_t + \gamma_2 (\text{Info+Bonus})_s + \rho_d + \epsilon_{i,s,t}$$

(17)
We see from figure 2 that teacher attendance increased due to the introduction of bonuses. On days that we conducted independent spot-checks, teachers are 8 and 9 percentage points more likely to be present in the Info+Bonus schools, compared to the Info and Control schools respectively. Attendance is also slightly higher in the Info schools relative to Control, but this difference is not statistically significant (column (1) in table 3). Furthermore, comparing columns (1) and (2) we see that results remain the same when controlling for baseline attendance rates. Prediction 1(i) is thus supported by the data.

Next, figure 3 compares the likelihood of committing policy mistakes. Each observation is an independent spot-check of teacher attendance, matched with the monitor report for the same teacher on the same day. The light-blue bars (also column (3) in table 3) indicates the proportion of total teacher-days that the teacher was absent and no report was submitted. This number is 18 percentage points higher in the Control than in the Info arm; and 6 percentage points higher in the Info than the Info+Bonus arm. Both these differences are statistically significant at the 1% and 5% levels respectively. Prediction 2(i) and 2(ii) are thus supported by the data. For completeness, the grey bars indicates teacher-days where the the monitor falsely reported a present teacher as absent. The number is close to zero on both treatment arms and can be interpreted as reporting error. The maroon bars (also column (4) in table 3) show the proportion of teacher-days where an absent teacher is falsely reported as present. Contrary to the theoretical predictions, we also observe false reporting in the Info arm. Furthermore, the headteacher is no more likely to falsely report an absent teacher as present in the Info+Bonus arm. Prediction 3 is thus not supported by the data.

Although the model was ambiguous as to which of the above two policy mistakes dominates, we can now estimate this empirically. The size of the bars in figure 3 indicate the total sum of all policy mistakes.
Figure 3: Policy Mistakes

Note: Figure is based on 4493 teacher-days with independent spot-checks.

also shown in column (5) in table 3.⑪ We find that the policymaker is 7 percentage points more likely to commit a policy mistake in the Info than the Info+Bonus schools.

Finally, figure 4 indicates the frequency of a monitoring reporting a teacher as present.⑫ In the Info+Bonus schools, teachers were reported as present in 59% of teacher-days. Reported presence was 14.5 percentage points lower in the Info arm. Prediction (4) is thus also supported by the data.

⑪Including the negligible proportion of teacher-days where a present teacher was falsely reported as absent.

⑫Although we have a larger sample of monitor reports, we restrict our sample to the days that we conducted independent spot-checks to assure comparability and consistency across the empirical tests. Results hold with the larger sample.
Figure 4: Reported Teacher Attendance

Note: Figure is based on 4493 teacher-days with independent spot-checks.
<table>
<thead>
<tr>
<th>Teacher Presence</th>
<th>Abs, No Rep</th>
<th>Abs, Rep Present</th>
<th>Mistake</th>
<th>Rep Present</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Info</td>
<td>0.0105</td>
<td>0.0145</td>
<td>-0.180***</td>
<td>-0.0724**</td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Info+Bonus</td>
<td>0.0919**</td>
<td>0.0934***</td>
<td>-0.264***</td>
<td>0.00382</td>
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<tr>
<td></td>
<td>(0.04)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.02)</td>
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<tr>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Baseline Control</td>
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<td>No</td>
<td>No</td>
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<td>Obs</td>
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<td>2432</td>
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<td>Control mean</td>
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<td>0.647</td>
<td>0.353</td>
<td>0.353</td>
</tr>
<tr>
<td>Info mean</td>
<td>0.660</td>
<td>0.660</td>
<td>0.172</td>
<td>0.087</td>
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<tr>
<td>Info+Bonus mean</td>
<td>0.744</td>
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<td>0.089</td>
<td>0.084</td>
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<td>Prediction 1</td>
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</tr>
<tr>
<td>Prediction 2</td>
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<td>Prediction 3</td>
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<td>Prediction 4</td>
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<tr>
<td>(i): Info - Control</td>
<td>0.0105</td>
<td>0.0145</td>
<td>-0.180***</td>
<td>-0.0724**</td>
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<tr>
<td></td>
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<td>(0.04)</td>
<td>(0.04)</td>
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</tr>
<tr>
<td>(ii): Info+Bonus - Info</td>
<td>0.0814*</td>
<td>0.0788*</td>
<td>-0.0843**</td>
<td>0.00382</td>
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<tr>
<td></td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.02)</td>
</tr>
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</table>

**Note:** The data is based on independent spot-checks of teacher attendance, matched with monitor reports that were submitted for the same teacher on the same day. Column (1), and columns (3) to (6) report regression results on post-treatment data only: Columns (1) (4) and (5) are estimated using equation 15; columns (3) and (6) are estimated using equation 18. Column (2) controls for baseline attendance, based on equation 16. In columns (1) and (2) the independent variable is teacher attendance; in column (3) it is teacher-days where a teacher was absent but no report was submitted; in column (4) it is teacher-days that the monitor falsely reported an absent teacher as present; in column (5) it is the total number policy mistakes, including both false reports and teacher-days where a teacher was present but no report submitted; in column (6) it is reported presence. The final four rows report results as they relate to the theoretical predictions of the model. Row (i) reports coefficient on (Info) treatment dummy; row (ii) reports the difference in coefficient sizes between the two treatment dummies; Standard errors are in parentheses and clustered at the school level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.
To summarize, predictions 1, 2 and 4 are all supported by observation. Contrary to our theory, we find that headteachers in the Info schools also falsely report absent teachers as present and are no more likely to do so when reports trigger bonus payments. Furthermore, whereas the theoretical prediction was ambiguous as to which policy mistakes from predictions 2 and 3 dominate—no report vs false report on days when the teacher is absent—we can test this empirically. We found, somewhat surprisingly, the policymaker is more likely to commit a policy mistake when no financial incentives are attached to the reports.

4.2 Welfare Analysis

In Section 4.1 we showed that the testable predictions from the theory (Predictions 1-4) hold in our experimental data. Indeed, the only feature of the model that did not fit data was the fact that the head-teacher occasionally Given this broad confirmation of the model, we now complete our analysis by using the experimental results to evaluate welfare using the criterion proposed in Section 2, i.e. the government bureaucracy’s expected payoff.

Plugging the estimates in the first row of Table 3 Panel B columns (2) and (5) into (13), we cannot reject that $E[U_{Info}^G] - E[U_{Control}^G] = 0$. In short, we have failed to find any evidence that expected welfare is higher in the Info arm than in the Control arm.

In contrast, using the estimates in the second row of Table 3 Panel B columns (2) and (5) and the estimate in the third row of Table 3 Panel B column (6) into (14) we have

$$E[U_{Bonus}^G] - E[U_{Info}^G] = n \epsilon^P \cdot 0.08 + \kappa \cdot 0.07 - \beta \cdot 0.59.$$ 

In our experiment, $\beta = \text{UShs} 2000$ (the daily value of the incentive payment, approximately USD 0.77). It is quite plausible that the total parental benefit from teacher attendance plus the gain from avoiding a policy mistake is substantially larger than this. Providing that $n \epsilon + \kappa \geq 16,000$, then our empirical results suggest that it is preferable to pay for locally monitored teacher attendance. The gain from higher teacher attendance and better quality information more than offsets the cost of incentive pay.

4.3 Enrollment

To what extent did the improved teacher attendance in the Info+Bonus arm lead to improved education outcomes? In this section we test for the impact of the program on pupil enrollment. Parents might be more inclined to send their children to schools if they believe returns to education are higher. Also pupils might be less likely to drop out of school if learning improves with teacher presence. School drop-outs is a serious concern in Uganda, where only 30% of pupil who enroll in grade one make it to grade 7 (Ministry of Sports and Education, 2014:121). In our sample of schools, for example, the number of pupils in grade 7 is one average only 40% of the number of grade 1 pupils.

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13: This suggests that there is some degree of transferable utility in teachers’ reputational cost, $\delta$, of being reported as present.

14: We say broad confirmation because the model (as it stands) did not predict that the head-teacher would falsely report the teacher to be present in the Info arm or falsely report the teacher to be absent under either treatment arm. We account for this discrepancy in what follows because we focus on the overall probability of a policy mistake.
Figure 5 shows the difference in pupil enrollment, based on data we collected at the baseline and endline school surveys. Whereas pupil enrollment remains the same in the Control schools (and slightly decreases in the Info schools), it increases by 50 pupils—or a sizable 9%—in the Info+Bonus arm.

In order to test the statistical significance of this result, table 4 reports the impact of the intervention on pupil enrollment figures, estimated by the following equation:

\[ Y_{i,s,t} = \theta Y_{i,s,PRE} + \gamma_1 (Info)_s + \gamma_2 (Info+Bonus)_s + \rho d + \varepsilon_{i,s,t} \]  

Column one presents enrollment figures that were collected both prior and after the intervention.\(^{15}\) We see that the point estimates are roughly the same as the naive comparison from figure 5: After controlling for baseline enrollment, there are on average 46.5 more pupils enrolled in the Info+Bonus schools, relative to the Control; and 70 more relative to the Info schools.

Furthermore, to address a possible concern that this effect is mostly driven by a few larger schools, we also take the log transformation of enrollment figures, shown in column (2). The coefficient now represents percentage change in enrollment. Enrollment is 8% higher in the Info+Bonus schools relative to the control, although this effect is marginally insignificant at conventional levels (p=0.11). However, the difference between Info+Bonus and Info remains statistically significant.

As a final robustness test, in column (3) we look at retention of a tracked cohort of pupils. We have identifying information of 20 pupils who we surveyed in grade 3 in 2010, in a subset 57 schools in our sample. We resurveyed these children during our post-treatment school survey in November 2013. Only 15% of these children are still enrolled in the same school. Furthermore, pupils are 13.8 percentage points more likely to be enrolled in the Info+Bonus schools. This difference is statistically significant.

Note that the increase in enrollment in Info+Bonus schools does not necessarily mean that more pupils joined these schools; it could also be because fewer pupils dropped out in later grades. Although we cannot distinguish between these two reasons for changed enrollment rates, there is some evidence that drop-outs decreased. There are no more grade 1 pupils in the Info+Bonus schools relative to Info and Control schools, but there are far more grade 7 pupils. As a result the ratio of grade 7 to grade 1 pupils is much larger in the Info+Bonus schools: 0.6 compared to 0.4 in Info and Control schools.

### 4.4 Robustness

In addition, column (7) in table 2 tests for a Hawthorne effect. The reader might be concerned that our own independent spot-checks lead to higher teacher attendance. This could bias our results if the impact of our presence varies between treatment arms. For example, teachers in the (info+bonus) arms might be more responsive to our visits, if they believe it could have an implication for their bonus payments. To test for a Hawthorne effect, we randomly varied the frequency of visits during each round of spot-checks: some schools received three visits, and some schools only received one visit. In column (7) we regress the number of spot-check visits on teacher attendance, controlling district and period fixed effects. We find no evidence of a Hawthorne effect, since the number visits does not significantly impact teacher attendance.

\(^{15}\)We substituted in 2012 enrollment figures from EMIS for two school where our own enrollment figures were not collected due to enumerator error. We feel confident doing this, because our enrollment figures are almost exactly the same as the EMIS data. In fact, the 2013 figures were exactly the same for the two schools with absent 2012 data. Results hold when we drop those two schools from the sample.
Figure 5: Change in Pupil Enrollment

Note: Bars denote the change in total pupils enrolled before and after the intervention, based on 85 schools.

Table 4: Enrollment

<table>
<thead>
<tr>
<th></th>
<th>(1) Enrollment</th>
<th>(2) Log Enrollment</th>
<th>(3) Tracked Pupils Enrolled</th>
</tr>
</thead>
<tbody>
<tr>
<td>Info</td>
<td>-23.52</td>
<td>-0.0508</td>
<td>0.0469</td>
</tr>
<tr>
<td></td>
<td>(26.15)</td>
<td>(0.06)</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Info+Bonus</td>
<td>46.50*</td>
<td>0.0817</td>
<td>0.138**</td>
</tr>
<tr>
<td></td>
<td>(24.12)</td>
<td>(0.05)</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Strata indicators</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Obs</td>
<td>85</td>
<td>85</td>
<td>57</td>
</tr>
<tr>
<td>Info=Info+Bonus: p-value</td>
<td>0.016</td>
<td>0.031</td>
<td>0.158</td>
</tr>
<tr>
<td>Control=Info+Bonus: p-value</td>
<td>0.058</td>
<td>0.111</td>
<td>0.015</td>
</tr>
<tr>
<td>Control mean</td>
<td>556.250</td>
<td>6.156</td>
<td>0.344</td>
</tr>
</tbody>
</table>

Note: Each column reports a separate regression, based on equation 16. The independent variables in columns (1) and (2) are enrollment figures collected during the pre-treatment and post-treatment school surveys, July 2012 and November 2013 respectively. In column (2) enrollment figures are log transformed. The independent variable in column (3) is the proportion of pupils that are still enrolled in the school, based on a tracked cohort of pupils that were surveyed in 2010. The third-last and penultimate rows report the p-statistics from testing the equality of the (info+bonus) arm with control; and testing the equality of the (info) and (info+bonus) treatment arms. Standard errors are in parentheses. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.
5 Conclusions

This paper has provided theory and evidence on the impacts of alternative designs for local monitoring of teacher attendance in Uganda. Theory provides not only a positive set of testable predictions, but also a normative criterion for deciding amongst alternatives, which guides our analysis. Consistent with the model, we find that teacher attendance, monitoring frequency, and the number of false reports all increased with the introduction of financial incentives. More surprisingly, but again consistent with the theory, we find that the number of policy mistakes actually decreased: there were more false reports but this effect was counter-balanced by more reports in general and (hence) fewer mistakes due to a lack of information. Taken together, our results suggest that social welfare was higher with financial incentives. This outcome results from a combination of low monitoring costs and high alignment of monitor preferences with government objectives. Such preferences and costs are key parameters determining the likely success of delegated monitoring schemes applied to other contexts.
Appendix A  Further theoretical results

Here we justify our claim that under Assumption 1, if the bureaucracy chooses the Info+Bonus arm, then the optimal bonus level is $\beta > \epsilon^H$. There are three cases to consider.

Case (i) $\beta \geq \epsilon^H$.

This is the case discussed in the text. Given $\beta > \epsilon^H$, the head-teacher is never constrained when making a side-contract offer to the teacher; i.e. if the head-teacher wants to incentivise the teacher to attend, then there is enough bonus to compensate the teacher for $C_T$. The bureaucracy’s expected payoff is given in (12). Note that $\frac{\partial^2 E[U_{\text{Bonus}}^G]}{\partial \beta^2} < 0$, implying that the objective is concave in $\beta$. The intuition here is that eventually (when $\beta \gg \epsilon^H$) there comes a point where it is not worthwhile increasing $\beta$. Doing so results in more collusion and socially wasteful monitoring for only a small gain in teacher attendance.

Case (ii) $\epsilon^H > \beta \geq \delta$.

It is straightforward to show that, in this case, the objective is always increasing in $\beta$. It follows that the bureaucracy would never want to choose $\beta$ in this region. This is because the head-teacher is constrained; she wants to offer an incentive side-contract but is unable to because she cannot cover the teacher’s participation cost.

Case (iii) $\delta > \beta$.

In this case, the objective is always decreasing in $\beta$. It follows that the bureaucracy would never choose $\beta > 0$ in this region. This is because $\beta$ yields only a small attendance gain relative to collusion.

All that remains is to establish whether the bureaucracy prefers $\beta \geq \epsilon^H$ to $\beta = 0$. Note that

$$E[U_{\text{Bonus}}^G]_{\beta=\epsilon^H} - E[U_{\text{Bonus}}^G]_{\beta=0} = (n \epsilon^P + \kappa) \left( \frac{\epsilon^H \delta (\epsilon^H + \epsilon^H + \epsilon^H)^2/2}{(C^T - C^T)(C^H - C^H)} \right) - \epsilon^H \left( \frac{(C^T - C^T)(\epsilon^H + \epsilon^H + \epsilon^H)^2/2}{(C^T - C^T)(C^H - C^H)} \right).$$

The first term on the RHS is the gain from paying the bonus (higher attendance and fewer policy mistakes), while the second term is the loss from paying the bonus $\beta = \epsilon^H$. Under Assumption 1,

$$n \epsilon^P \left( \frac{(\epsilon^H - \delta)(\epsilon^H + \epsilon^H + \epsilon^H)^2/2}{(C^T - C^T)(C^H - C^H)} \right) - \epsilon^H \left( \frac{(C^T - C^T)(\epsilon^H + \epsilon^H + \epsilon^H)^2/2}{(C^T - C^T)(C^H - C^H)} \right) > 0.$$  

Hence we can focus on the case where $\beta > \epsilon^H$.  

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Appendix B  Time-varying treatment effects

In this appendix, we allow for a more general specification where the treatment effects vary across terms, in order to test for learning across terms. We estimate the following equation:

$$Y_{is,t} = \sum_{t=1}^{r} \delta_t + \gamma_{1t} (\text{info}_{is,t}) + \gamma_{2t} (\text{bonus}_{is,t}) + \theta Y_{is,PRE} + \rho_d + \varepsilon_{is,t}$$  \hspace{1cm} (19)

where treatment effects $\gamma_{1t}$ and $\gamma_{2t}$ are allowed to vary across time.

Figure B.1: Teacher presence effects by term of exposure

Results are illustrated in Figure B.1, which shows teacher presence levels by treatment arm and by trimester of exposure. Impacts are remarkably stable across the duration of the experiment.
References


